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FRANCO SELLERI AND HIS
CONTRIBUTION TO THE DEBATE ON
PARTICLE PHYSICS, FOUNDATIONS
OF QUANTUM MECHANICS AND
FOUNDATIONS OF RELATIVITY
THEORY

Dottorando: Dott. Luigi ROMANO

Coordinatore: Chiar.mo Prof. Costantino ESPOSITO

Supervisore: Chiar.mo Prof. Augusto GARUCCIO

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To Giusi

I am still giving you, my love, the light inside me.

ARCHIVES, INITIAL AND ABBREVIATIONS

FSPA	Franco Selleri's Private Archive
AGAB-FS	Archivio Generale Ateneo Barese-Fascicolo di persona n. 8832-Selleri Franco
AGAB	Archivio Generale Ateneo Barese
ASIF	Archives of the Italian Physical Society
ASUB-FS	Archivio Storico Università di Bologna-Archivio n. 17282-Posizione n. 5179
AGPA	Augusto Garuccio's Private Archive
MBPA	Michele Barone's Private Archive
CU-DRMC	Cornell University-Division of Rare and Manuscript Collections
AIP	Niels Bohr Library & Archives-Center for History of Physics-American Institute of Physics
KPC	The Karl Popper Collection at University Library Klagenfurt-University of Klagenfurt / Karl Popper Library
ALG	Archivio Ludovico Geymonat

PP	Particle Physics
OPE	One Pion Exchange
QM	Quantum Mechanics
FQM	The Foundations of Quantum Mechanics
LR	Local Realism
PLR	Probabilistic Local Realism
PDC	Parametric Down Conversion
HoS	History of Science
HoP	History of Physics
PoP	Philosophy of Physics
RT	The Relativity Theory
SRT	The Special Relativity Theory
GRT	The General Relativity Theory
FRT	The Foundations of Relativity Theory
LT	Lorentz Transformations
IRS	Inertial Reference System
GT	General Transformations
ET	Equivalent Transformations
IT	Inertial Transformations
TIT	The Theory of Inertial Transformations
WR	Weak Relativity

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INTRODUCTORY NOTES

*“Nous sommes certainement convaincus que
la connaissance de l’histoire des sciences
est de la plus haute importance.
Je pense même qu’on
ne connaît pas complètement une science
tant qu’on n’en sait pas l’histoire”¹.*

Auguste Comte (1798-1857), *Cours de philosophie positive*, 1830-1842².

I.1-INTRODUCTION

The twentieth century saw the birth and the development of new areas of physics which were unthinkable until then: the physics of the elementary particles or Particle Physics (further referred to as PP), the Quantum Mechanics (further referred to as QM) as well as the new Relativity Theory (further referred to as RT), that is, the Special and the General Theory of Relativity (SRT and GRT, respectively) They have deeply integrated our knowledge and have changed, probably permanently, our perception of reality. They were developed in those which, in many respects, could be considered very intense and devastating years. Think, for instance, of the events of the 20th century: the First World War, the advent of Fascism in Italy, the Weimar Republic (whose meaning for physics is outlined by the controversial essay by Paul Forman³), and the advent of Nazism in Germany, the Second World War, the Cold War, the political upheavals from 1968 onwards which continued until at least the mid-1980s (with the peculiar Italian situation), the fall of the Berlin Wall in 1989, which had been erected overnight in 1961, the war in Korea (1950-1953), Vietnam (1955-1975), and Yugoslavia (1991-1995), and so on.

¹ “We are certainly convinced that knowledge of the history of science is of the utmost importance. I even think that we does not fully know a science as long as we do not know its history”.

² Introduction et commentaires par Florence Khodoss. Collection dirigée par Laurence Hansen Løve. Edition numérique: Pierre Hidalgo. La Gaya Scienza, © décembre 2012, pag. 147: http://www.ac-grenoble.fr/PhiloSophie/old2/file/comte_khodoss.pdf. Accessed 24 September 2020. For Marie Auguste François Xavier Comte see also <https://plato.stanford.edu/entries/comte/>. Accessed 24 September 2020.

³ Forman P. (1971), “Weimar Culture, Causality, and Quantum Theory, 1918-1927: Adaptation by German Physicists and Mathematicians to a Hostile Intellectual Environment”, *Historical Studies in the Physical Sciences*, Vol. 3 pagg. 1-115. In the following. I will mention it as (Forman 1971).

The above theories have proved to be an enormous advance for scientific knowledge also for the technological applications that now permeate our daily lives. However, they have also led to considerable epistemological, formal and axiomatic discussions on their coherence and effective validity in the description of the reality around us. Alongside the purely technical contributions, scholars started analyzing also the contributions linked to the philosophical, social and political diversities of the protagonists as well as the historical environment in which these theories were devised. Mainly for QM and the two Relativity theories, intense and far from exhaustive debates have been developed, which have currently made the Foundations of Quantum Mechanics (further referred to as FQM) and the Foundations of Relativity Theory (further referred to as FRT), as specific fields in the international cultural debate. In the framework of the debate on these Foundations is fully inserted the figure of the Italian Physicist Franco Selleri (1936-2013). He went through all three of the aforementioned areas of physics, i.e., PP, FQM, and FRT. As we shall see, Selleri has always had a unified vision, trying to relate them to each other for a better and more complete knowledge of reality. He was a remarkable character but he has not been fully analyzed yet. His figure and activity are quite known, but little studied by the historiography. Despite the relevance of Franco Selleri's work, his importance also as a teacher, a lecturer and a promoter of scientific lines of thought all over the world and during his whole life, he did not get the attention of historians of physics in his wholeness.

But, in order to disclose a perspective that could turn out to be a more complete view, with respect to the above judgement concerning the attention Selleri received, I am going to quote, during my whole dissertation, several scholars who have written and published on Selleri's scientific research. Some studies quoting, among others, Selleri's activity have been spreading recently, such as (Tassani 2013), (Freire 2014), and (Baracca, Bergia and del

Santo 2017)⁴, but are mainly focused on his specific activity in the FQM. Actually, Selleri has been certainly an active protagonist in FQM, but also in PP and FRT, as well as a producer of a considerable dissemination activity in different socio-political and training contexts (schools, associations, political circles etc.).

In this work, the aim has been to reconstruct Selleri's role from a historiography point of view, investigating his scientific activity in the three above research areas and the historical, social and political context in which he fully operated.

I.2-MOTIVATION

The historical attitudes that distinguished my approach include as a main goal the importance of studying lesser known situations, which are generally overlooked by the so-called mainstream history.

In the History of Science (further referred to as HoS) it sounds so intriguing to try to shed some light on the real relevance of some unusual directions and research programmes that have neither left a permanent trace nor been appreciated at all.

I firmly believe in trying to emphasize the creative and inspiring study of a HoS centered on the opportunity to investigate and to find out the connections between the well-known history and the contributions that may result from going off the beaten track. This approach could wholeheartedly constitute an enhancement for everyone.

In this vein, I agreed to direct my doctoral project and the present thesis, which constitutes its *summa*, to the investigation on the role of the Italian Theoretical Physicist Franco Selleri (1936-2013) in the international scientific and epistemological debate on Particle Physics (PP), Foundations of Quantum Mechanics (FQM), and Foundations of Relativity Theory (FRT).

⁴ Tassani I. (2013) eds., *Oltre la fisica normale. Interpretazioni alternative e teorie non standard nella fisica moderna*, Isonomia (a special issue published for Selleri's 75th birthday); Freire Jr. O. (2014), *The Quantum Dissidents. Rebuilding the Foundations of quantum Mechanics (1950-1990)*, Springer; Baracca A., Bergia S., Del Santo F. (2017), "The Origins of the Research on the Foundations of Quantum Mechanics (and other critical activities) in Italy during the 1970's", *Studies In History and Philosophy of Science Part B: Studies In History and Philosophy of Modern Physics*, vol. 57, pagg. 66-79, respectively.

The scientific career and the research programmes by Franco Selleri developed between two universities⁵: the University of Bologna and the University of Bari (mainly), over a period of 50 years, beginning at the end of the 1950s, and could definitely be defined as absolutely relevant. Moreover, it is worth pointing out again how his role, in the more general frame of the history of physics, is far from having been fully analyzed.

For instance, despite a conspicuous primary literature, published on the main science journals all over the world, it is possible to observe a small production of secondary literature. Practically, besides the works just mentioned on pages 3-4, there is only a book fully dedicated to Selleri's activity in FQM and FRT written by an Italian philosopher, Pompilio Nutricati⁶.

Concerning some citations on Selleri's activity, there are some impressive exceptions, such as the specific quotations in Max Jammer⁷ and Olivar Freire Jr.'s books (Freire 2014), as well as a fully structured interview held by Freire Jr. himself in June 2003⁸, and several reviews to Selleri's books made by outstanding scholars. In addition, as a further confirmation of the impressive role played by Selleri, the same Freire Jr. wrote in 2003, jointly to Joan Lisa Bromberg (1929-2015)⁹, a letter to the Physics Department of Bari University, highlighting the importance of the "relevant historical materials"¹⁰ found in Selleri's documents during the interview, and giving some tips in order to preserve all documents for an available use.

⁵ Obviously he spent several periods in a large number of other universities and laboratories, as a visiting professor or lecturer. For instance: CERN, Saclay, Cornell, Brookhaven, Gothenbourg, Nebraska, Dubna, San Paolo, and many others.

⁶ Nutricati P. (1998), *Oltre i paradossi della fisica moderna. Fisici italiani per il rinnovamento di teoria quantistica e relatività*, Bari: Edizioni Dedalo. ISBN 978-8822062000. (Nutricati 1998) in the following.

⁷ Jammer M. (1974), *The Philosophy of Quantum Mechanics. The Interpretations of Quantum Mechanics in Historical Perspective*, New York: John Wiley & Sons. Further referred to as Jammer 1974.

⁸ Freire O. (ed.) (2003), *Interview to Professor Franco Selleri, in two sessions*, Niels Bohr Library and Archives-American Institute of Physics:

Session I: <https://www.aip.org/history-programs/niels-bohr.../oral.../28003-1>. Accessed 24 September 2020.

Session II: <https://www.aip.org/history-programs/niels-bohr.../oral.../28003-2>. Accessed 24 September 2020.

During the whole dissertation, both are further referred to as AIP 2003.

⁹ Joan Lisa Bromberg (1929-2015), was a notable a physical sciences historian. For more information, see: <https://muse.jhu.edu/article/605288/pdf>. Accessed 24 September 2020.

¹⁰ Freire Jr. O., Bromberg J.L. to Erriquez O., 15 December 2003, FSPA. It will be shown in the appropriate section of this dissertation.

Hence, as far as this study is concerned, certain of not being able to be fully exhaustive, I have felt the need to investigate as deeply as possible the role of Franco Selleri, even in light of his research activities, which can be summarized as follows:

- he ranged in the peculiar areas of PP, FQM and FRT, with at least 350 published papers, both alone and in collaboration with several scholars¹¹, all published on international journals, along with the unpublished ones; 15 published books translated in 8 languages for a total amount of 30 books; and 16 edited books. They all received positive reviews on several journals;
- he always took special care for the History of Physics and the Philosophy of Physics (further referred to as PoP), two subjects that influenced and led a lot of his scientific researches as well as being a specific objective of some of his papers;
- he took part in the left wing party as a scientist and popularizer, but also as an activist in university and political circles during the busy political life of his times, the 1960s and 1970s, in which Italy experienced a tricky and puzzling season of political unrest. Social and political events surely played an outstanding role in his approach to scientific research, too¹²;
- he contributed to make the study of FQM no longer a niche activity but an increasingly widespread one, which has become, in our day, probably THE most developed research activity in HoP and PoP. A significant contribution by Selleri can also be found in some amazing review papers, really involved in making easy the understanding of the main issues;
- he kept an impressive, close correspondence (about 700 letters and e-mails) with some outstanding scholars, scientists and philosophers from all over the world, such as, Louis De Broglie, Karl Popper, Max Jammer,

¹¹ During his whole life, Selleri published joint papers (up to 40% of his papers) with scholars of about 15 different countries.

¹² This is pointed out in (Freire 2014), and (Baracca, Bergia, Del Santo 2017). As a further confirmation, I found several papers by Selleri and others scholars, as well as book reviews and clear evidence collected during several interviews with different scientists, all concerning historical and dialectical materialism and their connection with society and science.

John Stewart Bell, David Bohm, John Archibald Wheeler, Hans Albrecht Bethe, Maurice Allais, Edoardo Amaldi, Carlo Rubbia, Emilio Segrè, Enrico Bellone, Ludovico Geymonat, Tullio Regge, Eugene Paul Wigner and so on;

- he trained several researchers in physics, as a supervisor for up to 39 degree theses and PhD dissertations. His role was a towering one, according to the recollections of many scholars who experienced the opportunity to meet him. He was also a source of inspiration and a guide for anyone who came in contact with him. Proof of this are the many personal recollections and several scholars' memories, whom I myself interviewed. Many of them clearly spoke about a change in their professional choice and in their actual life;
- he also carried out a far-reaching popularization activity, with more than 174 lectures in high schools and universities; and at least 100 social-political and popularization papers¹³ (some of which being unpublished) and books reviews, in addition to 20 newspaper articles;
- he attended an enormous number (at least 130) of international and national conferences, as an invited speaker;
- he organized¹⁴ some huge and well-structured international and national conferences¹⁵, quite often being also the editor of their proceedings that have been published by major publishing houses like Plenum, Kluwer, World Scientific;
- he was a member of the scientific committees of: *Foundations of Physics Letters*, *Annales de la Fondation Louis de Broglie*, *Hadronic Journal*, and *Apeiron*, as well as casual referee at *Foundations of Physics*, *Physical*

¹³ This number is included in the total of 350 items reported on the previous page.

¹⁴ Among the conferences he organized it is worth mentioning the following: Perugia 1982 (The wave particle dualism), Bari 1983 (Open questions in quantum physics), Urbino 1985 (Microphysical reality and quantum formalism), Cesena 1991 (Bell's theorem and the foundations of modern physics), Trani 1992 (Waves and particles in light and matter), Olympia 1993 (Frontiers of fundamental physics), Athens 1997 (Relativistic physics and some of its applications) .

¹⁵ The most involved scholars of those time participated to these conferences.

Review Letters, Physics Letters, Physical Review, Review of Modern Physics, Journal of Physics, Nuovo Cimento and so forth;

- he was elected as a member at American Physical Society, New York Academy of Sciences, Fondation Louis de Broglie, Gdanskie Towarzystwo Naukowe (Poland), as well as a member of the Executive Board at Italian Physical Society¹⁶ during the years 1969-1970;
- he won the Prize of the Italian Physical Society for Young Researchers in 1961¹⁷;
- he won the Gold Medal by the Gdanskie Towarzystwo Naukowe (The Gdansk Scientific Society) in 1987¹⁸.
- he was proposed by several international scholars for the 1991 Lakatos Award¹⁹.
- he won the Telesio-Galilei Academy of Science Gold Medal in 2010²⁰;
- he received two dedicated issues of Foundations of Physics for his seventieth birthday²¹. In the first one it is stated:

We are honored to bring homage, in this issue and the next [...] of Foundations of Physics, to Franco Selleri as a special colleague and friend, to praise the enduring passion he has displayed in seeking answers to the deepest questions of physics, and especially to celebrate the ever skeptical attitude he has brought to bear on this pursuit.

¹⁶ Società Italiana di Fisica. SIF in the following.

¹⁷ In the SIF Bulletin n. 21, dated 28 February 1961 there is mention to the assignment of six "Physics encouragement awards". The initial thirteen competitors were reduced to nine at the interview, and only six were awarded. Selleri came second out of six winners, being graduated *summa cum laude* and presenting 2 papers and a third joint one.

¹⁸ In a letter I found in FSPA it is written: 1987, wrzesnia 23. Letter from Gdanskie Towarzystwo Naukowe, Poland "The Societas Scientiarum Gedanensis has the honour to present to Professor Franco Selleri its medal in recognition of his outstanding contribution to physics". signed by the President doc. W.A. Majewski (FSPA. My underline).

¹⁹ I found in FSPA Archive several recommendation letters to plead the cause for his book *Quantum Paradoxes and Physical Reality*. Giuseppe Bufo-Brest, Bronislaw Sredniawa-Krakow, Alexej Tyapkin-Moscow Virendra Singh-Bombay.

²⁰ In the motivation it is written: "From a very early stage in his career, Franco Selleri was worried by several, as he saw it, outstanding problems associated with the foundations of quantum physics and special relativity. His contributions to the foundations of special relativity must be noted in particular because, as a result, we now have a completely new theory which is different from relativity. Hence, although he has made contributions to the foundations of quantum theory, in special relativity he is responsible for creating a completely new theory and it is for this the award is made"

http://www.telesio-galilei.com/tg/index.php?option=com_content&view=article&id=11&Itemid=12. Accessed 24 September 2020.

²¹ Foundations of Physics 34 (11-12), 2004. Weirdly published two years before Selleri's actual seventieth birthday.

[...] we wish him many more years of this rare brand of creative activity (Tarozzi, Van der Merwe 2004, pagg. 1614-1615) .

So, in a nutshell, it seems clear that it is definitely worthwhile to investigate, with a peculiar attention, on Selleri's activities, taking into account the whole international background in which he developed his scientific research as well as underscoring the importance of tackling a possible reconstruction of his life and work, which has never been explored by historians in its wholeness so far.

My main goal is to try filling what I consider to be a biographical and bibliographical gap in the research work which should not be underestimated. It appeared advisable that a systematic and deeper analysis of his scientific production was to be carried out in order to enlighten the details of his whole work, also because there is currently no ordered archive relating to Franco Selleri.

I am confident that this study can lead to a more exhaustive knowledge, albeit not at all conclusive, of an intriguing contribution to a period of physics from which descends almost all of today's current developments and researches. All in all, Selleri's activity can lead to a better comprehensive breakthrough of the special relationship between science and society that emerged in the peculiar period of his activity.

I.3-OVERVIEW

The reconstruction of the role of Franco Selleri in the 20th and 21st century physics is still covering his life and scientific research. He went through a very long period with his research activities, covering the years from 1957 (actually, he published before his graduation) to 2013. During his long and uninterrupted work, he dealt with the different areas of PP, FQM and FRT, without a clear-cut time separation and related to one another, always within the epistemological, historical and political landscape of that specific period. The **first** chapter (Selleri and Particle Physics 1958-1970) aims at dealing with the early research of Franco Selleri regarding PP. At least 40 published papers were written by Selleri during this peculiar phase of his activity.

Some of them are submitted in this work. This area of Selleri's research has never been investigated before, and I thought it could set up an intriguing contribution to the historical debate. All documents I have found in different archives could shed some light on Selleri's truly impressive and quite unknown research in this field.

Starting with a brief, non exhaustive introduction on the historical topic concerning the physics of elementary particles, the chapter continues with Selleri's degree thesis, and with some correspondence about the opportunities of finding a job as soon as his thesis was over. Then his periods at CERN, Bologna University, Saclay and USA, as well as his return to Bologna University and his transfer to the University of Bari are also investigated. The chapter goes on with the analysis of his peculiar change to FQM, whose complex and deep-rooted scientific, sociological, political and epistemological reasons are all investigated further in this study, also through the priceless content of the interviews I managed to carry out. I aimed at showing how troublesome and appealing were the reasons behind his decision. Thus, the whole chapter is aimed at identifying this appealing topic of Selleri's scientific research on PP.

The **second** chapter (Selleri and the Foundations of Quantum Mechanics 1969-2003) interfaces with Franco Selleri's thirty-five years activity in FQM. A very long period, with a huge amount of published papers, several pivotal written²² and edited books, and a flood of organized and attended lectures and conferences all over the world as invited speaker. A special emphasis has been remarkably dedicated in this chapter to Selleri's seminal choices and papers deeply interrelated with epistemological, political and historical reasons, since my aim is to underline how these were related to his scientific approach. Here again, the chapter begins with an introduction about the historical events and the different interpretations of QM that led to the current situation, for the terrific, groundbreaking role of the issues related to

²² One of them sold at least twenty thousand copies in the Eighties, whereas two of them reached the third edition.

FQM in the current research activity all over the world. Then, Selleri's early papers on FQM, written in 1969, are examined. These are really useful for an understanding of his approach and also to shed light in Selleri's will to develop some topics throughout his career.

This chapter allowed me to highlight an outlook on the issues of the time, whose influences are peculiarly relevant for the main outline.

Consequently, a paragraph deals with the political turmoil which deeply marked the 1970s in Italy, to give the reader some historical guidelines, actually not exposed too extensively. Another paragraph investigates Selleri's key role played in the Executive Board of Italian Physical Society during the years 1969-1970²³, a period in which he remarkably contributed to several far-reaching resolutions in that forum.

International meetings usually sound really noteworthy, too, because they reveal strong insight in the scientific issues and in the atmosphere of the time. From this perspective, I highlighted in another paragraph how much special attention had been devoted to the first summer school on FQM, that took place in Varenna, on Lake Como from 29 June to 11 July 1970, within the international school of Physics "E. Fermi".

Further on in the chapter, I illustrated the main topics investigated by Selleri during his scientific research, describing what they consist of, evidencing the abstracts of his papers, showing his results and trying to summarise the main themes, linking them to the general context of the debates of the time. Eventually, the multiple reasons for his second peculiar move from FQM to FRT are thoroughly revealed.

The **third** chapter (Selleri and the Foundations of Relativity Theory 1990-2013) addresses the Selleri's research concerning FRT. He wrote almost 64 papers, though many remained unpublished.

At the beginning, there is a historical reminder about the birth of the SRT as well as to the fundamental struggle on FRT.

²³ Specifically, from 11 January 1969 to 30 October 1970.

Due to specific reasons, here recalled, there has always been a great opposition against the theory. Further in the chapter I carried out a detailed analysis of Selleri's activity on FRT. Being this last part of his research too recent and concluded just before his death, this subject has not been fully investigated so far. As in the first chapter, I thought my research could be an opportunity to bring a new and enriching contribution.

In the previous three chapters described I aimed to include Franco Selleri's heritage and legacy, too. This approach could convey the extent of his current weight on today's research as well as establish his memory.

Throughout this work several letters and email are introduced, in addition to pivotal recollections and interviews, showing the extent of his relationship with so many great physicists from all over the world. Through these documents I have tried to understand the kind of *liaison* he had had with them and also the scope of the attention they devoted to him. In every chapter of this study several reactions to Selleri's work are illustrated, so as to show the real impact of his scientific research and activity on the international scientific community.

In the **Summary and Conclusions** I recalled the various and broad outcomes achieved by Selleri's scientific research and popularization activity. The present work ends with a **Bibliography**, a **Sitography**, and **three addendums**.

In the **Bibliography** I decided to include only all Selleri's written and edited books, and all his English papers.

The **first addendum** (Addendum 1) deals with the Selleri's substantial and varied whole scientific production that I managed to retrace, whether published or not. All written and edited books, all his papers, chronologically subdivided according to their field (PP, FQM and FRT) are here reported. Furthermore, I decided to enclose also several newspaper popularization reports, all his papers written in several languages, and, lastly, all degree and PhD theses supervised by him.

In the **second addendum** (Addendum 2), a list of national and international conferences, high schools seminars, and scientific/cultural center lectures is exhibited, thus showing how intense Selleri's activity was.

Finally, in the **third addendum** (Addendum 3), some remarkable, noteworthy letters taken from Franco Selleri's huge correspondence are evinced, so as to reveal how deep and far-reaching has been his presence in the international scientific debate. Moreover, the full reproduction of documents whose excerpts and/or translations have been used within the text are also introduced

I.4-THE SOURCES

Franco Selleri's private archive (further referred to as FSPA-Franco Selleri Private Archive) had neither been analyzed yet nor organized so far, and this is another reason that prompted me to do this work. I had the opportunity to access all his archive, which is stored at Selleri's family home in Bari at the present.

This constituted the main repository for my research and an unexpected treasure chest of invaluable significance.

It should be definitely stressed that Selleri revealed a strange approach. Although he often showed a strong confidence about his scientific commitment, as well as the importance of his research, nevertheless he operated a strong selection of the papers and other documents to keep to the record. Indeed, he created a lot of lists and nomenclatures to categorise his papers and activities. In this respect, while obviously respecting his choice, which has a precise historical and methodological significance, I have carried out a research of all his production in order to display a bibliography that should be as complete as possible.

As for the primary sources, I considered the following:

- **Franco Selleri's Private Archive (FSPA)**.

At a historical-methodological level, the primary sources have been identified at an earlier stage.

Besides the publications (books, papers, articles on newspapers, and so on), I had the opportunity to find a huge amount of original documents, which, quite often had never been observed before. These historical materials include letters and emails, diaries, booknotes, manuscripts of lectures, reports, papers and preprints. But also protocols and registers of scientific institutions, applications for jobs, job advertisements and assessments by applicants, documents concerning admissions to cultural and scientific societies and institutions. There are conference notes, proceedings of international meetings, preprints, recollections and CVs.

He began his scholarly activity with the production of handwritten and typewritten documents, and, later in his career, of emails and digital storage, after the advent of computers. In this sense, I have found several USB memory sticks exceptionally full and rich of further unknown documents. I have also analyzed the foregoing lists in the choice he made of arranging his stuff, i.e., papers, seminars, conferences, lectures, courses, and so forth.

Only to confirm his active involvement in the society of his age, in his library there are many books and articles concerning HoP, politics, epistemology, and so on. He also collected a number of newspapers about conferences and others about general issues (sociological ones, articles on current events and so on). Thus I had the chance to experience the times he lived also through the scrutiny of these materials.

A large part of my research was also carried out in several Italian Archives, including:

- **Archivio Generale Ateneo Barese (AGAB)**

Having the opportunity to investigate in the personal file of Franco Selleri as well as in the Institute of Physics and the Department of Physics of University of Bari, I managed to locate a huge amount of information concerning his whole career as university lecturer, along with his official reports on research programmes, ministerial memoranda, legal documents, administrative communications on salary scales, competitions, facts and career advancement, CVs and so on.

Another source for this reconstruction is represented by the Annual Progress Research Reports of the former Institute of Physics (which later became the Department of Physics of the University of Bari), which also testify the natural evolution of Selleri and his group's scientific interests through the years, as well as their collaborations. Some Selleri's personal letters, paramount to understand the condition he went through due to his choices, completed my research in this archive.

- **Archivio Società Italiana di Fisica (ASIF)**

Through the Minutes of the Steering Committee, the Bulletins of SIF, and folders concerning the Summer schools held in Varenna in 1970 (on FQM), these Archives have proved to be fundamental in tracing the active participation of Selleri, as a counselor, and his role in the decisions taken by the Society on numerous far-reaching issues linked to major scientific, social, political, epistemological and historical events of that time. Several really subtle letters are presented here, too. Undoubtedly, the SIF Archives have played an essential role in my research.

- **Archivio Storico Alma Mater Studiorum-Università di Bologna (ASUB)**

I managed to reconstruct Selleri's university career as a student in engineering, along with his first period as a lecturer at Alma Mater Studiorum-University of Bologna, even if I discovered the absence of the last two years of Selleri's university studies in Physics. Despite the search with the efficient staff of the Archive, it was not possible to trace anything relating to the two-year period of physics followed by Selleri.

Some private archives, which had never been catalogued before, have also been scrutinized:

- **Augusto Garuccio's Private Archive (AGPA)**

In this archive, I found a lot of papers written by Selleri that were difficult to find due to his above-mentioned process of selection, several documents concerning conferences, and some disagreements on papers by different reviewers.

- **Michele Barone's Private Archive (MBPA)**²⁴

I can safely say that this archive has definitely been a cornerstone for my researches. I had the opportunity to bring to light many intriguing and very rare documents, unavailable elsewhere. Primarily, strong evidences concerning the Selleri's two seminal switchovers from PP to FQM and from FQM to FRT. Surely that has undoubtedly been a turning point in my research.

In addition, I had the opportunity to promptly receive the correspondence between the Nobel Prize in Physics Hans Albrecht Bethe and Franco Selleri before and during the American years of the latter at Cornell from the **Cornell University-Division of Rare and Manuscript Collections (CU-DRMC)**.

Furthermore, I went through the impressive and far-reaching interview made by Olival Freire Jr. to Selleri which is kept at the Center for History of Physics at the **American Institute of Physics (AIP)**, and it proved to be an outstanding document in which Selleri's thought is directly revealed.

The **Karl Popper Copyright Office-University Library Klagenfurt (KPC)** gave me the permission to quote several excerpts concerning the impressive correspondence between Selleri and Karl Popper.

In the **Archivio Ludovico Geymonat (ALG)**, in Milano, I got the chance to quote several excerpts by the seminal letters written by Selleri to the Italian philosopher and epistemologist Ludovico Geymonat.

Being fully aware of the potentially decisive importance but also of the possible risks associated with memories and recollections, I decided to use oral interviews in order to collect a record from several scientists, which would allow me to understand the real effect of Franco Selleri's life and activity on the debate on PP, FQM and FRT.

²⁴ An Italian physicist that worked at CERN and at National Center For Scientific Research-Demokritos in Athens, linked by a long-term friendship with Selleri.

Many contributors, co-authors and physicists who well knew Selleri are still alive, so a good source, however incomplete, of historical information has definitely been collected through the means of oral history investigation.

With regard to the secondary sources, some contributions which outline the main features of Franco Selleri and his work are quoted below in a chronologically order. Obviously, this list cannot be considered exhaustive:

- Nutricati P. (1998), *Oltre i paradossi della fisica moderna*, Bari: Edizioni Dedalo, ISBN 8822062000.
- Interview of Franco Selleri by Olival Freire on 2003 June 24, Niels Bohr Library & Archives, American Institute of Physics, College Park, MD USA, <https://www.aip.org/history-programs/niels-bohr.../oral.../28003-1>.
- Interview of Franco Selleri by Olival Freire on 2003 June 25, Niels Bohr Library & Archives, American Institute of Physics, College Park, MD USA, <https://www.aip.org/history-programs/niels-bohr.../oral.../28003-2>. (Both accessed 24 September 2020.).
- Bergia S. (2004), “The way we were: bubble chambers pictures, pion-nucleon interactions and polology”, *Foundations of Physics* 34 (11), pagg. 1761-1776.
- Two issues dedicated to Franco Selleri: *Foundations of Physics* 34 (11-12), 2004.
- Tarozzi G. (2011), “Oltre la fisica *normale*. Realtà della funzione d’onda e delle proprietà fisiche prevedibili nell’interpretazione di Selleri della meccanica quantistica”, in Tassani I. (2013) eds., *Oltre la fisica normale. Interpretazioni alternative e teorie non standard nella fisica moderna*, Isonomia (a special issue published for Selleri’s 75th birthday).
- Freire O. (2014), *The Quantum Dissidents. Rebuilding the Foundations of quantum Mechanics (1950-1990)*, Berlin, Heidelberg: Springer. ISBN 978-3-662-44662-1.

- Baracca A., Bergia S., Del Santo F. (2017), “The origins of the research on the Foundations of Quantum Mechanics (and other critical activities) in Italy during the 1970s”, *Studies in History and Philosophy of Science Part B: Studies in History and Philosophy of Modern Physics*, Volume 57, pagg. 66-79.
- Guerra F. (2017), “Franco Selleri: the courage of ideas”, 37° SISFA National Congress, Bari 26-29/09/2017.

Furthermore, throughout the years I looked into several manuals on PP, QM and RT, as well as into review articles written by experts on these subjects. In order to widen my view on the history and philosophy of XXth century physics, I eagerly used several books which constituted an inspiration for the reconstruction of the general background in which Selleri operated. Among the others, the names of, Forman 1971, Geymonat 1971, Geymonat 1972, Jammer 1974, Segrè 1980, Davies-Brown 1986, Heilbron 1988, Bacciagaluppi-Valentini 2009, Freire 2014, Crease-Goldhaber 2015²⁵ stand out.

Finally, I would like to point out that I translated into English all interviews I carried out, all the Italian documents mentioned in this work, some statements found on internet sites and all the Selleri’s Italian correspondence quoted in this dissertation. The original ones appear in the Addendum 3 and in some footnote.

Needless to say, the responsibility for any errors or misinterpretations rests entirely upon me.

I.5-BIOGRAPHICAL NOTE

There is not much biographical information on Franco Selleri, apart from some short autobiographical notes present in some of his CVs. Some essential news on his life can be found in some newspapers and in a few interviews.

²⁵ For further details see the Bibliography.

In this respect, in order to reconstruct some events of his life I used several major and remarkable data which come from my interview with Angela Pizzi, the second wife of Selleri, held in Bari on 11 July 2017; from the superb interview with Olival Freire Jr. to Selleri (held in Bari on 24/25 June 2003)²⁶; from the recollections of scholars I have surveyed; from the Selleri's personal folder and the material related to the Institute and Department of Physics in Bari, both in the AGAB archives; from the ASUB archives in Bologna.

Franco Selleri was born in Bologna on 9 October 1936, in a family with no specific scientific attitudes at all. He spent his childhood in Via Vittorio Veneto 22-24. His father Antonio was born in 1902 and worked for the Italian rail, specifically as a ticket collector. He passed away in 1987, at 85, with no white hair. Selleri's mother, Dina Capelli, a housewife, was born in 1910, and she had a very long life, passing away in 2008 at 98. Nadia, 9 years younger, is Selleri's sister and she is currently living in Rome.

Antonio Selleri suffered hearth diseases and the climate in Bologna was a source of suffering for him. Later, he and his wife relocated in Rome to stay with their daughter.

He was:

[...] a very principled man, a person of strong principles. He had never wished to swear allegiance to the fascist party, he had always considered fascism with great contempt. At that time there was still no obligation in swearing, though (Interview with Angela Pizzi, Bari, 11 July 2017).

Selleri attended scientific high school 'Augusto Righi' in Bologna.

²⁶ Further referred to as (AIP 2003). Being multiple citations, I quoted them in the whole dissertation with (AIP 2003). But this statement is for:

- Interview of Franco Selleri by Olival Freire on 2003 June 24, Niels Bohr Library & Archives, American Institute of Physics, College Park, MD USA, www.aip.org/history-programs/niels-bohr-library/oral-histories/28003-1

- Interview of Franco Selleri by Olival Freire on 2003 June 25, Niels Bohr Library & Archives, American Institute of Physics, College Park, MD USA, www.aip.org/history-programs/niels-bohr-library/oral-histories/28003-2

I found a school report dating back to 15 January 1952 and related to the school year 1951/52, when Franco was a 15 years old student in his third year. His father was already retired. Franco studied German as foreign language. In the first of the three trimesters, he got some bad grades: 3/10 in written German (an F), 4/10 in Philosophy (an E). At the end of the year, he did not fail any subject, getting 7/10 (a B) on Mathematics and Physics, and his highest grade 8/10 (almost an A) in Science, Chemistry and Geography. Concerning his high school years, he spoke notably about Prof. Aldo Graffi (1899-1974), who taught mathematics and physics for forty years at his same scientific high school. Selleri experienced an unusual relationship with Graffi's family. Dario Graffi (1905-1990)²⁷ was his Rational Mechanics professor at University²⁸. Selleri was also the supervisor of Sandro Graffi (1943-)²⁹, Dario's son, for his degree thesis and, moreover, they wrote a joint paper in PP, too. Aldo Graffi was a basic influence and this is clearly stated in Selleri's recollections:

[...] I think that I was very lucky from the point of view of school because I had an excellent teacher of mathematics and physics. [...] the name of the teacher was Graffi. [...] He had really a robust personality and he could push people to be interested in mathematics and physics. I think that if I decided to be a physicist it was his merit. Not that he suggested to me in particular to study physics at the university, but just his type of personality was very strong and influential for me (AIP, 2003. My underline).

In 1954, due to some economical problems in his family, Selleri decided to enroll in the faculty of engineering (matriculation number 5179) for the greater job opportunities it offered. In this regard, Selleri claimed:

Then I went to the university and there was a problem with my family who had not much money. Basically they said: you can go to university provided you have good grades and we do not have to pay taxes, even though at the time the taxes were not very strong, but still they were a problem for us.

²⁷ For Dario Graffi, see Adriano Morando, Dizionario biografico degli Italiani, Volume 58 (2002) [http://www.treccani.it/enciclopedia/dario-graffi_\(Dizionario-Biografico\)/](http://www.treccani.it/enciclopedia/dario-graffi_(Dizionario-Biografico)/). Accessed 24 September 2020.

²⁸ Concerning Dario Graffi, Selleri stated: "Graffi again, the brother of my high school teacher" (AIP, 2003).

²⁹ For Sandro Graffi, see <https://www.unibo.it/sitoweb/sandro.graffi/cv>. Accessed 24 September 2020.

This I succeeded in doing, but I inscribed at the university in engineering even though it was clear to me that physics was my interest. But I thought that as a physicist probably I could not get a job; as an engineer it looked much more promising, so that was the reason (AIP, 2003).

Conversely, at ASUB I found some forms with the exams sat by Selleri during his studies in engineering. Some of these are reported in the following charts:

<i>1954/55</i>	<i>Subject</i>	<i>Date of examination</i>	<i>Grade</i>
	Analytical Geometry-Part One	27/06/1955	26/30
	Experimental Physics Tutorials-Part One	07/07/1955	29/30
	General Chemistry	17/10/1955	28/30
	German language	20/10/1955	Passed
	Mathematical Analysis -Part One	31/10/1955	27/30
	Technical Drawing-Part One	07/11/1955	25/30
<i>1955/56</i>	<i>Subject</i>	<i>Date of examination</i>	<i>Grade</i>
	Analytical Geometry -Part Two	12/02/1956	28/30
	Experimental Physics Tutorials-Part Two	13/06/1956	26/30
	Experimental Physics	16/06/1956	27/30
	Technical Drawing -Part Two	26/06/1956	27/30
	Rational Mechanics	17/10/1956	30/30
	Mathematical Analysis -Part Two	03/11/1956	28/30

It seems that Selleri also passed an English and a Spanish language exam, even if I could not find any official certification.

The economical and health problems in his family also played a key role in his unlimited leave from the military service. On 27 May 1966, the military district of Bologna stated that Selleri Selleri was “released from compulsory military service as a supernumerary element and in a serious family situation” (AGAB-FS)³⁰.

³⁰ “Dispensato dalla ferma di leva quale elemento eccedente al fabbisogno quantitativo e qualitativo ed in grave situazione familiare”.

At a later stage, a recruiting advertisement on a local newspaper by Professor Giampietro Puppi (1917-2006)³¹, Director of the Physics Institute in Bologna at that time, in which young people were hired to work in particle physics, proved to be decisive for his choices. In fact, Selleri pointed out that:

[...] [Puppi] said, “Show me your libretto with the grades, the records”. I gave it to him and he said, “You can come even tomorrow.” So I switched from engineering to physics and then I was very happy because that was my real interest (AIP, 2003).

In the ASUB I only found something concerning Selleri’s first two years at the Faculty of Engineering. There is no trace whatsoever of his last two years in Physics, with the exception of a request to the Rector for his transfer from the second year of Engineering to the third of Faculty of Physics; and of a note in which his passage to physics on 4 December 1956 is stated. It sounds quite paradoxical, since Physics was a Faculty of the University of Bologna whereas Engineering was not, being a stand-alone Faculty. All jointly research, carried out with the help of the available and interested archive staff, have proved to be useless.

Concerning Selleri’s great influences during his university years in the various subjects, he mentioned Dario Graffi for Rational Mechanics, Giovanbattista Bonino (1889-1985)³², professor of Chemistry, and Antonio Pignedoli (1918-1989) as a professor of Mathematics. The scientific career of Pignedoli could also be meaningful for the similarities it presents with the research paths of Selleri.

³¹ He studied at Padua University with Bruno Rossi and Giancarlo Wick. Linked to his name we can recall, the so-called “Puppi’s triangle”, in PP, concerning the universality of weak interactions. He was also the Director of the first two Varenna Courses at the International School of Physics. For more detailed information, see Giorgio Dragoni (2016): *Dizionario Biografico degli Italiani*-Volume 85 (2016): [http://www.treccani.it/enciclopedia/giampietro-puppi_\(Dizionario-Biografico\)](http://www.treccani.it/enciclopedia/giampietro-puppi_(Dizionario-Biografico)). Accessed 24 September 2020. On 25 October 1988 in Bologna an international interdisciplinary symposium was dedicated to him, edited by R. Fanti, A. Forino, G. Giacomelli, L. Padrielli, and A. Speranza. Selleri participated to the conference with a paper on “The unsatisfactory foundations”. And also a collective issue of SIF Bulletin “Il Nuovo Saggiatore”, 23, N. 5-6, 2007 in memory of Puppi, was edited by Silvio Bergia, Paolo Capiluppi, Sergio Focardi, and Giorgio Giacomelli.

³² For Giovanbattista Bonino see https://it.wikipedia.org/wiki/Giovanni_Battista_Bonino. Accessed 24 September 2020.

After a degree thesis about the detection of elementary particles in the photographic emulsions, awarded with the Righi gold medal, Antonio Pignedoli focused on Quantum Mechanics and then on Relativity Theory.

As Isabella Barbieri well stated in the Treccani³³ Biographical Dictionary of the Italians on Pignedoli's attitude towards research, the latter was always deep involved in history and philosophy of science:

Concerning quantum mechanics he tries to combine the indeterministic approach of the Copenhagen School with a deterministic interpretation.

[...] [Pignedoli's] deep belief in the unity of knowledge which merges the sciences among them and also science to philosophy, meant that he united his strictly scientific production with an equal production devoted to philosophy of science, its history and its connexions with the other disciplines [...].

[...] The strong conviction that knowledge must be the patrimony of all led him, from the first years of his career, to an intense activity of lecturer who expressed himself in a unique way and style, for his exceptional communicating skills and by virtue of his wide and deep culture³⁴ (Barbieri 2015).

Selleri had a strong a relationship with his mentor Pignedoli, as certified by the correspondence I found in Selleri's archive. In one of those letters, Pignedoli promoted some of Selleri's features and approaches to research:

Dear Selleri. Congratulations on your high scientific work. Don't worry about fashions or taboos: work with guts! As always. I wish you every success³⁵ (Pignedoli to Selleri, 14 April 1986-FSPA).

³³ Currently, Treccani is an Institute whose goal is "...the preparation, the updating, the publishing and the spreading of the Italian Encyclopedia of Science, Letters and Arts started by Giovanni Treccani Institute and the arising works linked to it [...] for the development of humanistic and scientific culture as well as educational, research and social service needs". For more information, see <http://www.treccani.it/istituto/chi-siamo/>. Accessed 24 September 2020.

³⁴ "Nell'ambito della meccanica quantistica cercò di conciliare la visione indeterministica della scuola di Copenhagen con un'interpretazione deterministica. [...] La profonda convinzione dell'unitarietà del sapere che unisce le scienze fra loro e le scienze alla filosofia, lo indusse ad affiancare alla produzione scientifica in senso stretto una produzione di pari intensità dedicata alla filosofia della scienza, alla sua storia, alle relazioni con altre discipline... [...] Il forte convincimento che il sapere deve essere patrimonio di tutti lo portò, fin dai primi anni della sua carriera, a un'intensa attività di conferenziere che esprimeva in modo e con stile singolare, per le sue eccezionali doti di comunicatore e in virtù della sua ampia e profonda cultura" Isabella Barbieri-Dizionario Biografico degli Italiani-Volume 83 (Barbieri 2015), in: http://www.treccani.it/enciclopedia/antonio-pignedoli_%28Dizionario-Biografico%29/. Accessed 24 September 2020.

³⁵ "Caro Selleri. Rallegramenti per il tuo alto lavoro scientifico. Non preoccuparti delle mode o dei tabù: lavora con fegato! Come sempre. Ti auguro ogni successo" (Pignedoli to Selleri, 14 April 1986-FSPA).

Giampietro Puppi can be considered a truly relevant figure in Selleri's career. In addition to the aforesaid invitation in the newspaper, Puppi also proved to be essential for Selleri's work on his thesis, being, the former, his supervisor when Selleri had to find a new tutor. This is what happened, in Selleri's own words:

Then I don't know why, because I had very good grades, but I made a mistake. When came the moment for me to do the thesis I asked a lady, a very good theoretical physicist, to be my thesis advisor. And I think that was a mistake, because she gave me a book to read which I could really not understand. It was the highest level, okay, but very difficult. I later understood that it was a philosophical rejection on my side, but it was very difficult to understand anything. Then a new stroke of luck came.

This lady left physics, went to the United States to get married. And she told me, "I cannot follow you. You will have to find somebody else for your thesis." So I did not know what to do. I went to see the director, Puppi, and I told him the situation and he said, "Don't worry. You will do the thesis with me." Now Puppi at the time was a wonderful personality. Everybody liked very much his teaching. He loved the physics and made people love it as well. His interests were then weak interactions (AIP, 2003).

Deeply fascinated by Bruno Rossi³⁶ in Padua, who was planning the new headquarters of the Institute of Physics and dealt with cosmic rays, Puppi himself switched from Engineering to Physics. Puppi remembered the extraordinary teaching skills by Rossi as well as his habit of preparing the lessons with care. Puppi would always consider teaching as a fundamental activity and Rossi's style would always be an example to him. As Puppi himself pointed out in an intriguing interview collected in a book by Luisa Bonolis and Maria Grazia Melchionni (Bonolis, Melchionni 2003)³⁷:

[...] I learned to teach from [Bruno Rossi]. [He] was a fabulous teacher, he prepared his lessons in an incredible way, he did not leave anything to chance, and being very prepared he was able to speak in an extremely clear way.

³⁶ A remarkable reference to Bruno Rossi is certainly the outstanding work by Luisa Bonolis: "Bruno Rossi and cosmic rays: from Earth laboratories to physics in space", PhD dissertation in History of Science, University of Bari, 2009.

³⁷ See Bonolis L., Melchionni M.G. (eds.) (2003), *Fisici italiani del tempo presente. Storie di vita e di pensiero*, Venezia: Marsilio. (Bonolis, Melchionni 2003) in the following.

Remembering how he taught, I learned³⁸.

[...] I have always considered teaching to be a fundamental activity because what matters is preparing the future. I have always looked after my students a lot, working with them.

There is no other way but to work together³⁹ (Bonolis, Melchionni 2003, pag. 352).

Selleri was deeply affected by the education of his teacher, managing to put, according to many, the same care and attention in teaching.

Selleri graduated in Physics at Bologna University, “Alma Mater Studiorum”, *summa cum laude*, in the academic year 1957/58, specifically on 22 December 1958 (at the age of 22) with Puppi, discussing a thesis entitled “Analisi dell’interazione pione-nucleone”.

As told before, Selleri’s research career can be subdivided into three different periods, related to peculiar areas: PP, FQM and FRT, even if they are not clearly separated in time and are surely often intertwined with each other.

His first ever published work did not cover PP but other scientific subjects, a specific quality that will be present throughout his life. Consequently, in 1957, before his degree thesis, Selleri published with Luigi Baldinelli, on an Italian astronomy magazine, *Coelum*⁴⁰, a paper titled “Le supernovae”, in Italian. Meanwhile, in 1958, he published on the *Memoirs of Italian Astronomical Society*, “Alcune considerazioni sull’effetto Einstein”, written in Italian too.

Following his graduation in Physics, Selleri entered the INFN (Istituto Nazionale di Fisica Nucleare) as a fellow and then he went straight to CERN for two years with a grant, from 1 December 1959 to 30 November 1961.

During his period at CERN, Selleri met his first wife, Verena Badertscher, with whom he got married on 4 June 1962. They had two daughters, Cristina and Elisa.

³⁸ “[...] Da lui ho imparato a insegnare. Bruno Rossi era un didatta favoloso, si preparava in una maniera incredibile le lezioni, non lasciava nulla al caso, ed essendo molto preparato riusciva a parlare in modo estremamente chiaro. Ricordando come lui insegnava, ho imparato” (Bonolis, Melchionni 2003, pag. 340).

³⁹ “[...] Io ho sempre considerato come attività fondamentale l’insegnamento, perchè bisogna pensare che quello che conta è preparare il futuro. Ho sempre curato molto i miei allievi, lavorando insieme a loro. Non esiste altro sistema che lavorarci insieme” (Bonolis, Melchionni 2003, pag. 352).

⁴⁰ Magazine founded in 1931 by the Italian astronomer Guido Horn d'Arturo. It was issued until 1986.

The former was born in Paris in 1962, during the year Selleri spent in Saclay as a foreign associate (*collaborateur étranger*); the latter was born in Bologna in 1966. Franco and Verena got divorced on 17 December 1981. Then Franco experienced a second wedding with Angela Pizzi on 9 July 1983, having a daughter, Diana, in 1988.

Recollections by Angela Pizzi surely played a paramount role in outlining a personal, quite peculiar painting of Franco Selleri, including the flaws of his very sharp mood. Some excerpts on his disposition and idiosyncracies are quoted below, together with some impressive notes on his approach to popularization and teaching:

Franco was anti-USA, he boycotted Coca Cola, we could not even mention the name.

[...] After meeting each other, we lost track of each other for several years. Then, in 1978, a friend brought me to Franco, who had already separated and lived at his home in Via postiglione in Bari. He often invited friends, perhaps this loneliness was a burden to him. Franco was very hospitable, unlike in the future, when he became closed, reluctant to open up to others and unavailable. Such attitude of availability, of being with others, almost frivolously, I have never found again. And not because he did not know how to laugh or joke, on the contrary, he was full of irony, but he was no longer in a good disposition towards others, to be with others, to listen to others, to be available to others. He behaved this way at that stage of his life.

[...] We did not have that many meetings in Bari because he did not have great esteem of his colleagues, he did not have a good opinion of anyone, with the exception of Garuccio and his wife Enza. He preferred to share his views only outside of Bari.

[...] If Franco thought someone could not follow him and his speech, he'd let it go.

[...] It happened that you met people, you became acquainted with them and then they disappeared. Relationships ended abruptly in scientific disagreements. Franco was a strict person! "If someone did something to me", he said, "I would not talk to him anymore. It does not matter what he did to me in the past, if I have to be grateful to him. I disagree and I do not want to hear about it anymore".

[Regarding popularization] my impression was that he did not give much weight to this activity. He did it very well, he would conscientiously prepare himself, giving everything he had to give. But it was an appendix, nothing fundamental.

Quite different was his approach towards teaching. It was really essential for him. He was really pleased with his relationship with his students. He loved creating new prospects in the minds of those who were truly passionate. But with those who did not get there or were limited, he could also be contemptuous, dismissive. Franco had a very strict attitude for which the assessments were perfect, precise (Excerpts taken from my interview with Angela Pizzi, Bari, 11 July 2017. My underline).

Going back for a while to CERN period, Selleri worked there with several scholars, introducing various papers concerning the so-called Peripheral Model and the One Pion Exchange (further referred to as OPE) Model.

These two achievements allowed him to acquire a reputation in the world of particle physics. As a consequence of this new state of celebrity, Selleri decided really soon to go abroad.

He spent one year (1962-1963) in France, at Laboratoire de Physique Corpusculaire à Haute Energie, Centre d'Etudes Nucléaires de Saclay at Gif-sur-Yvette.

From 1963 to 1965 he was in USA at the Cornell University in Ithaca, attracted by the presence of Nobel Prize Hans Albrecht Bethe. In that period he also spent one year at Brookhaven National Laboratories. But, once in the USA, Selleri experienced an abrupt turning point concerning his thoughts on physics:

[...] slowly, I started to develop a critical attitude towards contemporary physics. I mean I had a very strong drive. I liked physics very, very much and I was very active, but it was soon evident that there were problems, fundamental problems in physics.

[...] I slowly realized that [...] there were great problems in physics. And somehow I got very soon the conviction that the problems came from the foundations of quantum mechanics. That is to say, the problems in elementary particle physics are due to the fact that quantum mechanics is poorly understood and anyway is a very abstract idea (AIP, 2003).

In 1965, Selleri decided to come back to Italy. Thanks to some documents found in ASUB, I managed to retrace his career in Bologna.

He was a lecturer in charge of Matter Structure from 1 November 1961 to 31 October 1962 and from 1 November 1966 to 31 October 1967. On 20 September 1963 he acquired the free lecturing qualification in Theoretical Physics, eventually confirmed in May 1969. In this respect, the final validation by the University of Bologna, ratified in the assembly on 29 November 1968 is cited below:

The scientific activity of prof. Franco Selleri during the years 1963-1968 took place, in the first two years, at the Laboratory of Nuclear Studies at the Cornell University, and then at the Physics Institute at Bologna University for two years. Since 1 November 1967 he worked at Physics Institute at Bari University. His scientific activity has been carried out almost entirely in the field of the strong interactions of elementary particles, a theoretical research related to the free teaching in theoretical physics. Prof. Franco Selleri enhanced his scientific production in these five years submitting 14 papers [...].

[...] The didactic activity of Prof. Franco Selleri consisted in having carried out at the Faculty of Science the teaching assignments of “fisica delle particelle elementari” in the academic year 1965/66 and of “struttura della materia” in the academic year 1966/67.

The Faculty expresses unanimously a favourable opinion to the confirmation of the free teaching in theoretical physics to prof. Franco Selleri⁴¹ (ASUB).

Thanks to this latter document, I can state that in Bologna University, Selleri was also a lecturer in Elementary Particle Physics from 1 November 1965 to 31 October 1966.

Anyway, once in Italy, Selleri found in Bologna a very detrimental workplace, summarized by him as follows:

[...] Bologna University was in trouble in physics at the time. There were struggles and problems. They had made mistakes in the choices of people to head the physics institute, that is my feeling anyway.

⁴¹ “L’attività scientifica del prof. Franco Selleri nel quinquennio 1963-1968 si è svolta, nei primi due anni, presso il ‘Laboratory of Nuclear Studies’ della Cornell university (U.S.A.) e poi presso l’Istituto di Fisica dell’Università di Bologna unibo per altri due anni: dal 1/11/67 egli lavora presso l’Istituto di Fisica dell’Unoversità di Bari. L’attività scientifica si è svolta quasi interamente nel campo delle interazioni forti delle particelle elementari, ricerche teoriche affini con la libera docenza in fisica teorica.

Il prof. Selleri presenta in 14 lavori la sua produzione scientifica nel suddetto quinquennio [...].

[...] L’attività didattica del prof. Selleri consiste nell’aver svolto presso la facoltà di scienze gli incarichi di insegnamento di “fisica delle particelle elementari” nell’anno accademico 1965/66 e di struttura della materia nell’anno accademico 1966/67. La facoltà unanime esprime parere favorevole alla conferma della libera docenza in fisica teorica al prof. Franco Selleri” (ASUB).

So that was strongly reflected on the “climate”, I mean the political and cultural climate. In Bari there was this new university and I met with Bari people in a congress and they told me they would be happy if I came to their university, so I left Bologna and its problems and came here (AIP, 2003).

Then, after spending two years teaching in Bologna University and after taking up the course of Matter Structure there, Selleri gave it up to relocate to the University of Bari.

He started getting teaching assignments at Bari University since 1 November 1967, for the academic year 1967/68, as external lecturer⁴².

As further confirmation of the exact period in which Selleri started teaching in the University of Bari, I found some notes in ASUB and AGAB: On 3 January 1968, the Rector of the University of Bologna formalized the fact that Selleri Franco, assistant professor of “Matter Structure” at Bologna University for the academic year 1967/68, had ceased as a result of withdrawal with effect from 1 November 1967. In addition, G. Lanza, the Inspector General of the Italian Treasury, sent a letter on 13 January 1968 asking the recovery of debts for the figure of 500 thousand Italian Lire (500,000 Lire corresponding to 260 €⁴³) to Selleri (for two checks already paid for the period 1/11/67-31/12/67) because in the academic year 1967/68 Selleri was assistant professor at University of Bari and therefore he had ceased from teaching “Matter Structure” at Bologna University since 1 November 1967.

Interestingly, in a letter dated 10 April 1967, Selleri had asked to the University of Bologna to get the teaching of “Matter Structure” back for the

⁴² Some documents found in the AGAB confirm Selleri’s stay in the surroundings of Bari in November 1967. On 9 October 1967, on his 31st birthday, Selleri asked Bologna University to confer him the mandate of Institutions of Theoretical Physics (IFT in the following) for the academic year 1967/68 at Bari university. Moreover, Selleri appeared to be member of SIF on 30 November 1967 and his address was: Physics Institute-Bari University-Via Amendola 173. in a note dated 11 January 1968 his address was in Vigna del Re-Palese, near Bari. Moreover, in the previous page, it is possible to underline the exact beginning of Selleri’s teaching in Bari University by the assembly in Bologna University, that is, 1 November 1967.

⁴³ Obviously the exchange rate mentioned is purely mathematical, and does not take into account the very different economic weight of the figure in the 1960s, when the cost of living in Italy was completely different from what it is today.

academic year 1967/68, too. So, between April and October 1967, Selleri experienced the thought of changing location.

In the AGAB Archive I also found several papers that shed light on Selleri teaching in Bari:

Academic year/Period	Topic	Note
01/11/1967-14/12/1967	Statistical Mechanics	Vacated
01/12/1967-31/10/1968	Institutions of Theoretical Physics	
01/11/1968-31/10/1969	Institutions of Theoretical Physics	Vacated
01/11/1968-31/10/1969	Mathematical Methods of Physics	
02/12/1968-31/10/1969	Quantum Theory	Additional course
01/11/1969-31/10/1970	Mathematical Methods of Physics	
01/11/1971-31/10/1972	Quantum Theory	
01/11/1972-31/10/1973	Quantum Theory	
13/12/1973-31/10/1974	Theoretical Physics	
01/11/1974-31/10/1975	Theoretical Physics	
01/11/1975-31/10/1976	Quantum Theory	
01/11/1976-31/10/1977	Theoretical Physics	Soon vacated
01/11/1976-31/10/1977	Quantum Theory	Ceased 30/1/1977
01/11/1976-31/10/1977	Atomic Physics	Ceased 22/2/1977
31/01/1977-31/10/1977	Institutions of Theoretical Physics	
01/11/1977-31/10/1978	Institutions of Theoretical Physics	
01/11/1978-31/10/1979	Quantum Theory	
01/11/1979-31/10/1980	Quantum Theory	
25/02/1981-31/10/1981	Quantum Theory	Free of charge
01/11/1981-31/10/1982	Quantum Theory	Free of charge
01/11/1982-31/10/1983	Quantum Theory	Free of charge
14/02/1981-13/02/1984 ⁴⁴	Extraordinary Professor Institutions of Theoretical Physics	
14/02/1984-31/10/2006	Full Professor Institutions of Theoretical Physics ⁴⁵	
01/11/2006-31/10/2008	Stay on active duty as Full Professor	
01/11/2008-31/10/2009	Out of place	
01/11/2009	Retired	

⁴⁴ Selleri turned out to be an extraordinary professor since 01/11/1980 but actually was given a permanent post as lecturer on 14/02/1981. After three years he became Full Professor after the reports of the relevant university bodies.

⁴⁵ During the academic years 1993/1994 and 1994/1995 Selleri was also lecturer in Relativistic Physics. He also taught History and Foundations of Physics as well as Physics Education in several advanced Schools.

As far as Selleri's activity is concerned, during the period in which he relocated to Bari, he slowly moved from PP to FQM. On this major change he stated that,

[...] In the beginning I thought that I would have made research both in particle physics and in quantum mechanics, but slowly I was completely attracted by the foundational problems (AIP, 2003).

His research on FQM covered at least 35 years with several books and up to 125 English papers on several groundbreaking topics.

During his stay in Bari, already full oriented towards research on FQM, Selleri tried two national competitions to become full professor in theoretical physics institutions; about these I managed to find several documents. He sat the first one on 23 October 1975 (n. 155/c). He was praised for his preparation. However, he did not get the position.

Common remarkable quotations by all six commissioners' assessments of 1975 are aimed at showing the importance of Selleri's work in his research fields of PP and are related to the substantive, far-reaching research on Peripheral model and to Selleri's papers, "widely cited internationally":

- 1) I consider remarkable the candidate's works on the phenomenology of strong interactions, and in particular on the peripheral model;
- 2) Important are his works on multi peripheral model;
- 3) [...] His works on the peripheral model, which has had wide international resonance, are particularly noteworthy;
- 4) The most important line of research of the candidate, which is also the most important for the purposes of this competition is the work on the peripheral model [...];
- 5) [...] of recognized importance are the works on the peripheral model; interesting those on the pion production in the nucleon-nucleon impact;
- 6) Of particular importance are the works on the peripheral model, widely cited internationally; the work on the implications of the condition of unity and the results on the pion production in the nucleon-nucleon collision⁴⁶ (AGAB-FS).

⁴⁶ "1) Considero importanti i lavori del candidato sulla fenomenologia delle interazioni forti, ed in particolare sul modello periferico.

2) Importanti i suoi lavori sul modello multiperiferico;

But only three of the same six commissioners' spent a few words on HoP and FQM, still confirming a minimal impact for the subject at the time:

- 2) I appreciated the commitment and seriousness of his recent work in the history of physics;
- 5) His studies on QM from an ideological-historical-social point of view deserve a separate mention;
- 6) [His] scientific activity shall cover the FQM with a particular interest in the theory of hidden variables⁴⁷ (AGAB-FS).

His second national competition was held on 11 August 1980 (n. 236) and it was the one in which Selleri finally succeeded in becoming full professor in Theoretical Physics Institutions (further referred in the text as IFT⁴⁸) to University of Bari with official appointment on 14 February 1984. The final assessments by the commissioners are reported below:

[...] [Franco Selleri] submits a plentiful scientific production published entirely in international journals that essentially consists of two periods of approximately equal duration.

In the first period the candidate has dealt with problems of phenomenology of the strong interactions among elementary particles obtaining brilliant results in the formulation and applications of the peripheral model of which he can be considered one of the initiators.

In the second period he dealt with the problems related to Foundations of Quantum Mechanics, obtaining original results, in particular on the extent of Bell's inequality.

3) [...] Si segnalano in particolare I lavori sul modello periferico, che hanno avuto larga risonanza internazionale;

4) Il più notevole filone di ricerca del candidato, che è anche il più importante ai fini del presente concorso è quello costituito dai lavori sul modello periferico [...];

5) [...] di importanza riconosciuta [sono] I lavori sul modello periferico; interessanti quelli sulla produzione di pioni nell'urto nucleone-nucleone;

6) Di particolare rilievo I lavori sul modello periferico, ampiamente citati in ambito internazionale; il lavoro sulle implicazioni della condizione di unitarietà ed I risultati sulla produzione di pioni nell'urto nucleone-nucleone" (AGAB-FS).

⁴⁷

2) "Ho apprezzato l'impegno e la serietà dei suoi recenti lavori di storia della fisica;

5) Una segnalazione a parte meritano i suoi studi sulla meccanica quantistica da un punto di vista ideologico-storico-sociale;

6) La sua attività scientifica verte...sui fondamenti della meccanica quantistica con particolare interesse per la teoria delle variabili nascoste" (AGAB-FS).

⁴⁸ IFT is the Italian acronym for Institutions of Theoretical Physics, namely, Istituzioni di Fisica Teorica.

The committee appreciates the high scientific and cultural level that emerges from the production of the candidate and considers him one of the most qualified to be included in the list of winners of this competition⁴⁹ (AGAB-FS).

I think it is substantial to submit here three documents found in the AGAB which reveal the different weight given by the committees to the research areas experienced by Selleri⁵⁰. The first one is related to the Mathematics, Physics and Natural Sciences Faculty Board of 17 October 1980 by which Selleri was chosen as full professor from the two candidates who submitted their application at the University of Bari⁵¹:

Prof. Franco Selleri has carried out his scientific activity dealing with:

- a) phenomenology of strong interactions of elementary particles. Of great importance are the relevant contributions in the formulation and application of the peripheral model;
- b) foundations of quantum mechanics, with particular emphasis on the results concerning the extension of Bell's inequality.

His scientific activity is witnessed by the very high number of publications produced in international journals. Professor Selleri has carried out a remarkable teaching activity in various subjects of the degree course in Physics at the Faculty of Sciences of the University of Bari; of great interest are the seminars held abroad and the courses held at the School of Advanced Physics.

[...] The Faculty Board [...] unanimously decides to call Prof. Franco Selleri to fill from 1 November 1980 the post of permanent professor of IFT put out to contest in group 236 (first discipline IFT) [...] with the following motivation:

“For the most conspicuous and diverse scientific production and for the most extensive didactic activity and most pertinent to the discipline according to the job vacancies competition, the Faculty Council is pleased to call Prof. Franco Selleri to fill the chair of IFT” (AGAB-FS).

The second one deals with the joint meeting of the Bachelor of Physics degree program and the Department of Physics Board held on 18 January

⁴⁹ “[...] [Franco Selleri] presenta una copiosa produzione scientifica pubblicata interamente su riviste internazionali che si articola sostanzialmente in due periodi di circa uguale durata. Nel primo periodo il candidato si è occupato di problemi di fenomenologia delle interazioni forti tra particelle elementari ottenendo importanti risultati nella formulazione e nelle applicazioni del modello periferico di cui può essere considerato uno degli iniziatori. Nel secondo periodo si è occupato dei problemi relativi ai Fondamenti della Meccanica Quantistica, ottenendo risultati originali, in particolare sull'estensione della disuguaglianza di Bell. La Commissione apprezza l'alto livello scientifico e culturale che emerge dalla produzione del candidato e lo ritiene uno tra i più qualificati per essere inserito nella rosa dei vincitori del presente concorso”(AGAB-FS).

⁵⁰ The original Italian texts of the three documents are in Addendum 3, pages 948-949.

⁵¹ The two applications were submitted by Franco Selleri and Benedetto Antonio de Tollis.

1984 in which they introduce their report proposal on the three-year period of Selleri's extraordinary professorship (1981-1983), accepting it unanimously:

Prof. Franco Selleri, winner of the competition n. 236 (first discipline: IFT) was called to hold the chair of IFT at the Faculty of Science of the University of Bari from 1 January 1981.

He has been entrusted with institutional didactic role of professor of "Institutions of Theoretical Physics B" for the applicative and didactic addresses of the Degree Course in Physics. In the [same] years 1980/81, 81/82 and 82/83 he was also given free of charge the teaching of "Quantum Theories" for the degree course in Physics. [In the same period] Prof. Selleri has been the supervisor of a number of degree theses in Physics in the field of foundations of quantum mechanics and in the field of physics teaching.

Prof. Selleri has always devoted himself with great commitment and dedication to the development of teaching activities, writing the handouts of the entire institutional course and making use of his long experience as a researcher in various fields of theoretical physics (from Elementary Particles to Foundations of Quantum Mechanics) to keep courses always up to date and at an excellent level.

Prof. Selleri has twice been a member of the selection boards for open competitions for university research job vacancies for group n. 86 (theoretical physics), at the universities of Bari and Parma respectively.

He has also carried out extensive and intensive seminar work, both as a reporter and as an organizer.

As a speaker, Prof. Selleri has held more than 20 scientific seminars on foundations of quantum mechanics at Universities and Scientific Centers in Italy and abroad (Austria, Belgium, France, Germany, Great Britain, Netherlands) and over 20 seminars on scientific and philosophical issues at high schools and cultural circles in Italy. He has also organized several scientific seminars at the Institute (now Department) of Physics of the University of Bari with speeches by Italian and foreign speakers, as well as two international conferences: one in Perugia on the dualism wave-particle (April 22-30, 1982) and one in Bari, at the Department of Physics on: "Open Questions in Quantum Physics" (May 4-7, 1983).

In the autumn of 1983, Prof. Selleri developed a project aimed at establishing contacts with about 100 Italian companies (or Italian representatives of foreign companies) with the aim of encouraging (through scholarships, degree theses or other) the integration of young graduates in Physics of the University of Bari in the world of work.

The initiative was approved by the Bachelor Council of Physics⁵² and the Council of the Department of Physics⁵³ and it was fully taken care of by Prof. Selleri.

⁵² Consiglio di Corso di Laurea, CCdL

⁵³ Consiglio di Dipartimento, CdD

The CCdL in Physics and the CdD in Physics express a strong appreciation and a highly positive judgement for the intense didactic, organizational and cultural activity of Prof. Selleri and for the way in which he has generally fulfilled his academic duties in the period of extraordinary professorship (AGAB).

And finally the third and last one stresses the official appointment as full professor given by the Faculty Board Commission:

Professor Franco Selleri took up his position as an extraordinary professor in IFT at the Faculty of MPN Science at the University of Bari on 1 June 1981. The faculty expressed a favourable opinion on the didactic and organizational activity of Prof. Franco Selleri. His research activity, which has been includes 11 publications, has been developed on Foundations of Quantum Mechanics, in particular on the paradox of Einstein-Rosen and Podolski. It is the opinion of this Commission that Selleri's approach to this issue sometimes neglects the physical aspect in favour of the ideological one. However, the Commission proposes that he be appointed as a full professor (Signed by proff. Giuliano Preparata (Bari), Luigi Arialdo Radicati di Brozolo (Pisa), Claudio Villi (Padova)) (AGAB-FS).

Angela Pizzi provided some evidence which emphasize the care taken by Selleri in order to reach his goal, as well as other subtleties of his character. She convincingly argued that:

Franco went through a very long process before he got to his permanent chair. He made an intense diplomatic effort to ensure that no one prevented him from getting to the professorship. He really thought that they would mobilize so that he would not have it. He took steps to weave diplomatic networks to prevent defeat from happening again. He made many trips, dialogues and meetings, even with those who did not see him favourably or did not consider his research truly valid or significant. He had done everything in his power to avoid having them as enemies. He did not want to make enemies. I remember phone calls he made to find out who was supposed to be on the committee. I remember all the work he did, of which I was very surprised. It was the first time that I saw him being capable of using diplomacy, of weaving relationships, of not going to the clash, something he would often do before.

All these efforts were rewarded by the victory which happened before our marriage in 1983. He was very happy after he made it. Although obviously for him it was the recognition of something. He was convinced he was so worthy that he had to be recognized. They had done their duty, in short. In the end, they had not given him

anything he did not deserve (Interview with Angela Pizzi, Bari, 11 July 2017. My underline).

Selleri also experienced a consulting role in a Swedish company, called Scanditronix⁵⁴, involved in the field of accelerators for sanitary use, specifically for radiation therapy. He organized two workshop in Bologna and Ravenna (at the School of Professional Nursing) in May 1981.

In my interview with Michele Barone (who shared with Franco Selleri a long friendship, an intense scientific debates as well as the political activity) it emerges a knowledge in medical physics and short-life radioisotopes by Selleri. Barone pointed out that:

When I became a manager in the company, Franco was in serious financial difficulties after his divorce. Albeit he was doing a very intense editorial job, he still continued to have a lot of financial problems, thereby he decided to become a consultant for the sale of cyclotrons.

[...] Selleri came to the Italian Communist Party (PCI) section in Bitonto for meetings and lectures, he had known a senior CGIL union leader, Renzo Rosso, a comrade and a partisans, who knew people in Emilia-based cooperatives. Hence Franco had the opportunity to meet several managers which could fund the purchase of our radiotherapy equipment and campaigned for this equipment to be acquired.

[...] Franco published in *Nuovo Saggiatore* some papers concerning medical physics and had a good background on short life radioisotopes, on their action and so forth.

[...] So, Franco had a big background on short-lived radioisotopes, what they did, how cancer was treated, etc. And in our tour we went to Malpighi and two or three other hospitals. So he had this contract as a cyclotron salesman. The director of the cooperatives was called Andreotti, he was a physicist, and had been introduced to Franco by Rosso. Scanditronix, because things had gone well, wanted to move me to Ispra, to follow the cyclotron and wanted to have an office in Italy. But I had met my wife in Geneva and we lived there. I opposed them and they fired me.

And so also the collaboration with Franco ended, whose interest had already diminished anyway. (Interview with the Michele Barone, Bitonto (BA), 1 June 2019. My underline).

⁵⁴ Scanditronix Magnet AB is a Swedish company producing magnets and coils for particle accelerators for medical, research and industrial applications and nowadays it is currently active. See <https://www.scanditronix-magnet.se/>. Accessed 24 September 2020.

Then, Selleri started a new activity of research almost exclusively on FRT since 1994⁵⁵, writing 64 articles, not all published, and some books. A possible reason for this involvement might come from the following evidence:

[...] the idea of real waves really requires a medium. And in recent years I switched to the foundations of relativity to see if a logical space existed for such a medium...With relativity I could go much deeper and I have basically a new theory that can replace special relativity [...]. I was influenced by Prokhovnik and his books and by John Bell who wrote an article on the foundations of special relativity (AIP, 2003)⁵⁶.

The research of Franco Selleri regarding the FRT has been just as important for him as that concerning the FQM, enough to make him say that:

[...] A point I would like to make is to stress again the importance of what I have done in the foundations of special relativity. Because now we have a completely new theory which is different from relativity[...].

[...] So I was unable to build a new quantum theory, but at least I built a new relativity and I strongly believe it is correct (AIP, 2003).

Selleri continued teaching in the University of Bari since his retirement in 2009. His last books, “Weak Relativity” was published in Italian in 2011, whereas the English edition was released posthumously in 2015.

Selleri kept on working steadily until the end, although his last years were blighted by a severe illness. He passed away in Bari on 30 November 2013, at the age of 77.

⁵⁵ I have found two papers showing Selleri’s contribution on Foundations of Relativity Theory has already started in 1990: 1) Selleri F. (1990), “Space-Time Transformations in Ether Theories”, *Zeitschrift für Naturforschung*, 46A, pp. 419-425. 2) Selleri F. (1990), “Special Relativity as a Limit of Ether Theories”, in *Physical Interpretations of Relativity Theory*, C. Duffy (ed.), London: British Society for the Philosophy of Science, pp. 508-514. For the sake of precision, there are some documents and recollections that seem to date back his embryonic interest as far as 1984.

⁵⁶ Prokhovnik S.J. (1967), *The logic of special relativity*, Cambridge University Press; Bell J.S. (1976), “How to Teach Special Relativity”, *Progress in Scientific Culture* I (2). With the former there is an impressive correspondence I reported in the following, not well readable, though, for the poor state of conservation.

CHAPTER 1

SELLERI AND PARTICLE

PHYSICS (1958-1970)

“I have done a terrible thing, I have postulated a particle that cannot be detected”.

Wolfgang Ernst Pauli (1900-1958)⁵⁷

1.1-A REALLY GENERAL GLIMPSE ON THE DEBATE ON PARTICLE PHYSICS

Let us start by briefly focusing on the landscape concerning elementary particle physics in the period just before and during Selleri’s years of his early researches on Particle Physics (PP).

The field of elementary particle physics has played a remarkable and dominant role in world physics in the last century.

There are obviously hundreds of interesting works able to portray, much better than me, the true core of those years. Nevertheless, I decided to introduce mine overview, operating a peculiar choice for my narration. The sources I mainly used here include a paper by Silvio Bergia (1935-)⁵⁸, the book by Bonolis and Melchionni cited in the Introduction, the book by Bonolis⁵⁹, a site found on Internet⁶⁰, and a paper by Nesvizhevsky and Villain⁶¹.

Of course, this is only an attempt to present the abovementioned landscape, so this paragraph is merely a sketch and definitely not a thorough analysis.

⁵⁷ <https://archives.library.illinois.edu/erec/University%20Archives/1515022/OriginalFiles/LITERATURE/PAULI/Wolfgang%20Pauli%20-%20Wikiquote.pdf>. Accessed 24 September 2020. Statement of 1930, after postulating the existence of the very elusive neutrino. As quoted by Frederick Reines, in his "Foreword" to *Spaceship Neutrino* (1992), (<http://books.google.com/books?id=JB0dejRmaMC>) by Christine Sutton, p. XI. For Wolfgang Pauli see also <https://www.nobelprize.org/prizes/physics/1945/pauli/biographical/>. Accessed 24 September 2020.

⁵⁸ Bergia S (2004), *The way we were: bubble chambers pictures, pion-nucleon interactions and polology*, mentioned in the following as (Bergia 2004). The work is an authentic testimony, intimately close to Selleri. Bergia and Selleri graduated in the same day in Bologna and then they had a long scientific relationship. Silvio Bergia graduated the same day of Selleri and they worked together in Bologna. See <http://www.inchiestaonline.it/author/silvio-bergia/>; Accessed 24 September 2020. A nice paper on the contributions by Bergia to FQM and RT is written by Tarozzi and Tassani, see <https://isonomia.uniurb.it/vecchiaserie/2007tarozzitassani.pdf>. Accessed 24 September 2020.

⁵⁹ Bonolis L. (2008), *Maestri e allievi nella fisica italiana del Novecento*, Pavia: La Goliardica Pavese, (Bonolis 2008) in the following. Luisa Bonolis, alone and jointly with Maria Grazia Melchionni has accomplished an enormous amount of preservation work about the oral history and memory of great Italian physicists involved in the physics of the XX century.

⁶⁰ (abyss.uoregon):http://abyss.uoregon.edu/~js/21st_century_science/short_history_of_particles.html. Accessed 24 September 2020.

⁶¹ Nesvizhevski V., Villain J. (2017), “The discovery of neutron and its consequences (1930-1940)”, *Comptes Rendus Physique* 18 (9-10), pag. 592-600.

I decided to start by recalling the discovery of the neutron by James Chadwick in 1932, after the experiments of Walther W. G. Bothe⁶² and Herbert Becker in 1930⁶³, as well as Frédéric Joliot and Irene Curie⁶⁴ in 1932⁶⁵, who basically conformed themselves to the outcome of the German scholars. Bothe and Becker showed that α -particles were emitted from a bar of plutonium, hitting then a plate of beryllium. From this plate was produced an extremely penetrating radiation, much higher than that emitted by protons, and even higher than gamma rays, the most penetrating radiation known at the time. In order to accelerate protons to the observed speeds, it would have taken a much higher energy than that of gamma rays. Nevertheless, Bothe and Becker, like Joliot and Curie after them, remained on the hypothesis of protons within the emitted radiation.

On 17 February 1932, James Chadwick, at the Cavendish Laboratory in Cambridge, wrote a seminal letter to the editor of Nature titled *Possible existence of a Neutron*. The letters appeared on Nature only 10 days after⁶⁶.

Chadwick made an impressive breakthrough compared to his predecessors, rejecting the idea of the presence of protons in the new and real penetrating radiation and coming to a revolutionary hypothesis with further evidence, just suggesting the only alternative to the relinquish of the application of the conservation of energy and momentum in these collisions, that is, a radiation made by particles:

[...] results [...] are very difficult to explain on the assumption that the radiation from beryllium is a quantum radiation, if energy and momentum are to be conserved on the collisions. The difficulties disappear, however, if it be assumed that the radiation consists of particles of mass 1 and charge 0, or neutrons (Chadwick 1932, pag. 312. My underline).

⁶² Awarded the Nobel Prize in Physics in 1954 “for the coincidence method and his discoveries made therewith”. <https://www.nobelprize.org/prizes/physics/1954/summary/>. Accessed 24 September 2020.

⁶³ Bothe W., Becker H. (1930), “Künstliche Erregung von Kern- γ -Strahlen”, Zeitschrift für Physik. 66, pag. 289.

⁶⁴ Both awarded the Nobel Prize in Chemistry in 1935 “in recognition of their synthesis of new radioactive elements”. <https://www.nobelprize.org/prizes/chemistry/1935/summary/>. Accessed 24 September 2020.

⁶⁵ Curie I., Joliot F (1932), “Émission de protons de grande vitesse par les substances hydrogénées sous l’influence des rayons γ très pénétrants”, Comptes Rendu, 194, pag. 273.

⁶⁶ Chadwick J. (1932), “Possible existence of a neutron”, Nature 129, pag. 312.

Chadwick's greatness lies in the fact that perhaps it was the only scholar who was ready for seeking a new particle, even because he had been searching for Rutherford's neutron⁶⁷ for a decade. He was awarded the 1935 Nobel Prize in Physics "for the discovery of neutron"⁶⁸.

Neutron has been the first subatomic particle with no electrical charge as well as the first unstable particle, and, after its discovery:

[...] the questions of the mechanisms of nuclear binding and decay [became] primary problems. Essentially, all of modern [particles were] later discovered in the attempt to understand nuclear interactions (abyss uoregon).

In addition, the neutron

became an essential actor on nuclear and elementary particle physics, and completely changed the whole research landscape. Enrico Fermi and his group applied it to artificial radioactivity, substituting neutrons to α -rays initially used by Joliot-Curie. [Fermi] also discovered that slow neutrons were more efficient than fast ones in certain nuclear reactions (Nesvizhevski, Villain 2017).

Among the several kinds of studies which developed from this discovery⁶⁹, I would like to focus on the one involving strong interactions, for their relationship with Franco Selleri's research.

The genesis of this research was inspired by some anomalies present in the behaviour of a proton close to an atomic nucleus and can be better explained this way: by measuring the deviation that a proton undergoes when it touches an atomic nucleus, anomalies were found since 1935 in this "diffusion" of a proton by the protons of the nucleus given that, instead of being rejected by the electric field, the proton was conversely attracted when it passed very close to the nucleus. In 1935 the Japanese Hideki Yukawa formulated the hypothesis that nucleons are tied in the atomic nucleus by a new field similar

⁶⁷ Rutherford put out the idea that there could be a particle with mass but no charge. He called it a neutron. There was no evidence for his ideas but Chadwick, his assistant director, was aware of this issue.

⁶⁸ <https://www.nobelprize.org/prizes/physics/1935/summary/>. Accessed 24 September 2020.

⁶⁹ Among them it deserves to be underlined the study that led to nuclear fission, namely, the Fermi theory of nuclear beta decay, with the introduction of the neutrino and a brand new interaction, the weak one.

to that which electrically binds the electron to the atom. This field must have a very weak range, because nucleons must be almost in contact to interact; in this case it is very powerful and this has given the name of strong interactions to the phenomena that depend on this field. Yukawa assumed that the nuclear field manifested itself as analogous to the photon of the electric field; since the distance between the nucleons is very small, a photon of this type should have a very small wavelength because of the great energy.

Yukawa also inferred that in these nuclear forces, the mass of the conjectured particles (mesons⁷⁰) was about 200 electron masses.

Using a Wilson chamber to study cosmic rays, Carl D. Anderson (who already discovered the positron) and Seth H. Neddermeyer, as well as Jabez C. Street and Edward Stevenson, found a particle (mesotron) of mass between 100 and 300 electron masses. Specifically, in 1938, the mesotron was found to be up to 240 electron masses. The same Yukawa believed to have discovered the meson, i.e., the quantum of the nuclear forces. But later it was recognized that the mesotron was actually a brand new kind of particle, currently known as muon μ . Indeed, it had no strong interaction with matter. It could seem as an electron, more massive and unstable, though. Starting the 1941 until 1946, in a borderline environment as the bombing of the city of Rome, Conversi, Piccioni, and later Pancini⁷¹, found an outcome which falsified the Tomonaga and Araki theory, according to which the mesotron was supposed to be the Yukawa meson.

According to (Uniroma), in 1944, Conversi and Piccioni perform a test of Tomonaga and Araki's predictions on the absorption of mesotrons (without magnetic lenses), using an apparatus previously used for another

⁷⁰ From the Greek word *μεσόνιο*, whose meaning is "in the middle", due to its mass, intermediate between those of electron and proton.

⁷¹ For a deep overview of the Conversi, Pancini, Piccioni experiment, I referred to: <https://www.phys.uniroma1.it/DipWeb/museo/collezione%20Fermi/cpp3frames.htm>, (Uniroma) in the following. Accessed 24 September 2020. All excerpts inside, written in *italics* are a testimony by Marcello Conversi.

experiment⁷², namely an iron absorber. With this first experiment of 1944 they find a result equal to 0.49 ± 0.07 ret.coinc/h⁷³, according to Tomonaga and Araki's calculations which predict 0.55 ret.coinc/h for the fraction of mesotrons decaying in Iron. Subsequently:

Conversi and Piccioni design and build a new apparatus to perform a further test of Tomonaga and Araki's predictions, inserting "magnetic lenses" that select the mesotrons of a given sign. Conversi recalls that in 1943, during a convalescence leave spent in Rome, Pancini had discussed with him and Piccioni the opportunity of using magnetic lenses (first used in cosmic ray physics by Rossi in 1931, at Puccianti's suggestion) in mesotron experiments. Conversi knew this technique well for having used it in 1940-41. Shortly after 25 April 1945, Pancini, who in the meantime had participated in the partisan struggle against the Nazi-Fascists, began to collaborate with Conversi and Piccioni [...]⁷⁴ (Uniroma. Underline in the original text).

The experiment was concluded at the end of September 1945 and seemed to confirm the predictions of Tomonaga and Araki. In 1946, instead, the three Italian physicists, according to the words of Conversi, decided "to replace iron with a low atomic number absorber, consisting of [...] graphite". Again with respect to what we read in (Uniroma):

According to Conversi, one of the main reasons for this choice, which Piccioni remembers as the only reason, was the hope to observe high energy γ rays eventually emitted after the nuclear capture of mesotrons. Conversi recalled that this choice had been also motivated by the "*taste for experimental completeness*", by the desire to verify Tomonaga and Araki's predictions even at low Z, and "*graphite was the only material at low Z obtainable in a short time on the market in those days*".

The results (expressed in number of retarded coincidences/hour) were:

- without absorber, about 0.01 ret.coinc/h;

⁷² The experiment, performed in 1942, was aimed at improving the τ measurement performed by Rasetti. Conversi and Piccioni began to realize a system of fast coincidences (to measure τ with the method of delayed coincidences (Translated by Uniroma. Underline in the original text).

⁷³ ret.coinc/h is for retarded coincidences per hour.

⁷⁴ "Conversi e Piccioni progettano e realizzano un nuovo apparato per eseguire un ulteriore test delle previsioni di Tomonaga e Araki, inserendovi "lenti magnetiche" che selezionano i mesotroni di un dato segno. Conversi ricorda che nel '43, durante una licenza di convalescenza trascorsa a Roma, Pancini aveva discusso con lui e con Piccioni la possibilità di impiego delle lenti magnetiche (per la prima volta utilizzate nella fisica dei raggi cosmici da Rossi nel 1931, su suggerimento di Puccianti) nella sperimentazione sui mesotroni. Conversi conosceva bene questa tecnica per averla utilizzata nel 1940-'41. Poco dopo il 25 aprile del 1945, Pancini, che nel frattempo aveva partecipato alla lotta partigiana contro i nazifascisti, inizia a collaborare con Conversi e Piccioni [...]" (Uniroma. Underline in the original text).

- with absorber were recorded:

mesotrons	Fe (ret.coinc/h)	C (ret.coinc/h)
positive	0.67 ± 0.07	0.36 ± 0.05
negative	0.03 ± 0.03	0.27 ± 0.03

“This result was completely unexpected, and we first believed that there was some dysfunction in our apparatus”.

However, all the tests carried out on the experimental apparatus are positive. The observed effect is therefore not instrumental; in this regard, it is very significant that when negative mesotrons are selected the delayed coincidences “disappear” by replacing graphite with iron⁷⁵ (Uniroma).

The so-called C.P.P experiment (from the first letters of their surnames) revealed how the mesotron existing in cosmic rays is a completely different object (except for the mass) from Yukawa’s meson.

The importance of C.P.P. experiment rests in the words using by Luis Alvarez in his Nobel Lecture in 1968⁷⁶:

As a personal opinion, I would suggest that modern particle physics started in the last days of World War II, when a group of young Italians, Conversi, Pancini e Piccioni, who were hiding from the German occupying forces, initiated a remarkable experiment. In 1946, they showed that the “mesotron” which had been discovered in 1937 by Neddermeyer and Anderson and by Street and Stevenson, was not the particle predicted by Yukawa, as the mediator of nuclear forces, but was instead almost completely unreactive in a nuclear sense.

⁷⁵ *“Secondo Conversi, una delle principali ragioni di questa scelta, che Piccioni ricorda come l'unica ragione, era la speranza di osservare raggi γ di alta energia eventualmente emessi dopo la cattura nucleare dei mesotroni. Conversi ricordava che questa scelta era stata anche motivata dal “gusto della completezza sperimentale”, dal desiderio di verificare le previsione di Tomonaga e Araki anche a basso Z, e la “grafite era l'unico materiale a basso Z ottenibile in breve tempo sul mercato in quei giorni”.*

I risultati (espressi in numero di coincidenze ritardate/ora) furono:

- senza assorbitore circa 0.01 coinc.rit./h.;
- con assorbitore venivano registrate

mesotrons	Fe (coinc.rit./h)	C (coinc.rit./h)
positive	0.67 ± 0.07	0.36 ± 0.05
negative	0.03 ± 0.03	0.27 ± 0.03

“Questo risultato era completamente impreveduto, e noi credemmo dapprima che ci fosse qualche disfunzione nel nostro apparato”.

Tutti i test cui è sottoposto l'apparato sperimentale danno però esito positivo. L'effetto osservato non è pertanto strumentale; in proposito, è molto significativo che quando vengono selezionati i mesotroni negativi le coincidenze ritardate “scompaiono” sostituendo la grafite con il ferro” (Uniroma. Underline in the original text).

⁷⁶ <https://www.nobelprize.org/prizes/physics/1968/alvarez/lecture/>. Accessed 24 September 2020.

Most nuclear physicists had spent the war years in military-related activities, secure in the belief that the Yukawa meson was available for study as soon as hostilities ceased. But they were wrong.

(<https://www.nobelprize.org/prizes/physics/1968/alvarez/lecture/>. pagg. 241-242).

Enrico Fermi, Edward Teller and Victor F. Weisskopf as well as Shoichi Sakata, Takeshi Inoue, Walther Bothe and Robert Marshak also contributed to this feature. In 1947 they stressed the great deal of difficulty in revealing the Yukawa's meson, due to its very short life.

The solution to this long-standing problem was found in 1947 with the joint discovery, by Cecil F. Powell⁷⁷, César Lattes, Giuseppe Occhialini at the University of Bristol, of another particle, the pion π , the real meson of Yukawa's strong interactions. It was discovered using cosmic rays, cloud chamber and photographic emulsion. Pion's mass is slightly greater than a muon and, unlike the latter, interacts strongly with matter.

Furthermore, there was the discovery that the pion has three charge states, one of which, the π^0 , is virtual. Studying under a microscope the traces that the mesons left in nuclear emulsions, Powell, Occhialini e Lattes realized that they all had three distinct parts. At the beginning there was a short trace produced by a particle which was identified as the real meson of Yukawa and which was indicated by the letter π . The second part of the trace was attributed to a particle, indicated by μ , produced by the disintegration of the previous one and identified with the μ meson discovered by Anderson and Neddermeyer; the third part was [initially] attributed to the electron produced by the disintegration of the particle μ . The small difference between the mass of the meson π and that of the muon prevented them from being distinguished for a long time. [...] Mesons π , or pions, exist in three charge states. π^+ and π^- have a mass of 139,5 MeV/c², whereas the neutral pion π^0 is a little bit lighter, with a mass of 135 MeV/c².

⁷⁷ The only one in the group to be awarded with Nobel Prize in Physics in 1950.

The latter must be therefore virtual, that is, it can only appear for sufficiently short moments so that the mass fluctuation is acceptable by Heisenberg's uncertainty relations. Since neutron and proton have a mass difference of only $1.3 \text{ MeV}/c^2$, they cannot exchange a particle as heavy as the meson is. In the early 1950s, investigation in particle physics was in full swing, with a huge amount of new particles, often tagged as:

[...] a 'particle explosion' [or] 'a true proliferation of particles'. Classification of particles and their decays became a major activity. Particle decays were classified on the basis of their half-life into two groupings: decays by strong interaction processes have a half-life of order 10^{-24} seconds; while decays via weak processes have half lives of 10^{-13} seconds and even much longer. [...] Particles were classified by spin (fermions or bosons) and by the types of interactions in which they participate (hadrons or leptons) (uoregon).

The remarkable, outstanding breakthrough related to the rise of particle physics, whose effects have been at the foundations of the so-called "big science", has been possible only with the birth of accelerators in the 1930s. They have become an overwhelming forefront branch in physics when the substantive cosmic rays (a truly stronghold in the nuclear and particle physics) could not ever fulfil the needs of higher energies at stake in several processes discovered during the next years. Nevertheless, it is worth recalling the great input experienced by the study of cosmic rays, which made possible the discovery of the muon μ , the pion π , and the τ particle.

Moving on to the late 1950s and the early 1960s, when Selleri started his research activity after his graduation. The landscape of the period is thoroughly depicted in (Bergia 2004). The enormous growth of particle accelerators as well as the remarkable study on data analysis from the US bubble chambers by the Italian Universities is stressed:

The late Fifties were going to be eventful for physics in Italy. Italian physics had acquired international reputation thanks to the work of the Fermi group in Rome in the Thirties, Fermi's activities in the United States during the war and at the resumption of the normal research activities in the immediate postwar period.

And to the first rate results achieved in cosmic ray research by people such as Marcello Conversi, Ettore Pancini, Oreste Piccioni, Giuseppe (Beppo) Occhialini, Bruno Rossi and Edoardo Amaldi [who], prior to anybody else, had striven to preserve this heritage during and after the war [...].

[moreover] several first rank physicists had since contributed to revive research activities at an international level in their “Istituti”⁷⁸.

A gap, however, had in the meanwhile arisen between the New and the Old Continents. Since the pioneering work of Ernest Lawrence in the early Thirties, accelerators had been developed and installed in various sites in the United States, and, associate with them, targets for their beams that were at the same time particle detectors of an unheard of effectiveness, like the bubble chamber, invented by Donald Glaser in 1952. Europe, and Italy in particular, had necessarily remained far behind. Europe was trying to bridge that gap. CERN had officially started its activities in the fall of 1954. The single countries, Italy in the first place, were not in the condition to compete right away at the highest possible level. Nevertheless, in that very 1958 [...] the “Istituto Nazionale di Fisica Nucleare” (National Institute for Nuclear Physics) had nearly completed the construction, at its National Laboratories in Frascati, near Rome, of a large particle accelerator, a 1100 MeV electron synchrotron.

[...] It was Bruno Touschek, an Austrian theoretician to whom Amaldi had offered a position in Rome, to foster, in 1960, the construction, in the Frascati Laboratories, of an electron-positron collider. It was baptized AdA (acronym for the Italian *Anello di Accumulazione*). In 1962 it was actually accepted the French proposal to move AdA to Orsay, in order to exploit their linear accelerator. AdA became operative in 1964. It was followed, in Frascati, by Adone, a 3000 MeV electron-positron collider conceived by Fernando Amman in the years 1961-1963. It has been often commented that Adone missed the discovery of the J/ψ particle only because the beam energy was 50 MeV too low! While these events were going on, most of the physicists working at the Physics Institutes of the Italian universities had to base themselves on data collected elsewhere in the world, specifically in the United States. A peculiar form of international distribution of the forms of research activities was thus going to characterize those years, in particular as far as relationships between Italy and the United States were concerned. The accelerators and the bubble chambers were on the other side of the Ocean. On the other hand, in Italy the natural heirs of the particle physicists [...] had developed uncommon abilities in dealing with the so-called nuclear emulsions, a visual technique used for the study of cosmic rays which required skills not unlike those used in connection with the

⁷⁸ Also in the remarkable abovementioned books (Bonolis 2008) and (Bonolis and Melchionni 2003) the paramount importance of having founding fathers who saved physics in Italy is pointed out. The outstanding interviews with lots of great physicists, allow for an acknowledgment of several breakthroughs in the field. For instance, the influential role of Edoardo Amaldi is currently considered as pioneering in the post-war recovery and the development of physics in Italy. The new discoveries were subsequently experienced by Selleri

bubble chamber, and could turn out useful in the necessary phase of the data analysis. Italian physicists who had become known in the United States and were in close relationship with their colleagues active on the opposite shore of the Atlantic, paved the way for a fruitful collaboration by obtaining that part of that data analysis would be carried out in Italy. Giampietro Puppi was one of them, and the one who, from 1954, had taken over this task at the Istituto di Fisica Augusto Righi in Bologna. [...] A host of bubble chamber pictures taken at the Brookhaven Cosmotron started flowing towards Italy, to be scanned and analyzed by teams that were in turn becoming operative thanks to the conversion of part of the experts in the nuclear emulsion technique (Bergia 2004, pagg. 1761-1763. My underline).

[...] I wish to stress the double fundamental role played by Giampietro Puppi, who on the one hand, due to his scientific prestige, acted as the intermediary between the two communities, on the other transmitted to us, the young people joining one or the other of the research groups active in the Istituto, the enthusiasm for research (Bergia 2004, pag. 1764).

Looking at these references above, it appears clear how Puppi (the supervisor of Selleri degree thesis) created at the University of Bologna a fruitful environment for the further development of Selleri's research. Selleri started his research on strong interactions analyzing data by the bubble chamber film from Columbia University. This was a great development in Italian research on elementary particle physics. As Bergia put it:

[In] the late Fifties and the early Sixties [...] we were fascinated by the realm of the strong interactions, which meant for most of us, due to the subjects we had been involved in pion-nucleon interactions. They were on the one hand exciting, a lot of results being obtained in scores of experiments (Bergia 2004, pag. 1768).

But, maybe this new realm did not sound so exciting for everybody. As it is clearly stated by Bergia:

[...] On the other hand, to those who were more theoretically inclined it was rather frustrating to be confronted with a situation, which did not seem to allow really exploiting the instruments of what ought to be the basis of all theories, namely the quantum theory of fields (Bergia 2004, pag. 1768).

Concerning the foregoing frustration, Bergia remarked in a footnote of his paper that: “I remember Franco [Selleri] telling me something that sounded ‘I turn to weak interactions when I feel the need of a share of order and elegance’ ”⁷⁹ (Bergia 2004, footnote n. 18, pag. 1768. My underline).

As well summarized by Emilio Segrè in his 1980 book⁸⁰ *From X-rays to Quarks: Modern Physicist and their Discoveries*, the high-energy physics in elementary particle physics was the natural field of research for young, newly graduated physicists, like Selleri. And that, thanks to the expertise of Italian physicists in the field of photographic emulsions, as well as its great cheapness, Italy became a good hub for particle physics:

[At the end of World War II] [...] a new field was about to emerge, or, rather, an old field was about to blossom: the study of the so-called elementary particles. [...] It was also clear to many [...] that elementary particles would be the next frontier in physics. They presented the most fundamental problems, and new experimental tools, in the offing, would allow avances in uncharted territory. Other fields, such as nuclear physics and solid-state physics [...] presented less fundamental [problems]; that is, less general, less new, and less apt to change some established concepts or to introduce some new and unexpected ones. Other sciences [...] such as geophysics, astrophysics, and molecular biology, also looked very promising. For a physicist, however, they required a retraining in completely new fields and the development of new mental habits that are not easy to acquire [...]. [So], physicists who wanted to be in the forefront of their own science would veer toward particle physics. [...] At the end of the war cosmic rays were still the only source of high-energy particles. [...] Cosmic rays also had the distinct advantage of costing nothing, and especially for the European nations, devastated by the war, this was a godsend. [...] The most powerful and also one of the simplest advances was the improvement of photographic emulsions. [Italy, for instance] had thus found [that high-energy physics was] a cheap and extremely interesting field of physics. (Segrè 1980, pagg. 242-243. My underline).

⁷⁹ Vincenzo Paticchio, a scholar at Bari University I interviewed, stated that Selleri advised him to choose the weak interactions physics, more than the mainstream high-energy physics. And Paticchio has always been grateful to Selleri for this advice, finding the former field of research more interesting.

⁸⁰ (Segrè 1980) in the following. Book published by W.H. Freeman and Company, San Francisco. The original Italian edition, dated 1976 is titled: *Personaggi e scoperte della fiica contemporanea. Da Rutherford ai quark*, Mondadori, Milano

[...] The technique [of photographic emulsions] was very cheap. [...] This activity was well suited for the European countries in the immediate postwar era, and in particular for Italy [...]. Thus Italy became an important center for this type of investigations, which is one of the reasons for the prominence given to particle physics in Italy. (Segrè 1980, pagg. 252-253. My underline).

Let us spend now a few words on the crucial activity in Frascati, a turning point in the Italian research in elementary particle physics. Giorgio Salvini⁸¹, speaking about the choices behind the project and the place regarding the Italian way to the research in PP, stressed that:

It was evident [in 1949] that in the swarms of cosmic radiation there was a variety of interactions and that their interpretation was complex. This explains the worldwide trend, started in the 1950s, to build high-energy accelerators⁸² in Europe and the USA⁸³ (Bonolis 2008, pag. 113).

[...] the choice, in 1953, was for synchrotron. Then, in 1954, it was decided for the weak focusing. [...] It was a relevant choice. Actually, the winning idea of the strong focusing was born in 1952-53 [...]. [But] I had decided to be conservative. [...] I have to say that it was a happy choice. In that same year, instead at Cornell University they had decided to build a synchrotron [...] using the strong focusing and, in the end, it took them longer. We got there first, in a year or two [...]. In 1954 it was also decided where to locate the synchrotron [...]⁸⁴ (Bonolis, Melchionni 2003, pagg. 390-391).

[...] At the end of '58 and the beginning of '59, ahead of schedule, we set to work for the first experiences. [...] And the beam came, and it was the most intense beam an electrosynchrotron had ever produced in the world⁸⁵ (Bonolis, Melchionni 2003, pagg. 399-400).

⁸¹ Giorgio Salvini (1920-2015): A whole and remarkable presentation of his outstanding role in physics is included in Bonolis, Melchionni 2003 and Bonolis 2008. The details on the personal archive are at <https://sapienzadipfisica.archiui.it/oggetti/5782-patrimonio-archivistico-del-dipartimento-di-fisica-di-sapienza-universita-di-roma/>. Accessed 24 September 2020. See also <https://www.sif.it/riviste/sif/sag/ricordo/salvini>, and <http://matematica.unibocconi.it/articoli/ricordo-di-giorgio-salvini>. Both accessed 24 September 2020. For his political activity, see http://www.senato.it/leg/13/BGT/Schede/Attsen/00002145_iniz.htm. Accessed 24 September 2020.

⁸² Brookhaven had a cosmotron. In Saclay there was a Protosynchrotron. At Cornell there was the synchrotron with a strong focus. Frascati had the electrosynchrotron, like Lund, in Sweden, and Caltech, near Los Angeles.

⁸³ “Era evidente (siamo alla fine del 1949) che negli sciami della radiazione cosmica era presente una varietà di interazioni e che l’interpretazione era molto complessa. Questo spiega la tendenza mondiale, dagli anni cinquanta, a costruire acceleratori di alta energia in Europa e negli Stati Uniti” (Bonolis 2008, pag. 113).

⁸⁴ “[...] la scelta, nel 1953, fu per il sincrotrone. Poi, nel 1954, si decise per il foceggiamento debole. [...] Fu una scelta importantissima. In realtà era nata nel 1952-53 l’idea vincente del foceggiamento forte [...]. [Ma] io avevo deciso di essere conservatore.[...] debbo dire che fu una scelta felice. Infatti, in quello stesso anno anche all’Università di Cornell avevano deciso di costruire un sincrotrone [...] ma avevano scelto il foceggiamento forte e, alla fine, ci misero più tempo. Arrivammo prima noi di un anno o due [...]. Nel 1954 si decise anche la sede del sincrotrone [...]” (Bonolis, Melchionni 2003, pagg. 390-391).

⁸⁵ “[...] Alla fine del '58 e all’inizio del '59, con anticipo sui programmi, ci mettemmo al lavoro per le prime esperienze [...]. [...] E il fascio venne, ed era il fascio più intenso che mai un elettrosincrotrone avesse prodotto nel mondo” (Bonolis, Melchionni 2003, pagg. 399-400).

According to Carlo Bernardini's⁸⁶ recollections, in 1959 Bruno Touschek⁸⁷ proposed to change the synchrotron in a storage ring for the e^+e^- physics. So, in 1966 the building of Adone started. It was completed very soon, in 1967, but it started working only in 1969, due to the political turmoil of 1968.

Salvini vividly described the role of social unrest by saying that:

Adone had to be completed in 1967-68 but was ready only in 1970 due to delays caused by social and trade union unrest that pervaded universities, institutes and laboratories throughout Italy. They were difficult years at all social levels. Others machines arrived before Adone⁸⁸.

On the unlucky choices made for Adone, Bernardini also stressed that:

[Adone's Physics was competitive] until the advent of SPEAR⁸⁹, in 1974, after which we were outclassed. Adone had, all in all, an involuntary mistake, which was the choice of 1,5 GeV per beam: in retrospect, it was just one of the most unhappy choices that could be made. Because it was under the narrow resonances and far above the low resonances ρ , ω , ϕ . In fact the French at Orsay and also the Russians at Novosibirsk, although the latter had much less efficient machines, made a very refined physics of low energy narrow resonances [...]. They had an earlier idea to make what is now called a ϕ factory. Now we have it too, the Da ϕ ne machine⁹⁰.

⁸⁶ Carlo Bernardini (1930-2018): See Bonolis, Melchionni 2003 and Bonolis 2008. See also <https://www.sif.it/riviste/sif/sag/ricordo/bernardini>. Accessed 24 September 2020. The details on the personal archive will be soon at <https://sapienzadipfisica.archiui.it/oggetti/5782-patrimonio-archivistico-del-dipartimento-di-fisica-di-sapienza-universita-di-roma/>. Accessed 24 September 2020.

⁸⁷ Bruno Touschek (1921-1978): See Bonolis, Melchionni 2003 and Bonolis 2008. See also <http://www.lnf.infn.it/edu/materiale/Bruno%20Touschek.pdf>. Accessed 24 September 2020. The details on the personal archive are at <https://sapienzadipfisica.archiui.it/oggetti/5782-patrimonio-archivistico-del-dipartimento-di-fisica-di-sapienza-universita-di-roma/>. Accessed 24 September 2020.

⁸⁸ "Adone doveva essere completato nel '67-'68, ma fu pronto nel '70 a causa di ritardi dovuti alle agitazioni sociali e sindacali che pervasero università, istituti, laboratori in tutta Italia. Sono stati anni difficili a tutti i livelli sociali. Altre macchine arrivarono prima di Adone" (Bonolis, Melchionni 2003, pag. 407). This statement evidenced the fundamental theme of political unrest, critical for Italian history as well as for Selleri's activity, which we will deal with in the next chapter.

⁸⁹ SPEAR is the acronym for Stanford Positron-Electron Asymmetric Rings. See also <https://cerncourier.com/a/the-ring-on-the-parking-lot/>. Accessed 24 September 2020.

⁹⁰ "[La fisica di Adone è stata competitiva] fino all'avvento di SPEAR, fino al 1974, dopo fummo surclassati⁹⁰. Adone ha avuto, tutto sommato, un errore involontario, che è stato la scelta dell'energia, 1,5 GeV per fascio: visto a posteriori, era proprio una delle scelte più infelici che si potessero fare. Perché stava sotto le risonanze strette e troppo sopra le risonanze basse ρ , ω , ϕ . Infatti i francesi di Orsay e anche i russi di Novosibirsk, sebbene questi ultimi avessero delle macchine molto meno efficienti, hanno fatto una fisica delle risonanze strette di bassa energia molto raffinata [...]. Hanno avuto anzitempo l'idea di fare quella che oggi si chiama una ϕ factory. Ora l'abbiamo anche noi, la macchina Da ϕ ne" (Bonolis, Melchionni 2003, pag. 27).

I made these two quotations, about the late start of Adone for political reasons as well as the role of Daφne and the ϕ -factories because they will be crucial for Selleri's activity related to FQM and for his subsequent turning to FRT in the 1990s, on which I will expand in chapter two.

1.2-SELLERI'S DEGREE THESIS

In 1958 Selleri completed his thesis, *Analisi dell'interazione pione-nucleone*. I took into account some specifics concerning this unpublished work written in Italian that I found in his private archive. These peculiarities are included in the following quotations. Firstly, Michele Barone, in his eulogy for Selleri's death, appeared on CERN Courier in 2013, maintained that:

Puppi and other members of his group were analysing bubble-chamber film from Columbia University, focusing on pion-nucleon cross-sections in the 500 MeV-1 GeV energy region. Franco calculated the spectra at an incident pion kinetic energy of 960 MeV. For the doctor degree he presented a research on proton-proton collision which he soon developed in the widely adopted one-pion exchange model (Barone 2013).

Conversely, Bergia, in his above mentioned paper, stated that:

[...] Lindenbaum and Sternheimer in two works written in 1957 and 1958 had brought about that the resonance⁹¹ could play a role as an intermediate step of the processes of single pion production in pion-nucleon collisions. If so, the pion center of mass momentum spectra would exhibit a peak and a bump, respectively at higher and lower momenta, the peak arising from events in which the pion was "directly" produced in pair with the resonance, (extra-pions), the bump due to decay pions. The results turned out to be in good agreement with the model.

In the meanwhile Franco Selleri [...] collaborated as a theoretician to the last-mentioned research, calculating the spectra for the incident pion kinetic energy of 960 MeV (Bergia 2004. My underline).

In the introduction to his work, Selleri vividly described the core of his thesis. The overview of Selleri's thesis is fully displayed below⁹²:

⁹¹A 3/2, 3/2 resonance in pion-nucleon scattering had been discovered by Fermi's group in Chicago at the 450-MeV synchrocyclotron which had become operative in 1951.

⁹² The original Italian text is in Addendum 3, p. 950.

The aim of this work is to give a partial picture of the current state of research on the pion-nucleon interaction.

In the first chapter, we will take up an already well known topic: that of isotopic spin. It has been considered from a more formal point of view and less dependent on heavy analogies with angular moments. The logical trace of the treatment is that of Wick, but most of the calculations and the physical discussion of various points are independent.

In the second chapter we will deal with the low-energy pion-nucleon interaction (kinetic energy in the laboratory of the incident pion < 300 MeV) and we will show several experimental and theoretical reasons that make us believe in the existence of a resonance in the state $T=J=3/2$. In the following chapter the analysis of the high-energy pion-nucleon interaction will be made with particular regard to the isobaric model. By means of the latter, a comparison will be made with the experience at 810 and 950 MeV using the experimental data of Bagget; and of V. Borelli, A. Minguzzi-Ranzi, P. Waloscheck and V. Zoboli (which I would like to thank for having given me the partial results of their work at 950 MeV, before the publication). Some predictions will also be made, susceptible to experimental verification, which will have to confirm or not in the future the hypotheses of the isobaric model. Finally, in the fourth chapter, the pion-pion interaction will be considered, both on the basis of the model of Piccioni, Dyson and Takeda, and on the basis of the recent research by Chew and Low (Selleri 1958, pagg 1-2).

Concerning the isobaric model, Selleri maintained that

[...] the comparison with the experimental data shows a good agreement at 809 MeV, while the agreement is not as good at 950 MeV. However, it should be noted that at 950 MeV the statistics are still poor and it is hoped that, continuing the experience of Waloscheck et. al., the agreement will improve (Selleri 1958, pag. 67).

[...] At the energy of 810 MeV almost all interactions $\pi^+ + p$ must be elastic. This is a second possible verification of the isobaric model⁹³ (Selleri 1958, pag. 70).

By reading Selleri's work, some already well-defined characteristics which distinguish him plainly emerge. One can notice how he always demanded that several experiments be carried out, in order to confirm his predictions about the validity of the models and also to analyse them in detail.

⁹³ "Il paragone con i dati sperimentali mostra un buon accordo a 809 MeV, mentre l'accordo non è altrettanto buono a 950 MeV. Bisogna tuttavia notare che a 950 MeV la statistica è ancora povera ed è sperabile che, proseguendo l'esperienza di Waloscheck e coll., l'accordo migliori" (Selleri 1958, pag. 67).

"[...] all'energia di 810 MeV quasi tutte le interazioni $\pi^+ + p$ debbono essere elastiche. Questa è una seconda possibile verifica del modello isobarico" (Selleri 1958, pag. 70).

He also clearly explained known theories and arguments with independence of thought and autonomy of calculation.

Obviously it would be impossible to explain in detail every physical concept and moreover it would also go beyond the scope of this thesis. The results have been reported to show the validity of Selleri's work as early as the time of his thesis production.

From here on I am reporting the meaning of Selleri's work, without direct quotations but remembering that the source is always his thesis.

Selleri recalled that the components of total isotopic spin are given by three first integrals and that, furthermore, it is thought that the isotopic spin conservation theorem also derives from an analogous theorem in the interaction between a nucleon and the mesonic field. Nucleons interact through a field whose meson is π , so also the meson states are three and π must exist in three states of charge. In this way the existence of the discharge pion was predicted many years before its discovery.

Regarding the low energy π - N interaction, when the kinetic energy of the incoming particle in the laboratory is less than 300 MeV, Selleri stated that all information on low-energy meson-nucleon scattering is contained in the phase shifts and that experimental and theoretical reasons of various kinds lead to the conclusion that the Chew and Low theory foresees a resonance in the state $T=J=3/2$ for the pion-nucleon scattering, that is, the relative phase shift reaches and exceeds 90° .

Selleri also underscored that this key conclusion had so many confirmations that it should no longer be discussed. The analysis of high-energy experiences could be based on it. The equation for meson-nucleon scattering is also the basis for the non-relativistic treatment of this problem and it could be demonstrated that it included in itself the condition of unity of the S matrix.

As for the high energy π - N interaction, Selleri pointed out that the phenomenon of resonance is present in nuclear reactions.

That is, neutrons which affect nuclei with low energies give rise to a cross-section showing sudden peaks and dips as the energy changes.

The interpretation given to resonances is that the incident neutron is captured by the nucleus with which it forms a compound system. But such existence occurs only for a certain period, after which it decays by re-emitting a neutron. In analogy with the case of nuclei, one is led to speculate on the possible existence of a pion-nucleon compound system. But it makes no sense to talk about a pion inside the nucleon, as for neutron inside the nucleus: it is only the superposition of the wave functions of the two particles. Selleri exposed how this compound system could henceforth be called isobar and how it could be thought of as a real particle having mass $M=m+m_N+Q$ (that is, the sum of the pion mass, the nucleon mass and the variable excitation energy respectively); spin $J=3/2$, isospin $T=3/2$, charge $q = +2, +1, 0, -1$ and average life of 10^{-23} sec. The latter being calculated with the uncertainty principle starting from the width of the resonance in the cross section $\pi^+ + p$.

For the analysis of high-energy pion production ($\pi + N \rightarrow \pi + \pi' + N$), Selleri proposed a very useful schematization, the so-called isobaric model. It is based on the following assumptions:

- The pion-nucleon interaction occurs through the excitation of nucleons in the isobaric state $T=J=3/2$.
- The decay of the isobar is not affected by the recoil pion. That is, the isobar decays after a sufficiently long time to have separated from the extra-pion
- The probability of forming an isobar with the aforesaid mass M is proportional to the total cross section $\pi^+ + p$ taken at the energy Q and the density of the final states.

The isobaric model was initially applied to the nucleon-nucleon interaction and qualitatively explained the known phenomena regarding the scattering pion-nucleon and nucleon-nucleon. There was an excellent qualitative agreement with the experimental data.

But, at the end of this section, Selleri showed that a failure to verify that the creation of the isobar was the only fact responsible for the pion-nucleon interactions did not necessarily mean that the model was wrong but could simply mean that there were other ways for the pion-nucleon interaction and that it was necessary to obtain more stringent confirmation of the agreement between the predictions of the isobaric model and the experimental outcome. Comparison with experimental data showed a good agreement at 809 MeV, while the agreement was not as good at 950 MeV. But in this last scenario the statistical data were still poor and Selleri hoped that, continuing the experience, the agreement would improve.

At the energy of 809 MeV almost all interactions $\pi^+ + p$ must be elastic. This was for him a second possible verification of the isobaric model.

Eventually, concerning the pion-pion interaction, Selleri emphasized that the total cross section for negative pion-proton scattering shows a second maximum at 900 MeV, while for positive pion there is no maximum at all. This maximum is due to several angular moment states, all with $T=1/2$.

The isobaric model explained the existence of this maximum but only qualitatively. Another attempt came from Piccioni, Dyson, and Takeda, and was based on the assumption of a strong interaction between the incident pion and a pion of the nucleon cloud. At high energy also the pion-pion interaction must contribute to the total cross section for pion scattering on nucleons.

The experience is against Dyson's model which assumes that the pion-pion interaction takes place in the state $T=0$ of the pion-pion system, in order to have an effect in the state $T=1/2$ and no effect in $T=3/2$. And it is also pronounced against Takeda's model that a nucleon is given by a core and a pion cloud with negligible probability of having more than one pion. Moreover there is interaction between the incident pion and the virtual pion in the cloud, with resonance at $T=1$. Finally, the incident pion does not interact much with the core.

Even if experience says otherwise, that does not mean that pion-pion interaction is not important. It could just be a simplistic representation because a strong interaction between the nucleon and the two pions after the collision could hide the pion-pion interaction, or that one of the two pions is absorbed by the nucleon with the formation of an isobar.

In Selleri's view, in the Chew-Low's theory the two pions after the collision were in a barycentric system. Their method for the analysis of pion-nucleon scattering allowed to obtain the total cross section for the pion-pion interaction. But this procedure had not been performed for the small number of events available.

Finally, in addition to the fundamental subjects that will be developed later by Selleri, which will lead to the peripheral model and the OPE, from these excerpts it is possible to find some issue that will be the topic of Selleri's subsequent articles. For instance, the unity of the S matrix and the elastic and inelastic interactions. Selleri's degree thesis proved to be a good preparatory work for his later work in PP, also showing his skills.

1.3-THE FIRST JOB OFFERS

During the years of the so-called "miracolo economico", or "economic boom", a period in which Italy experienced an accelerated economic growth, many factories used to request the names of very recent university graduates, giving them an opportunity to start working very early.

Accordingly, soon after his degree in 1958, Selleri received several letters by a number of different firms, all asking for his immediate employment, even though he did not choose any of them, being completely focused and interested in his research in the field of physics. Actually, he decided⁹⁴ to save only two of them in his archive, which are reported below. They were all sent to Selleri's address at that time, i.e., Via Vittorio Veneto 24 in Bologna.

⁹⁴ Once again, I remind here that, fully aware of his role as national and international researcher, Selleri has made an unexplainable selection of his documents.

The first letter came from ENI⁹⁵, possibly the biggest Italian oil company, born in 1953:

The University Institute, from which you recently graduated, has sent us, on our request, a list of young graduates in which your name appears. Should it be of your interest, we could consider your profile with a view to a possible employment by this Institution or by one of the companies of the Group⁹⁶.

The second one came from Olivetti Spa, a very outstanding Italian company founded by Adriano Olivetti⁹⁷:

Dear Doctor,

we have taken your name from the lists of the University where you obtained your degree. We are contacting to propose you a program of placement for new graduates in our company, which could both be of your interest and concern. And by referring to the type of degree you obtained, we are able to explain to you the objective data of the program.

The tasks concern: mathematical problems applied to electronic laboratory studies. Studies and research in the electronic field (only for graduates in Physics). Technical-commercial services of programming on projects for the mechanization of accounting by means of perforated card systems or electronic calculators.

The main aim of our search is to bring university level youths into these specialized fields.

[...] We therefore propose a meeting that could take place at our offices in Milan for an exchange of information, without mutual commitment. We will then have the opportunity to learn about your professional orientations and aspirations, and in turn we will be able to explain in detail the career opportunities reserved in our organization for graduate staff.

⁹⁵ ENI is an acronym for Ente Nazionale Idrocarburi (National Hydrocarbons Agency in English). See also <https://www.eni.com/it-IT/home.html>. Accessed 24 September 2020.

⁹⁶ “L’Istituto Universitario, presso cui la S.V. ha di recente conseguito la laurea, ci ha trasmesso, su nostra richiesta, un elenco di giovani laureati nel quale figura il Suo nominativo. Ove fosse di Suo gradimento, potremmo esaminare la di Lei posizione ai fini di una eventuale, futura utilizzazione alle dipendenze di questo Ente o di una delle Società del Gruppo” (ENI Recruitment service to Selleri, 1959, unreadable month and day-FSPA).

⁹⁷ An enlightened entrepreneur who, together with his staff, had amazing industrial ideas and worked so hard at the forefront of the working environment in the company.

See also http://www.fondazioneadrianolivetti.it/lafondazione.php?id_lafondazione=1. Accessed 24 September 2020.

[...] This invitation is also confirmed in the event that you still have to do the Military Service because it seems to us that, in any case, a preliminary contact could be useful or mutually interesting.

[...] Please consider your travel costs at our expense.

We are pleased to send you our best regards⁹⁸.

(Signed by Furio Colombo⁹⁹-Ing. Olivetti & C. S.p.A-Secretariat for the selection of graduate staff).

Making reference to (Treccani 2013), it should be that Olivetti Spa and ENI, the letters of which are mentioned above, were two among the greatest Italian companies at that time, with an international scope:

One of the most innovative private Italian companies was Olivetti, which showed a strong focus on social sciences and industrial organization models developed in the United States. Founded as a small workshop in the early twentieth century, it specialized first in the production of typewriters and then, since the 1940s, of calculators. In 1948 it produced Divisumma 14, the first writing calculator in the world capable of performing the four operations. Despite tough competition from Japanese and American electronic giants, in 1965 he presented the world's first commercial electronic calculator, the P 101, in New York, which was a resounding success¹⁰⁰ (Treccani 2013).

It could be argued that Adriano Olivetti and his staff would have certainly have constituted a valid alternative, and an excellent competitor, to the US computers companies. His untimely demise and the subsequent weakening and dismantling of his company, combined with the short-sighted intervention of a certain policy were one of the many Italian defeats.

The story linked to ENI and its founder Enrico Mattei is also emblematic, in my view, of the watershed that brought Italy to the secondary role it still currently retains on the international chessboard:

⁹⁸ The original Italian text is in Adendum 3, p. 950.

⁹⁹ Furio Colombo (1931-) is an Italian journalist, writer, politician and academic. See https://it.wikipedia.org/wiki/Furio_Colombo. Accessed 24 September 2020. He has just written a book, along with Maria Pace Ottieri concerning Adriano Olivetti: "Il tempo di Adriano Olivetti", Edizioni di Comunità, 2019.

¹⁰⁰ The original Italian text is in Adendum 3, p. 951.

Very original was also the story of ENI, created in 1953 by Mattei, who in the summer of 1945 had received the task of winding up AGIP (Azienda Generale Italiana Petroli), a creation of Fascism. Mattei opposed this liquidation, however, which [instead] was seen with particular favor by the so-called seven sisters, the major American and British oil companies, interested in establishing their distribution networks in Italy. [...] Mattei relaunched AGIP, not only by opening new petrol stations, but also by opening a chain of motels and the first service stations. Subsequently, he promoted a drilling campaign in the Po Valley, which led to the discovery of methane deposits and within a few years made ENI a holding company at the head of a varied industrial empire.

A skilled entrepreneur, but also a passionate patron, Mattei knew how to surround himself with intellectuals and journalists [...].

Accustomed to moving with unscrupulousness and with few qualms, he made contracts with Iran, Egypt and even the Soviet Union, but on the evening of 27 October 1962 the small plane on which he was travelling disintegrated in the sky of Pavia. In 1997 the Pavia Public Prosecutor's Office reopened the investigation into this accident, and in 2005 the analysis of one of its experts, Professor Donato Firrao of the Politecnico di Torino, showed that a bomb explosion had occurred on the plane.

[...] With Mattei's departure from the scene, the era of those independent entrepreneurs who were the architects of reconstruction and pioneers of economic development [in Italy] came to an end (Treccani 2013)¹⁰¹.

These last lines confirm what I said before on some of the potential underlying reasons about Italy's low weight in the international context.

1.4-A GLANCE ON SELLERI'S RESEARCH:1959-1961.

PERIPHERAL MODEL AND OPE MODEL

There are at least forty papers concerning Franco Selleri's activity in PP ranging from Peripheral Model and One Pion Exchange (with an extension to many pions exchange) to scattering off-shell, and continuing with unitarity, anelastic and elastic processes, close singularity models, nuclear reactions theory, reformulation of the theory of the $\pi+N \rightarrow P+N$ process and eventually on nonconservation of angular momentum and the Regge poles.

I highlighted the basic works of Selleri in the field of PP, along with the achieved outcome, showing in parallel the different issues at stake.

¹⁰¹ The Italian original text is in Addendum 3, p. 951.

In 1959, Selleri joined the INFN (Istituto Nazionale di Fisica Nucleare) as a fellow, but, thanks to a basic contact with Pietro Bassi¹⁰² (1922-1984), Daniele Amati¹⁰³ (1931-) and Sergio Fubini¹⁰⁴ (1928-2005), he could go straight to CERN, at first for a simple meeting, then for two months and eventually for two years (from 1 December 1959 to 30 November 1961), thanks to a grant. This was surely an essential and exciting period in his career.

In order to take a glimpse on Selleri's arrival at CERN and the importance of his time spent in Geneva, it is worthwhile to take a look at his original, unpublished typewritten Curriculum¹⁰⁵ I found in his Archive, where Selleri explains the period immediately following his graduation. It was prepared in 1961 for an exam in which Puppi was the committee chairman. At the beginning he pointed out a nice portray of the lessons he received by Puppi as well as his willingness to be a theoretical physicist:

I took my degree in physics in Bologna in December 1958, with a thesis on the application of the isobaric model in inelastic pion-nucleon interactions. Proposer and supervisor of this thesis was G. Puppi who taught me something very important: that one can understand physics even without a great deal of mathematics; that "making physics" means taking experimental data and trying to understand, through their characteristics, what happened at the time of interaction; and that only when you believe you have understood this, it is worthwhile to start making calculations. I will not linger on the merits of my thesis supervisor because he is the committee chairman for this examination.

I spent the following year in the vain attempt to educate myself in the theoretical field and in passive resistance to the suggestions of working in an experimental group. The success of such resistance was decreed by Pietro Bassi, who took me with him to Geneva for a few days in order to get me to discuss with Sergio Fubini on some dark issues of the isobaric model. The fact that the desired discussions did not practically take place (thanks to Fubini's well-known ability to avoid talking about what he does not care about) does not change the decisive importance that this trip had for me.

¹⁰² See https://archivistorico.unibo.it/System/76/32/bassi_pietro.pdf. Accessed 24 September 2020.

¹⁰³ See https://en.wikipedia.org/wiki/Daniele_Amati. Accessed 24 September 2020.

¹⁰⁴ A deep portrait of the great role played by Fubini is visible in the attached PDF titled "Commemorazione" by Vittorio de Alfaro e Giuseppe Furlan (Atti ufficiali, 2004-2006, pag. 103-121) present at: <https://www.accademiadelle scienze.it/accademia/soci/sergio-fubini>. Accessed 24 September 2020.

¹⁰⁵ Cited in the following as (Selleri unpublished document, 1961-FSPA).

It was Fubini himself who suggested that I go to Geneva (subsidised by Italy) for a couple of months to work at CERN.

So I began to work on a numerical application of dispersion relations, suggested by D. Amati, which should have led to the determination of the meson [...]. [They were] very advantageous because of their insensitivity to the contributions of the non-physical region and their independence from the sign of transition amplitudes, but they turned out to be highly dependent on the low-energy K+P cross section [...] where the experimental data was missing. Before this work was finished there was a providential economic crisis of the INFN which resulted in my call to base. At this point, Amati and Fubini suggested that I apply for a fellowship at the theoretical division of CERN. That is why I called the INFN crisis providential. I am sure that my two-years stay (1959-1961) at CERN has been very formative for me, in the sense that I have learned much more than I would have in another less lively environment. So I am very grateful to Amati and Fubini for giving me the opportunity to stay at CERN (Selleri unpublished document, 1961, pagg. 1-2-FSPA. My underline)¹⁰⁶.

During his stay at CERN, Selleri formulated, with Francesco Bonsignori¹⁰⁷, the idea of Peripheral Model. They explained its meaning in the pivotal joint paper “Pion Cloud Effects in Pion Production Experiments”¹⁰⁸:

A simple model is proposed for the study of single pion production in collisions of a generic particle “a” on a target nucleon. The model consists essentially in neglecting all interactions but the one between the incoming particle and a single pion of the nucleon cloud.

[...] an experiment is proposed which could give more detailed information on pion-pion interaction (Bonsignori, Selleri 1960, pag. 465. My underline).

Somehow broadening Selleri’s degree thesis with some reference to the previous outcomes, they illustrated the coherence of the Peripheral Model with several experiments:

It has [...] been remarked that from pion production experiments it should be possible to obtain some information regarding the interaction of the incoming particle with the virtual pions and methods have been proposed to obtain the cross-section of this

¹⁰⁶ The Italian original text is in Addendum 3, pp. 951-952.

¹⁰⁷ One of the five physicist (jointly with Wolfgang Alles, Silvio Bergia, Dino Boccaletti, and Franco Selleri) who graduated in Bologna in the same day with the same mark.

¹⁰⁸ Bonsignori F., Selleri F. (1960), “Pion Cloud Effects in Pion Production Experiments”, *Il Nuovo Cimento*, 15 (3), pp. 465-478. (Bonsignori, Selleri 1960) in the following.

interaction. Yet practical difficulties, depending also from the great statistic needed have, until now, made impossible the application of these methods.

[...] We want to examine [...] two recent experiments of pion production in proton-proton and pion-proton. The first one, by Batson, Culwik, Hill and Riddiford, regards pion production in proton-proton collisions: $p + p \rightarrow p + n + \pi^+$. The second one, by Alles-Borelli, Bergia, Perez-Ferreira and Waloscheck, regards pion production in negative pion-proton collisions: $\pi^- + p \rightarrow \pi^- + \pi^+ + n$ and $\pi^- + p \rightarrow \pi^- + \pi^0 + p$.

[...] We will use a model consisting essentially in neglecting both direct interaction and more of one pion exchanges between the incoming particle and the nucleon. This model will be presented [...] together with the experimental and theoretical justifications that can be given.

[...] It will applied to the quoted experiments [...] [in order to obtain] some information on proton-pion and pion-pion interactions. We will see that the obtained pion-proton cross-section agrees reasonably well with the experimental one, and this fact will give further support to the method.

[...] The interaction responsible for the absorption of the incoming particle from the elastic channel takes place also with the pion cloud of the target nucleon; this conclusion [...] suggests the existence of pion-pion interaction in the case of pion-nucleon scattering.

On the other hand the mean number of pions in the nucleon cloud [...] turns out to be about one.

[...] All this suggests the interaction of the incoming particle with more than one virtual pion to be an unlikely event. In the following we will make the explicit assumption that the only important contribution to the interaction between the incoming particle and the target nucleon responsible for single pion production comes from diagrams with only virtual intermediate pion (Bonsignori, Selleri 1960, pagg. 465-467. My underline).

[...] We will analyse the experimental data with this model in mind: the incoming pion hits on a single pion of the cloud and makes it real. After this interaction has taken place the two pions are yet in the volume of interaction and have a certain probability of interacting with the nucleon (Bonsignori, Selleri 1960, pagg. 473-474. My underline).

[...] Finally [...] we want to propose an experiment from which more detailed information about $\pi\pi$ interaction should be obtained. It consists of a simple one pion production experiment in pion-nucleon collision, but at an energy sufficiently high as to have a reasonable number of events with Q values of both pions with respect to the nucleon ranging in the interval (300-500) MeV (Bonsignori, Selleri 1960, pag. 477).

[...] in any case the experiment should be a test of the proposed idea that the only important elementary interaction in single pion production comes from diagrams with pion-pion interaction vertices (Bonsignori, Selleri 1960, pag. 478).

The previous excerpts revealed a deep and careful attention to the experiments necessary to confirm the model. After explaining the model, some interesting outcomes have been shown, like, for instance, the major contribution of only a virtual intermediate pion in the single pion production. It is interesting to see that Selleri studied the single pion production starting with the isobar model he used in his degree thesis.

Bergia significantly pointed out the depth of the authors' reflections, oriented towards a wider search for a theory, not only a model:

[...] the model dealt only with the aspect of the final state interactions, leaving completely unexplored that of the interaction responsible for the production. As he and his coauthor Francesco Bonsignori wrote in their paper, “the pronounced diffraction-like forward peak” showed by high-energy proton-proton and pion-proton elastic scattering gave for the nucleon radius “a value [...] very similar to the Compton wavelength of the pion”. This strongly suggested “that the interaction responsible for the absorption of the incoming particle took place [...] with the pion cloud of the target nucleon”. As one sees, these reflections were much along the lines of the kind of phenomenological analysis that the field seemed to require prior to any attempt at formulating a specific theory or model. Still along these lines was the next step, concerning the question “how many pions of the cloud one should take into account”. Phenomenological and near-theoretical studies strongly suggested that this number should be close to one (Bergia 2004, pag. 1769).

At this stage the authors formulated what they called the basic assumption of their paper: “In the following we make the explicit assumption that the only important contribution to the interaction between the incoming particle and the target nucleon responsible for single pion production comes from diagrams with only one virtual intermediate pion”.

Even limiting oneself to that single contribution, however, as the authors stressed, one should have to evaluate “all possible vertex corrections and all self-energy processes of the intermediate pion”, and no specific field theoretical treatment of the pion nucleon and pion pion interactions existed allowing that (Bergia 2004, pagg. 1769-1770. My underline).

[...] What I shall now try to do is to report on the comparison with the experiments outlined in the paper and to follow up the development of Franco's investigations on the subject. A first comparison was made with data on pion production in proton-proton collisions at 970 MeV. A potentially meaningful spectrum promised to be that of the laboratory kinetic energy of the recoil nucleon, which, according to the model, had to present a sharp peak at about 60 MeV. The data showed in fact a marked deviation from a purely statistical behavior due to a maximum in the predicted position.

Since what [Bonsignori and Selleri] had at hand was a theory, not a model with free parameters, it implied defined (different) values for the height of the maxima in the cases the lab. kinetic energy plotted was that of the proton or of the neutron, and these values did not come out correctly. The reason was neatly spotted by the authors: there were in fact two diagrams contributing to the spectra.

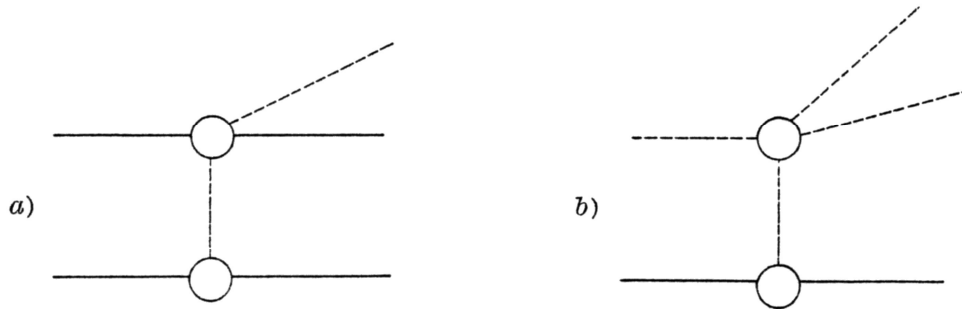


Fig. 1.

In their words: “the first one corresponding to the emission of a π^0 from the ‘lower’ proton that propagates and interacts with charge-exchange with the ‘upper’ proton, the second one to a π^+ emitted and interacting ‘elastically’ with the ‘upper’ proton. Thus, roughly speaking, the observed neutrons come sometimes from the first diagram where they are something like decay neutrons of an isobar 33, sometimes from the second where they are recoil neutrons. Therefore their spectrum must be given by the superposition, with interference, of the spectra predicted by the two diagrams for the recoil and decay neutrons respectively. Similar reasoning holds obviously also for the proton” (Bergia 2004, pagg. 1771-1772. My underline).

The paper by Bonsignori and Selleri received a good acceptance, as Bergia (who referred to their article as BS) properly stated:

The BS paper received very early citations by authors active in the field. I. Derado analysed experimental data collected with the Alvarez hydrogen bubble chamber exposed at a beam of 1.14 GeV/c negative pions at the Berkeley Bevatron according to the BS indications and got results similar to theirs. Single pion production in pion nucleon collisions at 960 MeV was also analysed, using further bubble chamber photographs from the same irradiation at the Cosmotron as that used by our group, by Pickup, Ayer and Salant. The authors quoted, of course, our results, but also the BS analysis. They gave further evidence for the single pion exchange mechanism (Bergia 2004, pag. 1772).

[...] preliminary results [connected to BS conclusions] were opening a new interesting chapter in the realm of strong interaction physics. It seems fair to say that BS has played a relevant role in the initial stage (Bergia 2004, pag. 1774. My underline).

So, the peripheral interaction (as Selleri sometimes called the peripheral model) experienced an instant hit. Actually,

the peripheral model, confirmed by the S-matrix theory of strong interactions by Geoffrey Chew, also received significant experimental support, and can be considered the progenitor of the Drell–Yan process in that it concerns the exchange of a virtual particle in strong interactions. This model was always extended to proton–proton interactions during Franco’s stay at CERN (1959–1961), thanks to the contribution of Ezio Ferrari and the suggestions of Sergio Fubini (Barone-CERN Courier 2013).

Moreover, Selleri also proposed the One Pion Exchange (OPE) model for the first time as a ruling process in the high energy inelastic processes. That is, the suggestion that in an inelastic process it is possible to change a pion. In this view, during a collision between two protons, the target proton emits a virtual positive pion, becoming a neutron; the incident proton scatters elastically on this virtual pion, making it as a real pion in the final state. Selleri recalled the birth of his idea. As he vividly stated in an excerpt taken from the already widely quoted Freire’s interview:

[...] [at CERN] I had what I consider a very good idea. That was the one pion exchange model; that is to say, the idea that in some inelastic processes really what is going on is that one pion is exchanged. For example, if you have two protons colliding and producing one pion, let us say a positive pion, you have in the final state a proton, a neutron and a positive pion. The idea was that the target proton (for example) emitted a virtual positive pion, becoming a neutron, and that the incoming proton scattered elastically on this virtual pion producing it as real in the final state. So this one-pion-exchange model was really my idea, my first idea, and it was immediately a remarkable success in fitting the experimental data. I became known in the whole of particle physics (AIP, 2003).

Looking at the interplay between the two models, Bergia claimed that,

It seems to me that it became proper to speak of a “peripheral model” when the data seemed to suggest that “the OPE (one-pion exchange) contribution was probably dominant also for high momentum transfer” and dominance of OPE was “assumed throughout the whole physical region and not only for very small momentum transfers as has been done in recent work” (Bergia 2004, pag. 1774).

The importance of this and the subsequent works on these two models mentioned above by Selleri is well highlighted by an evidence expressed to the present author by the Italian physicist Erasmo Recami¹⁰⁹. During a SISFA¹¹⁰ conference held in Messina in September 2018, after my seminar on Selleri's activity in PP, Recami approached me reminding the fundamental value for him and several scholars of the foregoing article and of those immediately following, surely some key papers for his training as well as for many physicists of his generation.

1.5- AN ADDITIONAL LOOK AT SELLERI'S RESEARCH: SOME PAPERS IN PARTICLE PHYSICS.

For a better comprehension of the issues at stake concerning Selleri's research in PP, I am going to mention in the following paragraph, some abstracts and descriptions taken by his papers of those years, subdivided according to the phase. I will start with the CERN period:

In 1960, Selleri wrote "K-Meson Parity from Dispersion Relations"¹¹¹, (Selleri 1960a), released only a month after its reception on *Il Nuovo Cimento*. In it Selleri described how many scholars had tried to study the K-parity problem using the dispersion relations without getting to any conclusions at that time. By using an approach proposed by D. Amati, Selleri concluded that

The present evidence for a weak energy dependence of σ^+ at low energy is consistent only with a pseudo-scalar *K* meson. [...] To ensure our conclusion [...] it would be extremely interesting to have a direct experimental determination of σ^+ in the very low energy region (Selleri 1960, pag. 990).

I decided to mention this paper mainly because kaons have proved to be important for several studies.

¹⁰⁹ Erasmo Recami (1939-): See https://it.wikipedia.org/wiki/Erasmo_Recami. Accessed 24 September 2020.

¹¹⁰ SISFA is the acronym for Società Italiana degli Storici della Fisica e dell'Astronomia, a society which was born on 1999. See also <http://www.sisfa.org/>. Accessed 24 September 2020.

¹¹¹ Selleri F. (1960), "K-Meson Parity from Dispersion Relations", *Il Nuovo Cimento*, 15, pp. 986-990.

Selleri will use them in detail starting the 1983 and during the 1990s as well as the early 21st century in order to differentiate between Quantum Mechanics and Local Realism (LR).

In the following papers at CERN, Selleri worked out the $PP-PN\pi^+$ process with interference terms and form factor to describe the virtuality of the exchanged pion as well as the presence of two pion-pion resonances.

The paper “Role of the Peripheral Interaction in Proton-Proton Collisions”¹¹² (Selleri 1961), issued on *Physical Review Letters*, provided key evidence in favour of the peripheral collisions, always with the support of experimental results and with Selleri’s precise growing desire to extend the study of the phenomenon:

It has been suggested in a recent paper by Bonsignori and the writer that the inelastic scattering of any particle on a target nucleon may be dominated by the exchange of one single virtual pion. Analyzing some known experiments, it was there shown that in the BeV region there is good evidence for such a mechanism, when the incoming particle is a π meson or a nucleon. Further experiments have shown the same qualitative evidence which consists essentially in a low-energy peaking of the laboratory spectrum of the recoil nucleon. It seemed therefore desirable to go further and calculate the cross section for the process $p + p \rightarrow p + n + \pi^+$ (1) under the assumption of dominating peripheral interaction. We wish to stress that this dominance is assumed throughout the whole physical region and not only for very small momentum transfers as has been done in recent work. The reason for choosing reaction (1) is that final-state interactions between the nucleons are negligible at high energy, while the (3,3) interaction of the pion is likely to involve only one of the nucleons and thus not to destroy the typical effects of the one-pion exchange (Selleri 1961, pag. 64).

[...] It seems very desirable to have a deeper comparison between this model and the experimental data (Selleri 1961, pag. 66).

Speaking about the paper above, and also about Selleri’s desired extension and his burgeoning result, Bergia pointed out that:

¹¹² Selleri F. (1961), “Role of the Peripheral Interaction in Proton-Proton Collisions”, *Physical Review Letters*, 6 (2), pp. 64-66.

[In this paper] an important step forward was achieved [by] Franco.

[...] He took up again the issue concerning production in proton-proton collisions dealt with in the BS paper and found the way to minimize the disturbing effect due to contribution to the spectrum of nucleons arising from decay of the 33 resonance. The recipe was simply to limit oneself to considering the reaction $p + p \rightarrow p + n + \pi^+$.

The reason for that was that the (3,3) interaction of the pion was likely to involve only one of the nucleons, so that it would not obscure the typical effect of the one-pion exchange. Together with the diagram (Fig. 2), in which the exchanged charged pion gave rise to a (3,3) interaction with the incoming proton p_1 ,

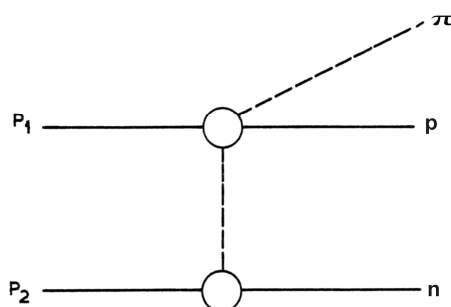


Fig. 2

one ought to consider the other three diagrams obtained by exchanging the initial protons and the final nucleons. But Franco quickly realized, on the basis of isospin arguments, that the separate contribution to the total cross section for the above process of the two diagrams in which the pion and the neutron emerge from the same vertex is one ninth that of the diagram above. I remember that Antonio Stanghellini had put Virginio Bortolani and myself on the same problem. But Franco's clever simplifying shortcut played us out: his paper was accepted for publication while we were still busy computing all possible contributions! Agreement with the experimental spectrum was very good [...].

As in the previous paper, and for the same reason, the total cross section did not come out correctly, implying that something more than the diagram of figure 2 was contributing to the process. However that "something more" did not exceed a 30% of the "peripheral" pion-pion contribution.

[...] The relevant case was of course that of the pion-nucleon interaction. The data already examined gave in themselves a qualitative evidence of a pion-pion interaction, but one could aim at some quantitative result (Bergia 2004, pag. 1773).

As emphasized by Bergia (in the underlined statements), Selleri showed strong skills in solving many complex problems.

Another step in Selleri's activity on PP is given by the remarkable and brilliant collaboration, started at CERN and still continued during Selleri's

stay at Bologna and Saclay, with Ezio Ferrari¹¹³, which gave birth to several papers, some of which are recalled here. Concerning the partnership with Ferrari, Selleri vividly and a bit self-ironically stated that:

The collaboration was advantageous because of the complementarity of our characters: on the one hand he has the advantage over me of being much more exact and tidy; on the other hand I have the disadvantage compared to him to have much less desire to work!¹¹⁴ (Selleri unpublished document, 1961, pag 10-FSPA).

The first joint paper by the foregoing collaboration was “Quasi-Elastic Scattering of High-Energy Protons in the One-Pion-Exchange Model”¹¹⁵, presented in a Conference of High Energy Physics held in Geneva, (Ferrari, Selleri 1961a) in which they made further assumptions in order to “calculate accurately the complete OPE contributions by neglecting only the “form-factor” corrections.

[In the process $p + p \rightarrow p + n + \pi^+ (1)$] the theoretical curve has been calculated by G. Da Prato by assuming that the interaction proceeds via the OPE contribution. The further approximations made in this calculation are the following:

- The assumption that the πN vertex and the π propagator can be well represented by their lowest order perturbative expressions.
- The assumption that the partial wave amplitudes for off-shell pion-nucleon scattering do not depend on the squared four-momentum of the virtual pion and are thus equal to their on-shell (experimentally known) value.

[...] not only the existence, but also the position and absolute value of the peak are correctly predicted, thus leaving little doubt about its origin. With increasing energy, where the [two above] approximations can no longer be trusted, the fit becomes worse in normalization but still reproduces the general behaviour of the experimental spectrum. This suggests the OPE can be more important than expected for reaction (1) (Ferrari, Selleri 1961a, pag. 297).

¹¹³ The details on Ezio Ferrari personal archive will be soon at <https://sapienzadipfisica.archiui.it/oggetti/5782-patrimonio-archivistico-del-dipartimento-di-fisica-di-sapienza-universita-di-roma/>. Accessed 24 September 2020.

¹¹⁴ “La collaborazione si presentava vantaggiosa per via della complementarità dei nostri caratteri: da una parte lui ha su di me il vantaggio di essere molto più esatto e ordinato; mentre dall'altra io ho rispetto a lui lo svantaggio di avere molto meno voglia di lavorare!” (Selleri unpublished document, 1961, pag 10-FSPA).

¹¹⁵ Ferrari E., Selleri F. (1961), “Quasi-Elastic Scattering of High-Energy Protons in the One-Pion-Exchange Model”, in *Geneva Conference of High Energy Physics*, CERN report 2010/th, pp. 297-305.

Being Δ^2 the squared 4-momentum of the exchanged pion and μ^2 the pion mass, then the two opportunities they found are, in case of higher Δ^2 ,

1) Only one pion is actually exchanged between the colliding protons also for rather high values of Δ^2 (say $\Delta^2 \approx 30 \mu^2$).

2) Soon after the region $\Delta^2 \approx \mu^2$ the many-pions exchanges are important.

[...] Therefore we arrive at the conclusion that the two possibilities [...] are, in principle experimentally distinguishable. The importance of this point is immediately clear. Should possibility 1) be verified in a certain Δ^2 region we could conclude that the OPE is dominant in this region [...]. (Ferrari, Selleri 1961a, pag. 304).

Another interesting joint paper is “Pionic Form Factor Effects in Peripheral Nucleon-Nucleon Collisions”¹¹⁶, (Ferrari, Selleri 1961b) appeared on *Physical Review Letters*. These represented further steps in the analysis of nucleon-nucleon collisions as well as the OPE contribution.

There is increasing evidence that in inelastic nucleon-nucleon collisions the one-pion exchange (OPE) contribution plays an important role. This evidence rests:

- At a few (~ 1 -3) Gev on the experimental lab energy distributions, and c.m. angular distributions;
- At many (≥ 10) Gev on the analysis of quasi-elastic scattering data, of general features of the interactions of emulsion experiments, and of cosmic-ray interactions.

A further step in the analysis of such reactions consists in understanding the quantitative importance of the direct many-particle exchanges, which are expected to be increasingly important with increasing momentum transfer. The effect of these exchanges cannot, however, be revealed by comparing the OPE calculations performed till now with experiments, because such calculations are correct only for small values of the nucleon momentum transfer.

A method for understanding the limitations of the OPE approximation has been suggested by the authors. Since the matrix element for off-shell pion-nucleon scattering in the region of the 3,3 resonance has been calculated, one can evaluate a peripheral graph for single (and possibly double) pion production in nucleon-nucleon collisions, by taking full account of the virtuality of the exchanged pion. This calculation is possible for energies of the πN scattering not too far from the 3,3 resonance, and is undetermined only by an unknown multiplicative function of the squared four-momentum of the virtual pion.

¹¹⁶ Ferrari E., Selleri F. (1961), “Pionic Form Factor Effects in Peripheral Nucleon-Nucleon Collisions”, *Physical Review Letters*, 7 (10), pp. 387-390.

This function is simply related to the pionic form factor of the nucleon. These facts make possible a check of the peripheral model and possibly allow the determination of the pionic form factor. If the experimental data at different energies were fitted with a unique choice of this function, then one might conclude that the OPE contribution is probably dominant also for high-momentum transfer, because the discrepancies generated from the many-particle exchanges are expected to depend also on the initial energy. In this Letter we show that the existing experimental data do indeed suggest full dominance of the OPE term and we obtain a tentative behaviour of the pionic form factor

[...] [Some facts] suggest an OPE contribution much more important than expected also at high Δ^2 . [...] The main purpose of this Letter is thus to make clear the importance of further experimental information of reactions [$p+p \rightarrow p+n+\pi^+$ and $p+p \rightarrow p+p+\pi^0$] (Ferrari, Selleri 1961b, pagg. 387-388).

A review paper, which is useful to shed light and clarity on the different contributions to the issue, introduced by other scholars before Selleri, as well as to summarize several Selleri's papers, is undeniably "Peripheral Model for Inelastic Processes"¹¹⁷ (Ferrari, Selleri 1962), edited on *Supplemento del Nuovo Cimento* and written on the way back from CERN to Bologna University. It is sixty-three pages long and the abstract is a real index in three chapter plus four Appendices:

- I. The importance of the peripheral mechanism of interaction in the description of physical processes
 - 1. Introduction
 - 2. Present situation in the peripheral description of physical phenomena
 - II. Mathematical formulation of the peripheral model. Case of three particles in the final state.
 - 1. S-matrix and cross-section
 - 2. Peripheral S-matrix element
 - 3. Invariants and kinematics
 - 4. Calculation of cross-sections
 - 5. Occurrence of different peripheral graphs for the same process
 - III. Many particles in the final state
 - 1. The most general peripheral diagram for many-particle production
 - 2. The particular case of one or two particles coming out from one of the vertices.
- Appendix I. Appendix II. Appendix III. Appendix IV (Ferrari, Selleri 1962, pag. 453).

¹¹⁷ Ferrari E., Selleri F. (1962), "Peripheral Model for Inelastic Processes", *Supplemento del Nuovo Cimento*, 24 (2), pp. 453-515.

In the introduction the authors presented a state-of-the-art research in the field of peripheral interaction, also reporting in Appendix IV a list of existing works on this topic. In addition, there is a further overview of peripheral interaction and OPE interaction, where the latter could sound as a more proper extension of the former:

In the past few months there have been indications that in high-energy phenomena peripheral interaction play an important role [between 1960 and 1961 there were several papers made by Friedländer; Dremin and Chernavskij; Derado; Da Prato; Fickinger, Pickup, Robinson, Salant; Barnes, Bugg, Dodd, Kinson, Riddiford; Nguyen Chi Hien]. By “peripheral”, one usually means that kind of interaction which is transmitted by the least massive system which can be exchanged between the colliding particles (for the most part of strong interactions this system consists of a single pion). We will extend the meaning of this word and call peripheral any interaction which is transmitted through the exchange of a single virtual particle (even not the lightest possible one). A more proper denomination of this kind of interaction should be “one-particle exchange interaction”, but the name “peripheral” will be used throughout for simplicity.

In this paper we want to discuss in detail the predictions obtained when inelastic processes are described in terms of a dominant peripheral interaction (Ferrari, Selleri 1962, pag. 453. My underline).

Then there is an overview describing the index (contained in the abstract) more in depth:

[...] The work will be divided in three Parts and four Appendices.

Part I will present a summary of the present status in the application of the peripheral approach to inelastic phenomena. Part II and III will deal with the explicit derivation of formulae for the energy spectra, angular distributions and total cross-sections predicted by the peripheral model. Part II deals with the particular, but important case of two particles transforming into three. Part III deals with the case of more than three particles in the final state. Appendices I, II and III contain some of the phase-space calculations. The last Appendix (Appendix IV) gives a list of the papers which contain numerical evaluations of the peripheral effects relevant to some experimentally observed reactions (Ferrari, Selleri 1962, pagg. 453-454).

Selleri and Ferrari explained the different contribution on the issue at stake, pointing out that:

Considerations of peripheral diagrams in inelastic processes have been first made by Chew and Low [in 1959]. [...] in Chew and Low's approach, the experimental knowledge of the inelastic process was assumed as a starting point to obtain further information, and no predictions on it were attempted. A different point of view was assumed, however, in the most part of the following peripheral calculations [by C. Goebel in 1958, Bonsignori, Selleri 1960 and S.D. Drell in 1960 and 1961] (Ferrari, Selleri 1962, pag. 455).

If the pole contained in the peripheral diagram is the nearest to the physical region and is not too far from it, it should be expected that in the part of the physical region near the pole the peripheral diagram is still dominant.

[...] the cases of interest for which the peripheral model is expected to work refer generally to the exchange of a single pion; in some cases, one can consider also the exchange of a single K-meson.

[...] Goebel showed that in the process of single pion production in pion-nucleon collisions the cross. Section should have a maximum for small momentum transfers to the target nucleon, if the π - π interaction is not negligibly small.

The observation of this maximum was further discussed by Bonsignori and Selleri who found the first direct evidence of π - π interaction confirmed by many following investigations. [...] it is also shown that there is a non-negligible peripheral contribution to the processes of pion production in nucleon-nucleon-collisions. On the basis of further work on this subject ["Role of the Peripheral Interaction in Proton-Proton Collisions", (Selleri 1961); "Pionic Form Factor Effects in Peripheral Nucleon-Nucleon Collisions", (Ferrari, Selleri 1961b)], it has been found that the one-pion exchange term plays a very important role in such processes, in particular in the energy region of a few GeV in the laboratory system. This point of view has been also extended further by tentatively calculating peripheral effects also far from the region of "quasi-real" intermediate particles. In this case the dominance of the pole term over the other contributions has no direct theoretical justifications, but has to be taken as an assumption. By this approach, it is possible to get complete predictions for the experimental energy and angle distributions of all final particles: these predictions can be calculated numerically, and compared with experiment (Ferrari, Selleri 1962, pagg. 456-457).

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[...] it is also possible to calculate a peripheral diagram in a more refined way by taking into account the corrections in the vertices induced by the virtuality of the intermediate particle. This point of view [...] is supposed to give a more realistic evaluation of the peripheral effects at higher momentum transfers. It has been discussed in detail by Ferrari and Selleri ["Off-Shell Pion-Nucleon Scattering and Dispersion Relations"] for any process in which the vertex (virtual π) + N $\rightarrow$ π + N enters.

The amplitude for such a vertex has been shown there to be given by a known function of the energy and the momentum transfer multiplied by the “pionic form factor” of the nucleon. Therefore, the calculation of a peripheral diagram containing this vertex is undetermined only by an unknown multiplicative function of the momentum transfer alone. This leaves open the possibility of an experimental check of the extension of the peripheral approach to the whole physical region. If the theory were found to disagree from experiment only for a momentum transfer (and not initial energy) dependent function, then one could reasonably infer that the peripheral effect is important also at large momentum transfers and try to determine the form factor from the experiment. By following this approach, a first tentative evaluation of the pionic form factors has been made in [Pionic Form Factor Effects in Peripheral Nucleon-Nucleon Collisions]. We point out that this procedure is not necessarily limited to the pionic form factor of the nucleon and could eventually be applied to other phenomena such as the production of strange particles (Ferrari, Selleri 1962, pag. 457).

Always in his 1961 Curriculum, Selleri summarized in the following way his first, bright and productive period¹¹⁸:

I started working with F. Bonsignori on a possible application of Chew and Low’s extrapolative method to the experimental results achieved in Bologna on reactions

$$p + p \rightarrow p + n + \pi^+ \quad (1) \quad \text{and} \quad \pi^- + p \rightarrow \pi^- + \pi^+ + n \quad (2).$$

This application of the extrapolative method had been suggested by A. Minguzzi but soon proved impossible with the limited statistics available. At this point we wondered whether it was not possible to find already in the physical region the effects of the non-physical pole to which we initially wanted to extrapolate.

[...] The basic point of Chew and Low’s analysis is precisely the fact that the diagram with only one exchanged pion, OPE, becomes infinitely large in one point while at the same point other contributions remain finite. Therefore, it is clear that there is a surrounding of that point where OPE is dominant. Unfortunately, it did not seem possible to determine the extent of this surround and thus to know whether it extends to the physical region or not. We therefore wondered what typical effects one should expect if OPE was actually dominant also in the physical region for small values of $T = -\mu^2/2m$. The first thing that came to mind was the following. The reaction (2) is dominated by the formation of the isobar (3,3) in the final state [...]. Whatever the origin of the isobar, it is certainly a different contribution from the OPE. Therefore, the OPE is not dominant in the whole set of energies for reaction (2).

¹¹⁸ All the original Italian texts of the excerpts reported in the following pages (75-79) and taken from (Selleri unpublished document, 1961-FSPA) are in Addendum 3, pp. 952-956.

[...] the differential cross-section predicted by the OPE in T has a maximum for low T and decreases rather quickly as T increases.

Thus the OPE predicts a higher number of events at low T values than with phase space predictions alone (statistical theory). Further experiments [Derado 1960; Rushbrooke and Radojicic 1960; Pickup, Ayer and Salant 1960; Pickup, Robinson and Salant 1961; Erwin, March, Walker and West 1961, all published on *Physical Review Letters*] showed this feature even more convincingly and proved that in processes (1) and (2), for energies around 1 GeV, the pion-pion interaction is of primary importance (Selleri unpublished document, 1961, pagg. 3-5-FSPA)

[...] In a subsequent letter to *Nuovo Cimento* ["K-Meson Parity from Dispersion Relations", (Selleri 1960a)] I again discussed in a qualitative way the inelastic $\pi n \rightarrow 2\pi n$ high energy interactions, advancing the following explanation. The primary interaction that originates the single production [in the two above-mentioned reactions] is always the interaction of the incident pion with a virtual pion of the hit nucleon cloud. As a next step the two outgoing pions can still interact with the nucleon. This interaction will now be of low energy since the outgoing pions are slower than the incoming one. [...] Such interaction in the final state destroys the typical effects of OPE. [...] The second [reaction] appears to be the best one to reveal the typical effects of OPE (Selleri unpublished document, 1961, pag. 5-FSPA).

[...] In the work [Bonsignori, Selleri 1960] I noticed that the experimental data of the reaction $p + p \rightarrow p + n + \pi^+$ (*) showed a characteristic concentration of low kinetic-energy neutrons in the laboratory, qualitative evidence of an important OPE contribution to the process. This reaction has [...] two very important advantages over the [above] reactions (1) and (2) with an initial pion:

- a) There are unlikely to be major interactions in the final state beyond those that are already automatically included in OPE.
- b) The πN interaction entering one of the vertexes is well known experimentally.

Therefore [...], I jump headfirst into the middle of the line to get the accuracy of Bonsignori-Selleri idea. I soon had to realize that the traditional method with four-dimensional spinors led to an extreme complication in the calculations even when only two of the four OPE-type diagrams were taken into account (as I did). Fortunately I discovered that the use of two-dimensional spinors greatly simplified the complexity of the calculations and this allowed me to get to the result.

[...] The agreement is good as a general trend but illusory as normalization, since the result obtained and published ["Role of the Peripheral Interaction in Proton-Proton Collisions" (Selleri 1961)] is wrong by a factor of two as I was pointed out first by A. Stanghellini in private discussions, then by G.C. Wick and F. Salsman by letter.

The origin of this error was basically in my poor understanding of the mathematical effects of Pauli's principle and secondly in the fact that the same wrong normalization is passed off as good in Bogoliubov and Shirkov's book [*Introduction to the theory of quantized fields*, New York: Interscience Publishers 1959].

Fortunately, before I realized the error, new data came from Yale regarding the higher energy reaction (*) and unequivocally showing the presence of the low-energy peak. The complete calculations [...] were made by G. Da Prato.

[...] The result was that correct normalization led to a surprisingly correct prediction of the system, position and absolute normalization of the low-energy peak in the neutron laboratory spectrum from the reaction (*).

[...] The situation was as follows:

- excellent agreement between the OPE model and experimental data at low transferred moments.
- Increasing discrepancy as the transferred moment increased.

[...] Since Da Prato's work had definitively clarified that the OPE plays an important role in the reaction (*), it was time to push the analysis further in order to understand the limitations of the model with only an exchanged pion and possibly reveal the contributions with more particles.

On this line of thought I began working with Ezio Ferrari who had arrived at CERN in October 1960. The collaboration was advantageous because of the complementarity of our characters: on the one hand he has the advantage over me of being much more exact and tidy; on the other hand I have the disadvantage compared to him of having much less desire to work! (Selleri unpublished document, 1961, pagg. 8-10-FSPA).

[...] The idea of determining the pyonic form factor of the nucleon using the method discussed above was presented [with Ferrari] at the CERN Conference on High Energy Phenomena ["Quasi-Elastic Scattering of High-Energy Protons in the One-Pion-Exchange Model"] (Selleri unpublished document, 1961, pagg. 14-FSPA).

In his wide and intense Curriculum, almost an everyday narrative, Selleri showed also several interests in various topics. It is useful to see the process of construction of a work as well as the possible quality of the results obtained:

- 1) Peripheral radiochemistry: The radiochemical group at CERN... [had] found [a year ago] a cross section much larger than expected for nuclear reactions induced by protons of very high energy [...] in which the atomic number of the impacted nucleus changes by one unit, while its atomic weight remains unchanged.

[...] G. Cocconi suggested that a peripheral interaction could generate these phenomena. T. Ericson, R. Van de Walle and I developed the theory of elementary interaction with one of the nucleons of the nucleus and its subsequent reabsorption. The results are in good agreement with the experimental data¹¹⁹.

2) Double photoproduction: In a discussion we had while walking under the arcades of Bologna, D. Boccaletti and I launched the ambitious program of “explaining everything” in the processes of double photoproduction, in particular in the reaction $\gamma + p \rightarrow p + \pi^+ + \pi^-$ (**). Our idea was the following: Since S.D. Drell had suggested that the diagram he considered at very high energy (tens of GeV) could be very important even at low energies (0.5-1.5 GeV) and since we expected an important contribution from the process $\gamma\pi \rightarrow \pi\pi$, being the initial π one of the virtual pions of the nucleon, we thought to put the two things together and calculate angular distributions, energy spectra, etc. Unfortunately, the ambitious initial program proved to be unattainable due to the fact that Drell’s diagram gave a much smaller contribution than expected. [...] Drell’s idea that there was evidence for his effect in the low-energy experimental data proved to be false. It must be said, however, that the importance of Drell’s work in the field of interactions induced by very high energy photons is not in the least diminished by these considerations. [Our results] were collected in a letter [...] to the *Nuovo Cimento*¹²⁰.

3) High energy elastic p - p diffusion: It happened one day at CERN that an experimental young man with a famous surname came to offer his services (in order to practise theoretical physics, he said) to E. Ferrari and me. We gave him to calculate the photoproduction process of pions [...] and with the disdainful certainty that we would never see him again. Instead, he reappeared in two days with the finished and exact calculations. So I decided that U. Amaldi [Jr.] was a very smart guy and that it was worth working with him.

We began to work together on an application of the condition of unity that had been suggested to me some time before by Fubini. The basic idea is as follows. The elastic P - P diffusion at high energies around the GeV appears to be dominated by the diffraction caused by the large anelastic cross section. On the other hand, the matrix element of the anelastic processes is known partly theoretically, partly logically, by the work of E. Ferrari and myself, regardless of whether one believes in the total dominance of OPE or not. Therefore, from the simple condition of unity we can arrive at calculating that part of the elastic matrix element that comes from diffraction (part that we believe to be dominant).

¹¹⁹ The paper mentioned is: Ericson T., Selleri F., Van de Walle R.T. (1962), “Low Momentum Transfer Reactions in Complex Nuclei at High Energy”, *Nuclear Physics*, 36, pp. 353-367

¹²⁰ The letter mentioned is: Boccaletti D., Selleri F. (1961), “Some Remarks on Double Photoproduction”, *Il Nuovo Cimento*, 22 (5), pp. 1099-1103.

The ultimate purpose of these calculations is the comparison of the angular cross sections calculated in this way with the experimental ones¹²¹ (Selleri unpublished document, 1961, pagg. 15-17-FSPA).

[...] I would like to discuss briefly what developments can reasonably be expected from the results we have achieved. A [theoretical] problem of great practical importance is, in my view, the complete and general solution of the interactions involving virtual particles. [...] Given the success of the OPE model at energies above 900 MeV, it is natural to ask what is the lower energy limit for the validity of this model. Unfortunately, the model is certainly not valid at low energy, in its current version. This is due to the proven presence of a strong interaction in the final state which sometimes leads to the formation of deuteron. Therefore it would be interesting to modify the model, taking into account this interaction, and then compare the results obtained with experimental cross sections. Another problem, in my opinion interesting, is that of elastic proton-proton diffusion at energies below the threshold for inelastic reactions. [...] The terms that can be calculated are: the exchange of a single pion. [...] the exchange of two pions contributing to interactions with large impact parameter. Since the pion-pion resonance was found to be at very high energy, the exchange of one resonant bipion between two impacting nucleons gives rise to a very small radius force. Therefore, a force of great range comes from the exchange of two mesons that do not interact with each other. such an exchange can also be calculated if we consider that in each of the two vertex we have substantially to do with a pion-nucleon diffusion in which both the incoming and the outgoing pion are virtual. Therefore there is the hope to reproduce the experimental low energy data taking into account the two previous terms as well as a parameter to be determined experimentally, which represents all the short-range contributions (bipion, tripion, etc.). [...] Future developments should not forget an obvious extension of the OPE model to other physical processes

[...] In conclusion, I believe that the problems listed here can already give a person of good will a year's work. And it seems to me that their solution allows to publish decent works, although certainly not of sublime importance (Selleri unpublished document, 1961, pagg. 18-20-FSPA).

Concerning the last statement, it should be said that Selleri showed different mood in several situations, switching between a high awareness, sometimes bordering on arrogance, in a profound and groundbreaking contribution to physics by his works, always defending his ideas against all odds; and a great

¹²¹ The collaboration between Amaldi and Selleri gave birth to two paper: Amaldi U. Jr., Selleri F. (1962), "Nucleon-Nucleon Diffraction Scattering", *The Physical Review*, 128 (6), pp. 2772-2783. And Amaldi U. Jr., Selleri F. (1964), "Unitarity Condition and Connections between Pion-Pion and Pion-Nucleon Scattering", *Il Nuovo Cimento*, 31, pp. 360-376.

humbleness in admitting his own mistakes and abandoning ways that are recognized as wrong.

1.6-EXPERIENCING THE WORLD

As a consequence of the new state of celebrity, related to his idea on OPE as well as to the pivotal papers written at CERN, Selleri decided to go abroad again. He spent one year (1962-1963) in France, at LPCHE, Laboratoire de Physique Corpusculaire à Haute Energie, Centre d'Etudes Nucléaires, Saclay, Gif-sur Yvette, hosted by professor A. Berthelot. I could not find any recollections concerning his period in France except several letters exchanged in the 2000s with Etienne Klein who worked at Saclay and also a brief reference in a Selleri's letter dated 15 May 2000 sent to a friend, in which he stated:

I have had a high level of scientific education (Bologna-CERN-Saclay-Cornell: this has been the training portion of my path¹²².

In any case, in Saclay he wrote four papers. The first one was "Off-Shell Pion-Pion Scattering in the $T=J=1$ State"¹²³ (Selleri 1962), published in *Physics Letters*.

Secondly, another joint work with Ezio Ferrari, "An Approach to the Theory of Single Pion Production in Nucleon-Nucleon Collisions"¹²⁴ (Ferrari, Selleri 1963), sent on *Nuovo Cimento* and there released when Selleri's present address was at Saclay, even though he was officially stationed in Bologna.

In the summary it can be read that:

The matrix element for the process of single pion production in nucleon-nucleon collisions is constructed under the hypothesis of a dominant contribution from the One-Pion Exchange term. The effects of the virtuality of the π -meson are fully taken into account. It is found that the finite size of the nucleon cannot be neglected. The method used allows the determination of the "pionic form factor" of the nucleon.

¹²² "Io ho avuto un'educazione scientifica di alto livello (Bologna-CERN-Saclay-Cornell: questa la parte formativa della mia traiettoria)". (Selleri to his friend Mariella, May 15th, 2000-FSPA).

¹²³ Selleri F. (1962), "Off-Shell Pion-Pion Scattering in the $T=J=1$ State", *Physics Letters*, 3 (2), pp. 76-77.

¹²⁴ Ferrari E., Selleri F. (1963), "An Approach to the Theory of Single Pion Production in Nucleon-Nucleon Collisions", *Nuovo Cimento*, 27 (6), pp. 1450-1483.

It is shown that, by the choice of form factor similar to the e.m. ones, a satisfactory agreement with the experiment is obtained from incident energies of (1-3) GeV. For higher energies, the connection with a possible “Regge pole behaviour” for such reactions is briefly discussed (Ferrari, Selleri 1963, pag. 1450).

Thirdly, “On the Universal Coupling of the ρ -Meson”¹²⁵, published on *Physics Letters* (Daudin, Jabiol, Kochowski, Lewin, Selleri and Mongelli, Romano, Waloschek 1963)¹²⁶. This joint paper sounds peculiarly seminal mainly because it was written by scholars working at Saclay and Bologna, including, for the first time, scholars from Bari. There is also a reference to a collaboration among the universities of Orsay, Saclay, Bari, and Bologna. It could be that during his period at Saclay or even before, Selleri might have known the experimental physicists of Bari University. Incidentally, they proposed in the text quoted below that the

isobar production is consistent both with the ρ -exchange model and with the hypothesis of the universal coupling of the ρ -meson (Daudin, Jabiol, Kochowski, Lewin, Selleri and Mongelli, Romano, Waloschek 1963, pag. 127).

So, the widening of the OPE exchange to other physical processes, expressed and wished for by Selleri in 1961 in his abovementioned Curriculum, seemed to find application here.

Analyzing the correspondence of Selleri, I was able to come across what seems to be another outcome of the collaboration between him and Amaldi U. Jr.¹²⁷, namely a 1967 paper entitled “Weak Interactions and Unitarity” But there is neither a sign of publication nor any real presence of the paper, though. Conversely, there is a letter sent by John Bell to Selleri:

Dear Dr. Selleri, I was very intrigued by your recent work with Amaldi on unitarity. After some effort I come to the following conception of the situation. I would be interested in your reactions.

¹²⁵ Daudin A., Jabiol M.A., Kochowski C., Lewin C., Selleri F., and Mongelli S., Romano A., Waloschek P. (1963), “On the Universal Coupling of the ρ -Meson”, *Physics Letters*, 7 (2), pp. 125-127.

¹²⁶ However, here Selleri was officially at Saclay but with a present address in USA, at Cornell University.

¹²⁷ They have already written “Unitarity Condition and Connections between Pion-Pion and Pion-Nucleon Scattering” (Amaldi U. Jr, Selleri 1964), appeared on *Il Nuovo Cimento*.

[...] Here is a case, it seems, where the exact solution of approximate equations can be misleading (John Bell to Selleri, 28 March 1967-FSPA).

Selleri replied to the arguments made by Bell with the following *erratum*:

Erratum: “Weak interactions and unitarity” by U. Amaldi Jr (Istituto Superiore di Sanità, Roma) and F. Selleri (Istituto di Fisica dell’Università, Bologna).

In going from equation (13) to equation (16) some terms have been unduly neglected. These terms, when retained, show that the discriminant of the system is zero. As a consequence the result about the non-consistency of weak interactions theory with unitarity is wrong. Sorry (Selleri to Bell, FSPA).

And by a handwritten remark in which he recognized his mistakes¹²⁸:

Dear Bell,

the work on weak interactions and unitarity was wrong, as you suspected, perhaps for reasons different from the ones of your letter. Sorry for the time you spent with it. Now there is consistency with unitarity and only now I understand it had to be this way because of the very construction of the S-operator. Best regards. Franco Selleri (Selleri to Bell, FSPA).

A possible explanation I have given myself is that the foregoing paper by Amaldi U. and Selleri circulated only as a preprint and that, as a result of Bell’s objections and the consequent recognition of errors, was withdrawn and therefore not published.

After the year spent in Saclay, in 1963, Selleri decided to relocate to the United States of America, where he had the opportunity to choose between five essential universities to which he sent an application letter.

Though Selleri in the end decided to go elsewhere, the first three letters below reveal the provision of two universities, the University of Maryland and the Northwestern University in Illinois, respectively:

Our department of Physics and Astronomy would be quite pleased to have you spend some time with us.

¹²⁸ I will report the whole original Bell’s letter, together with the erratum and the handwritten remark, at the end of the Addendum 3.

[...] [We can offer you] 700 \$ per month. [...] We would ask you to teach one course or two or three lectures per week, but the remainder of your time would be free for research of your own choice, either alone or in collaboration with members of our group [...] we hope you would be able to stay for as long as two years (John S. Toll-chairman of physics and astronomy-University of Maryland to Selleri, 9 November 1962-FSPA).

Your prompt reply to our offer of a faculty position is much appreciated. We are sorry to learn that you are unable to come to Maryland, but we are pleased that you will be in our country at Cornell next year. I hope that you will visit our department whenever you are in the Washington area. I will convey your best regards to prof. Greenberg (John S. Toll-chairman of physics and astronomy-University of Maryland to Selleri, 29 November 1962-FSPA).

I can arrange for your appointment to be of one year's duration. [...] transportations on a military flight [...]. I would like to explore with you the possibility of your coming here as an assistant professor. 8500\$ for nine month and you are free in the summer to accept other employment. Most people get paid an additional three month's salary working on research contracts. I mention this to you because I would imagine that you are not familiar with the American system [...]. People are typically employed twelve rather than nine months (Martin M. Block-Chairman Depy. of Physics-Northwestern University, Illinois to Selleri, 15 November 1962-FSPA).

Selleri had seminal contacts with very prominent scholars. The two following excerpts belong to different letters between Gian Carlo Wick, at Brookhaven at that moment, and Selleri, who was also going to spend some time in Brookhaven National Laboratory during his stay at Cornell:

Dear Selleri, I was very pleased to hear that you would be interested in spending some time at Brookhaven [...]. it is not necessary for you to send reprints of your papers. However, a couple of letters of recommendation from people who know you well in Europe would help in pushing the thing through with our administration (perhaps Weisskopf or Van Hove or Fubini). Are you thinking of a period of six months or a year?...I would just like to have an idea (Wick to Selleri, Brookhaven, 15 October 1962-FSPA).

I would be very interested in hearing your ideas about n-p exchange scattering and so, I am sure, is Barry Palevsky (Wick to Selleri, Brookhaven, 26 December 1962-FSPA).

I also found a significant letter by Puppi that was an answer to Selleri's inquiry about going to the USA (Selleri was at Saclay at that time).

It is reported below and it reveals a Puppi's extreme care for Selleri's choice as well as some valuable advice:

Dear Franco,

Thanks for your letter and I assure you that I have already written to Wick.

I heard from Cocconi that there would also be good chances at Cornell University because Bethe would be interested in having you.

Next year in Cornell there will also be a young theorist named Wilson, currently at CERN, who made a very good impression on me¹²⁹.

Of course, the two possibilities are very different. Cornell is a quiet and cosy, collected place while Brookhaven is a kind of battlefield. Both solutions are good and it's up to you to choose according to your inclinations. I strongly recommended you to Wick for Brookhaven, but if by any chance you prefer Cornell let me know.

In this year you will be spending in America, with both Wick and Bethe, do not worry about publishing works, but try to critically consolidate your culture and deepen your mathematical knowledge. In a while, I hope to return to Saclay and then we could talk about this issue¹³⁰ (Puppi to Selleri, 23 November 1962-FSPA. My underline).

In the end Selleri decided to go to Cornell University, maybe attracted by the presence of Hans Albrecht Bethe there, who would be awarded the Nobel Prize in 1967.

Actually, during his stay in Saclay, he sent a letter to Giuseppe Cocconi¹³¹, asking for some information concerning the opportunity to go to Cornell:

Dear Professor Cocconi,

As I would have decided to come to the United States next year, I would like to ask you if there is any chance for me to work at Cornell.

You will find attached to this letter a brief curriculum vitae and a list of my works¹³².

¹²⁹ Kenneth G. Wilson (1936-2013) won the Wolf Prize and the Nobel prize in 1980 and 1982 respectively.

¹³⁰ "Caro Franco, Ti ringrazio della tua lettera e ti assicuro che ho già scritto a Wick. Ho saputo da Cocconi che ci sarebbero anche buone possibilità a Cornell University in quanto Bethe sarebbe interessato ad avverti. Il prossimo anno ci sarà a Cornell anche un giovane teorico di nome Wilson, attualmente al CERN, che mi ha fatto molta buona impressione. Naturalmente le due possibilità sono molto differenti.

Cornell è un luogo tranquillo e raccolto mentre Brookhaven è una specie di campo di battaglia. Tutte e due le soluzioni sono buone e sta a te di scegliere secondo le tue inclinazioni. Io ti ho raccomandato fortemente a Wick per Brookhaven, ma se per caso tu preferissi Cornell fammelo sapere.

In quest'anno che passerai in America, sia con Wick sia con Bethe, non preoccuparti di pubblicare lavori, ma cerca di consolidare sul piano critico la tua cultura e di approfondire la conoscenza matematica. Fra poco, spero di ritornare a Saclay e allora potremo riparlare di questa questione" (Puppi to Selleri, 23 November 1962-FSPA).

¹³¹ Giuseppe Cocconi (1914-2008): https://en.wikipedia.org/wiki/Giuseppe_Cocconi. Accessed 24 September 2020.

[...] I am currently working on the interpretation of experimental data of single production in pion-nucleon collisions and on the theoretical interpretation of the $P+P \rightarrow D+\pi^+$ process.

I am also working on a general theory of nucleon-nucleon scattering¹³³ (Selleri to Cocconi, 10 October 1962-FSPA).

Before relocating to Cornell University, Selleri had also a correspondence with Bethe. Several excerpts I received by Cornell University-Division of Rare and Manuscript Collections¹³⁴ are here reported. In the first letter Bethe introduced to Selleri several details concerning the salary, the opportunity of extending the appointment for a second year and a clear explanation of the physical research made at Cornell:

Dear Dr. Selleri:

Professor Cocconi has given me your letter of October 10 to him. Accordingly I should like to invite you to come to Cornell for one year as a Senior Research Associate. I assume from your letter that you want to come for the next academic year, i.e. about September 1963 to 1964.

[...] We can offer you a salary of 7200\$ for the academic year of nine months. [...] We can pay you [...] for two additional months during each summer. We should also be happy to extend your appointment for a second year if this is satisfactory to both you and ourselves after a few months of your stay here.

You probably know the type of physics research we are doing here. We have an active theoretical group on high energy physics, including T. Kinoshita, P. Carruthers, Kenneth Wilson and several younger people. I am also beginning to work more in this field. We have a very strong experimental group led by professor Robert R. Wilson working on the electron synchrotron.

This machine, now at 1.4 GeV, will soon be rebuilt to give 2.2 GeV. The main experiments presently being done are electron scattering by nucleons and production of strange particles by gamma rays.

¹³² In his CV, it is interesting to stress that Selleri presented his career at the moment as many as 12 papers, also underscoring the prize for Young Researchers he won in 1961 at the Italian Physical Society.

¹³³ "Caro Professor Cocconi, siccome avrei deciso di venire negli Stati Uniti l'anno prossimo, vorrei chiederle se c'è per me qualche possibilità di lavorare a Cornell. Troverà insieme a questa lettera un breve CV e una lista dei miei lavori. [...] Sto lavorando presentemente all'interpretazione dei dati sperimentali di produzione singola in urti pione-nucleone e all'interpretazione teorica del processo $P+P \rightarrow D+\pi^+$. Lavoro inoltre su una teoria generale dello scattering nucleone-nucleone" (Selleri to Cocconi, 10 October 1962-FSPA).

¹³⁴ 14-22-976_b45_f48_all-BOX 45-FOLDER 48-Title: Hans Bethe papers,[ca. 1931]-1995. Author: Bethe, Hans A. (Hans Albrecht), 1906-2005. Volume/Box: box 45. CU-DRMC.

Professor Cocconi, to whom you addressed your letter, is unfortunately no longer here but has accepted a position at CERN. However, we have many other good professors both in theory and experiment.

I have discussed your application with all members of the theoretical nuclear physics group, and they all join me in the hope that you will accept our offer [...] (Bethe to Selleri, 7 November 1962-CU-DRMC).

Selleri accepted Bethe's proposal, and he explained his willingness to go back to Italy after one year, even if he left the door open for a second year in the U.S., as was later the case.

Dear Prof. Bethe,

[...] It is for me a pleasure and an honour to accept your offer to come next year to Cornell. My desire would be to come for the period you mentioned, namely for the next academic year, starting September 1, 1963. After this my present program is to go back to Italy but it might be that I decide to stay one more year in the United States.

Before asking some questions of economical nature, let me stress that my decision to come to Ithaca is not conditional on the answer to them [...] (Selleri to Bethe, 16 November 1962-CU-DRMC-My underline).

But above all, a long series of requests and economic facilitations began, which have been the subject of several letters, of which only a few are reported here. In the letter below, Bethe explained in detail how to live in the U.S. and tried to accomodate Selleri's concerns:

Dear Dr. Selleri,

I was very happy to get your acceptance of our offer of a research associate position for the coming Academic Year, 1963-64. You should be able to live comfortably on 800 \$ a month in Ithaca. It is the usual salary of our assistant professor, and they generally seem to manage quite well. You were probably given an exaggerated idea of the deductions as well as the prices in this country. Your deductions will not be anything near 30%...

[...] It is likely that your taxes will run less than 10% in 1963 and somewhat over 10% in 1964.

As far as I can tell by going to Europe, prices for food in stores are as high in Europe as they are here; occasionally some things are cheaper here. There are three things which may make living expenses higher in the U.S. than in Europe:

1. Rents are generally higher but you should be able to find an apartment for your family for \$ 100 to \$125 a month, or a furnished house for about \$150 a month [...].

2. You have to have an automobile which in Europe you do not need to have. On the other hand, second-hand cars are very cheap and can be resold after a year with a loss of perhaps \$ 200 or \$ 300. Gasoline is very much cheaper than in Europe and perhaps the only major expense apart from the purchase price is insurance.

3. Any kind of domestic help is expensive, \$ 1.00 to \$ 1.25 an hour, but there are so many electrical appliances that domestic help is far less needed than in Europe. Probably your wife will have to get along with little or no domestic help. Because services generally are expensive, meals in restaurant are apt to be expensive as well. But the University has two good cafeterias...which serve good meals at about \$ 1.00. I am mentioning all this to show you that \$ 800 a month should be quite ample to cover your needs.

However, in view of your doubts about the financial side, and especially because you have had your doctor's degree longer than most of our research associates, we shall be able to raise your salary to \$ 7500 for the academic year of 9 months.

[...] It will certainly be possible to pay your own expenses by air, economy class.

[...] If you go by boat, I suppose you can save some money.

I understand from your letter that you would come here in September. We are very much looking forward to your coming.

In due time you will receive an official letter of appointment as a research associate [...] (Bethe to Selleri, 14 December 1962-CU-DRMC. It is the only letter found in FSPA).

Then, in the next three letters below, there are some clear statements of interest, understanding and longing for the experience that was about to begin:

Dear Prof. Bethe,

Thank you for your very reassuring letter of December 14. I am particularly indebted for the salary increase and for your kindness in explaining me the American life.

[...] I hope very much (for my good) that you shall not change mind about working more in elementary particle physics, next year. Waiting for the official letter, I remain sincerely yours. (Selleri to Bethe, 8 January 1963--CU-DRMC. Handwritten).

Dear Dr. Selleri,

[...] I am happy to hear that you are planning to be in Cornell about the 5th of September. This is a very convenient time. I expect to be back from my summer travels just about the same time [...] (Bethe to Selleri, 30 April 1963-CU-DRMC).

Dear Prof. Bethe,

[...] I look forward to the day of my arrival in Cornell, sure that the time spent there will be very profitable and interesting for me. I sincerely hope that Cornell will get something out of me too (Selleri to Bethe, 10 May 1963-CU-DRMC).

At the end of Selleri's experience at Cornell, Bethe sent to the administration of his University a letter to validate Selleri's activity there, with no great details, though:

Dear Sir:

This is to certify that Dr. Franco Selleri has been here as a Research Associate since October 1963 and expects to return to Italy this summer.

Dr. Selleri was on leave from the Istituto Nazionale di Fisica Nucleare, Sezione di Bologna, in Bologna, Italy, and a job is awaiting for him there upon his return.

Dr. Selleri's work here served for the improvement of his professional capabilities as a research physicist... (Bethe to Internal Revenue Service, 16 February, 1965-CU-DRMC).

At Cornell, Selleri attended the Annual summer conference on high-energy physics at the Institute for Theoretical Physics in Boulder, University of Colorado presenting two talks. By looking at the conference program I managed to identify their subject:

- 30 June 1964, at 9.15-10: F. Selleri (Cornell): πN collisions below 1.5 BeV/C;
- 3 July 1964 at 3.15-4.00 PM: F. Selleri (Cornell): On the reaction $p + p \rightarrow D + \pi^+$.

It has already been said that during Selleri's stay in USA, he spent some time at Brookhaven National Laboratories (BNL). The two letters below, signed by R. Christian Anderson, Assistant Director of BNL, actually confirmed that Selleri carried out his research there during the summer:

Dear Dr. Selleri: I am happy to inform you that you have been appointed Consultant in the Physics Department effective November 4, 1964 (Brookhaven National Laboratories to Selleri-4 November 1964-FSPA).

You have been appointed visiting associate physicist [...] effective July 26 to September 3, 1965.

[...] It is understood that you will remain affiliated with Cornell University and will be here during a normal recess (Brookhaven National Laboratories to Selleri-11 March 1965-FSPA).

In the USA, despite Puppi's advice, Selleri still found the time to publish some papers, some of which are reported in the abovementioned conference proceedings. The first paper was "Deuteron Production in p-p Collisions"¹³⁵, (Chahoud, Russo, Selleri 1963¹³⁶), appeared on *Physical Review Letters*. A work supported in part by the US Office of Naval Research and also published in 1964 in the Bulletin of the American Physical Society. It showed another confirmation of the OPE Model:

In a recent note, Cocconi et al, have shown that the total cross section for the reaction $p + p \rightarrow d + \pi^+$ (1) is likely to present a maximum at total c.m. energy [that exceeds] the well-established maximum. The above authors observed also that these values of E^* correspond almost exactly to the masses of the first and fourth pion-nucleon resonances plus a nucleon mass.

It is the aim of the present note to point out that these facts, together with the nonappearance of the second and third πN resonances, are explained by a simple model of deuteron production. As the reaction (2) $p + p \rightarrow p + n + \pi^+$ in the same energy region is well explained by the one-pion exchange (OPE) model, we assume that reaction (1) is generated by the OPE mechanism followed by an interaction in the final state, which binds p and n into a deuteron (Chahoud, Russo, Selleri 1963, pag. 506).

[...] We conclude that the total cross-section for reaction (1) is very sensitive to the exchanged isospin in the production step and that the present experimental evidence points to a rather strong depression of the exchange of zero-isospin systems. This results agrees with the OPE model for reaction (2) (Chahoud, Russo, Selleri 1963, pag. 507).

Then, two papers both appeared in *Lectures on Theoretical Physics*, Vol. VII B made by the University of Colorado Press, and also presented at the Elementary Particles Conference, held at the Institute for Theoretical Physics, University of Colorado, in the summer of 1964 (the same conference I mentioned at the previous page). They are:

¹³⁵ Chahoud J.N., Russo G., Selleri F. (1963), "Deuteron Production in p-p Collisions", *Physical Review Letters*, 11 (11), pp. 506-507.

¹³⁶ Selleri's two co-authors would work with him in another paper in Bologna University three years later

“Theoretical Models for Simple Inelastic Reactions”¹³⁷, (Selleri 1965a) where he continued to compare the OPE predictions with experimental observations, also considering the absorption effects; and, “Pole Model of Isobar Production”¹³⁸, (Selleri 1965b).

The next three papers from the American period sound a little bit unusual. They are quite definitely written in USA, all three are supported in part by the US Office of Naval Research, but, really unexpectedly, they are all published on the Italian journal *Il Nuovo Cimento*, maybe when Selleri came back to Italy. It could also be that, having already published in that magazine, he deemed appropriate submitting these papers to it.

It could be interesting to open a parenthesis analyzing the role of this journal. *Il Nuovo Cimento* was founded in 1855 and became an official publication of the Italian Physical Society in 1897, year of SIF’s foundation. It is currently a release of the Italian Physical Society (SIF), whose editorial committee are usually made up of members of the Steering Committee of the Society and whose director is normally the President of the same SIF¹³⁹. They distinguished themselves in the international scene especially for the publication of many remarkable articles on FQM from all over the world but, as shown here, they also accommodated several articles on PP. Selleri was very much present in both fields. For instance, he published in the foregoing journal¹⁴⁰ up to 24 papers on PP (out of 40) during the years 1960-1970; and 17 papers on FQM (out of 125 written in English) during the years 1969-1985.

Unlike the field of PP, concerning FQM, Selleri could rely on a greater differentiation between the journals, even publishing in different languages (as, for instance, French and Italian). Actually he had his works translated in German, Spanish, Greek and Japanese.

¹³⁷ Selleri F. (1965), “Theoretical Models for Simple Inelastic Reactions”, in *Lectures on Theoretical Physics*, Vol VII B, University of Colorado Press, pp. 183-205.

¹³⁸ Selleri F. (1965), “Pole Model of Isobar Production”, in *Lectures on Theoretical Physics*, Vol VII B, University of Colorado Press, pp. 236-248.

¹³⁹ Selleri joined the SIF Steering Committee starting on 11 January 1969 until 30 October 1970.

¹⁴⁰ Selleri published also on *Supplemento al Nuovo Cimento* and *Lettere al Nuovo Cimento*.

Foundations of Physics is another outstanding journal, really active since those years and totally devoted to the new groundbreaking subject of FQM, which would become, starting from the 1980s, a mainstream research topic.

Anyway, the three papers written in the USA by Selleri are:

“On the Theory of Pion Production”¹⁴¹, (Selleri 1965c), published on *Il Nuovo Cimento*, in which he summarized several results obtained until then:

We express the matrix element and the cross-section for single-pion production in pion-nucleon collisions in terms of four scalar invariant amplitudes. [...] pion-pion scattering is also included. Full account is taken of the virtuality of the different intermediate states. The model provides a natural explanation of the 1512 MeV D_{13} resonance, which should be accompanied by a P_{11} resonance at roughly the same energy.

Notwithstanding the large amount of experimental information accumulated in the last 3-4 years, there is still little theoretical understanding of the inelastic pion-nucleon collisions. Below about 1 GeV the dominating process is the production of the (3,3) isobar and isobar models have successfully explained several qualitative features of these reactions. Above 1 GeV isobar production becomes more rare in single-pion production at least in the state of isospin $\frac{1}{2}$ (Selleri 1965c, pag. 278).

[...] We have presented a formulation of the theory of single-pion production in πN interactions based on the assumption of dominance of the singularities nearer to the physical region. Actually we have neglected one of the nearer singularities, the isobar intermediate state. This omission is a necessity at the present stage, due to ignorance of the $\pi\Delta\Delta$ vertex. There are, however, some hopes based on empirical evidence that this term may actually be small.

From the results of this paper alone it will not be possible to make quantitative comparison with experiments. What we have achieved is only an expression of the matrix element of pion production in terms of more elementary quantities, namely:

The form-factors of the πNN vertex for a virtual nucleon.

The form-factors of the nucleon propagator.

The partial-wave amplitude for πN scattering in the $T=J=3/2$ state with one virtual nucleon.

The form-factors of the $\pi\pi\rho$ -vertex and of the ρ -propagator.

The magnetic dipole amplitude of the process $\rho N \rightarrow \pi N$

¹⁴¹ Selleri F. (1965), “On the Theory of Pion Production”, *Il Nuovo Cimento*, 37, pp. 278-300.

These quantities are all calculable, under certain assumptions, and deserve separate theoretical analysis. Their determination forms the objects of separate papers (Selleri 1965c, pagg. 299-300).

“Virtual Nucleon Effects”¹⁴², (Selleri 1965d), *Il Nuovo Cimento* and “Virtual Pion-Nucleon Scattering”¹⁴³, (Selleri 1965e), always released on *Il Nuovo Cimento*. In the latter, Selleri explained that:

[...] the problem of pion-nucleon scattering when the initial pion is virtual with the aid of one-dimensional dispersion relations, under the usual assumption that the dispersive integrals are dominated by the $I=J=3/2$ partial-wave amplitude. We reconsider the problem of obtaining the off-shell behaviour of the latter amplitude by avoiding certain approximations which had been used previously. The improvements are important at high momentum transfer. We calculate furthermore the other partial-wave amplitudes. The significance of this result in certain production processes is discussed (Selleri 1965e, pag. 236).

[...] One of the aims of the present paper is therefore the calculation of the “small” partial waves when the incoming pion is virtual. Another one is the re-derivation of the linear integral equation for the $I=J=3/2$ amplitude. This equation was obtained in the static limit with an approximate projection of the above amplitude, which neglected the presence of higher waves in virtual pion-nucleon scattering. [...] The problem needs clearly to be rediscussed. It will be shown that exactly the same equation is obtained with the exact projection and without the static-limit approximation (Selleri 1965e, pag. 237).

In the above three works written in the USA, Selleri elaborated the peripheral model applied to inelastic models by discussing the virtuality effects of the pion and nucleon as well as using dispersion relations.

To sum up, in the USA Selleri not only perfected his preparation, as advised by Puppi, but continued to publish new works and to propose his ideas in different fields.

¹⁴² Selleri F. (1965), “Virtual Nucleon Effects”, *Il Nuovo Cimento*, 39, pp. 1122-1139.

¹⁴³ Selleri F. (1965), “Virtual Pion-Nucleon Scattering”, *Il Nuovo Cimento*, 40, pp. 236-249.

1.7- BACK TO THE UNIVERSITY OF BOLOGNA, ITALY

After his experience at Cornell University and Brookhaven National Laboratories, Selleri decided to come back to Italy. As I have shown, in a letter to Bethe, Selleri had in mind his homecoming to Italy after one or two years long before his trip in USA:

After this my present program is to go back to Italy but it might be that I decide to stay one more year in the United States (Selleri to Bethe, November 16th, 1962).

And Bethe declared in the final letter to the Internal Revenue Service that:

Dr. Franco Selleri [...] expects to return to Italy this summer. [...] a job is awaiting for him there upon his return (Bethe to Internal Revenue Service, February 16th, 1965).

In addition, concerning the reasons for Selleri's homecoming, one should bear in mind what Selleri disclosed to Freire Jr. in his 2003 interview:

[...] I could choose what to do, so I decided I wanted to go to the United States. I applied to five different universities and I got five positive answers so that in 1962 I decided to go to Cornell. I was attracted by the presence there of the famous physicist Hans Albrecht Bethe. But then, slowly, I started to develop a critical attitude towards contemporary physics. I mean I had a very strong drive. I liked physics very, very much and I was very active, but it was soon evident that there were problems, fundamental problems in physics. Already in 1965 I could see them.

[...] My model was, so to say, overcome by a different model that was called the one pion exchange model with absorption. Absorption means that instead of having plane waves in the initial state and in the final state you have to use distorted scattering waves. But that was very odd conceptually, because absorption of waves means that the initial nucleons and the final hadrons had strong interactions, and strong interactions go for example via pion exchanges and other similar ways.

So you had the one-pion-exchange model with absorption describing the inelastic process more or less as follows: a complicated interaction in the beginning, then a one-pion-exchange, then another complicated interaction in the final state. But then what is the sense of talking about one-pion-exchange?

Perhaps today it does not seem so terrible, I know, but for me it was shocking. That was a frustrating development. I was very young and naive, so it was very difficult to take. So I slowly realized that if something like that was possible, that meant there were great problems in physics.

And somehow I got very soon the conviction that the problems came from the foundations of quantum mechanics. That is to say, the problems in elementary particle physics are due to the fact that quantum mechanics is poorly understood and anyway is a very abstract idea.

There were also other problems. I mean not only my one-pion-exchange model. For example the Regge poles. They are singularities in the complex angular momentum plane, and one can well wonder what they could have to do with the physical world. I soon disliked the idea because theoreticians did not only use poles, but very soon to fit the experimental data were forced to use the so-called Regge cuts. In the pole you have a coupling constant that is free, but in the cut you have a whole free function, the discontinuity across the cut, meaning that you can use this freedom to fit just about anything. You can for example fit the weather of the last month over Europe. The whole enterprise with Regge poles did not look like good physics to me.

So what kind of physics was that? I mean that Regge was not responsible for this, because his work had been done in potential scattering and was applied to high energy particle physics by other people in California who were trying to understand more. It was like a fashion that was followed by other fashions: dispersion relations as dynamical tools, Mandelstam representation [...] and so on. These were very abstract ideas, very different from reasonable conjectures about the objective reality to hope that they could possibly work. With such a start it is clear that I am not at all surprised of the great difficulties that particle physicists face today. These difficulties are the natural outcome that has been building up for a long time (AIP, 2003. My underline).

Something happened during his stay in the USA! Something which was relevant in his choice to return to Italy, namely, his awareness of the difficulties that particle physics faced already at that time and their relationship with Foundations of Quantum Mechanics.

It would have been interesting to analyse a few details about the Selleri's experience in the USA. In the next chapters I will report some evidences by Selleri himself, always very critical and sharp on the USA and their *modus vivendi*. Criticism surely linked to the role played by the USA in foreign policy and by the adhesion of Selleri to the Italian Communist Party (PCI). But also related to the direct experience of Selleri in the USA.

Unfortunately, he had that experience with his first wife, which I was unable to contact in order to do an interview.

His new job after his return to Italy was the teaching of quantum field theory, as early as 1965 and this is explained below by Selleri himself:

When I came back to Italy I had important teaching to do. It was quantum field theory at the so-called “Scuola di perfezionamento” to people who had already a degree in physics. And I was teaching using the book by Bogolyubov¹⁴⁴ and I remember it was very hard. I was working very, very hard to prepare those lectures. And in research I was continuing to do what I had learned for more complicated versions of models for strong interaction (AIP 2003).

That is the first evidence of Selleri’s commitment in QM!

During his teaching activity in Bologna, Selleri wrote several papers, all published on *Il Nuovo Cimento*. The first one was “Is Absorption Necessary in High-Energy Inelastic Processes?”¹⁴⁵, (Selleri 1966). In the abstract he introduced, using two different topics, the fact that the absorption model to explain the inelastic high-energy processes is not necessarily valid:

We present two arguments against the presence of important absorptive effects in high-energy physics. The first comes from the phenomenological analysis of single-pion production in pp collisions and the second from the application of the random phase approximation to the unitarity condition for an inelastic process (Selleri 1966, pag. 835)

Then, “Theory of Deuteron Production in Proton-Proton Collisions”¹⁴⁶, (Chahoud, Russo, Selleri 1966). In this, as in the previous work with Chahoud and Russo, Selleri proposed a theory, based on the peculiar analytical properties of the matrix element of formation of a state bound with small binding energy, to explain the process of formation of a deuteron in the

¹⁴⁴ Nikolay Bogolyubov (1909-1992): See https://en.wikipedia.org/wiki/Nikolay_Bogolyubov. Addressed 24 September 2020. The book mentioned by Selleri can be one of the two published by Bogolyubov in those years, that is:

N. N. Bogoliubov, B. V. Medvedev, M. K. Polivanov (1958): *Problems in the Theory of Dispersion Relations*. Institute for Advanced Study, Princeton; or

N. N. Bogoliubov, D.V. Shirkov (1959): *The Theory of Quantized Fields*. New York, Interscience. This is the first text-book on the renormalization group theory. And this is also the book Selleri mentioned in his (Bonsignori, Selleri 1960). It could probably be this latter book the one he chose.

¹⁴⁵ Selleri F. (1966), “Is Absorption Necessary in High-Energy Inelastic Processes?”, *Il Nuovo Cimento*, 42, pp. 835-843.

¹⁴⁶ Chahoud J.N., Russo G., Selleri F. (1966), “Theory of Deuteron Production in Proton-Proton Collisions”, *Il Nuovo Cimento*, 45, pp. 38-57.

proton proton collision obtaining results in good agreement with the experimental data.

Another paper was “Nearest-Singularity Model for Vector-Meson Photoproduction”¹⁴⁷, (Graffi, Selleri 1967) in which the authors proposed a dispersive model to calculate the photo-reproduction of vector mesons without presenting any definitive results, though.

The last paper was “On the Solution of Dispersion Relations for the Photoproduction and Electroproduction of the $T=J=3/2$ Isobar”¹⁴⁸, (Grecchi, Selleri, Turchetti 1967), in which the authors proposed a relativistic solution of the dispersive equations of the photoreproduction of the isobar $I=J=3/2$, with a similar result for its electroproduction; the results are discussed in comparison with the experimental data.

Actually, there are other four articles certainly written in Bologna but that were then published when Selleri was already at the University of Bari. They are also all issued on *Il Nuovo Cimento* but one, here listed as (Selleri 1967).

In “Azimuthal Dependence of $\pi\pi$ Scattering”¹⁴⁹, (Grecchi, Selleri, Turchetti 1967), they account for the Yang-Treiman anisotropy in the process $\pi+N \rightarrow \pi + \pi + N$, assigning the effect to the interference between the ruling channel $\pi + N \rightarrow \rho + N$ and the less important channel $\pi + N \rightarrow \pi + N^*$.

Then, “Cross Sections for the $C^{12}(\pi^-, \pi^- n) C^{11}$ and $Cu^{65}(p, p \pi^+) Ni^{65}$ Reactions”¹⁵⁰, (Selleri 1967), issued on *The Physical Review*, in which he worked out the kinematic corrections to the processes thus improving the agreement of the total cross sections with the experimental ones.

In “Rho-Meson Coupling Constants and e.m. Form Factor of the Pion”¹⁵¹, (Selleri 1968a), he showed that the electromagnetic form factor of pions is

¹⁴⁷ Graffi S., Selleri F. (1967), “Nearest-Singularity Model for Vector-Meson Photoproduction”, *Il Nuovo Cimento*, 51A, pp. 221-226.

¹⁴⁸ Grecchi V., Selleri F., Turchetti G. (1967), “On the Solution of Dispersion Relations for the Photoproduction and Electroproduction of the $T=J=3/2$ Isobar”, *Il Nuovo Cimento*, 52A, pp. 314-318.

¹⁴⁹ Grecchi V., Selleri F., Turchetti G. (1967), “Azimuthal Dependence of $\pi\pi$ Scattering”, *Il Nuovo Cimento*, 52A, pp. 1306-1312.

¹⁵⁰ Selleri F. (1967), “Cross Sections for the $C^{12}(\pi^-, \pi^- n) C^{11}$ and $Cu^{65}(p, p \pi^+) Ni^{65}$ Reactions”, *The Physical Review*, 164 (4), pp. 1475-1478.

¹⁵¹ Selleri F. (1968), “Rho-Meson Coupling Constants and e.m. Form Factor of the Pion”, *Il Nuovo Cimento*, 53A (3), pp. 657-664.

independent of the high-energy behaviour of the phase δ in $I=J=1$; and it then calculates the coupling constant $\rho\pi\pi$.

Finally, in “Unitarity and Inelastic Processes”¹⁵², (Selleri 1968b), he claimed that:

It is well known that unitarity provides a set of relations between the matrix elements for transitions between different coupled channels.

[...] We consider the transitions between two states $|1\rangle$ and $|2\rangle$ describing two sets of particles. We show that the unitarity alone specifies completely the matrix element for the considered process, once other matrix elements at the same energy are given, if there is at least one further state $|3\rangle$ to which the states $|1\rangle$ and $|2\rangle$ can make a transition (Selleri 1968b, pag. 522).

1.8-A BRAND NEW PLACE: BARI UNIVERSITY

After spending two years at the University of Bologna, Selleri decided to relocate to the University of Bari, where he received teaching assignments for the academic year 1967/68, as external lecturer.

Concerning the reasons related to his passage to Bari, Selleri summarized in his recollections that:

[...] Bologna University was in trouble in physics at the time. There were struggles and problems. They had made mistakes in the choices of people to head the physics institute, that is my feeling anyway. So that was strongly reflected on the “climate”, I mean the political and cultural climate. In Bari there was this new university and I met with Bari people in a congress and they told me they would be happy if I came to their university, so I left Bologna and its problems and came here.

[...] [In Bari University] they were experimentalists, because Bari at that time had no theoretical physicists yet. It so happened that I published the first paper in theoretical physics of Bari University. But it was a very good experimental group because there was a bubble chamber collaboration, called Saclay-Orsay-Bari-Bologna from the names of the participating institutions, that was well known internationally at those times.

In Bari there were several good experimental physicists. The director was Michelangelo Merlin, other people included Aldo Romano, Nicola Armenise, Vittorio Picciarelli, Alberto Silvestri, Francesco Romano with whom I kept good relations.

¹⁵² Selleri F. (1968), “Unitarity and Inelastic Processes”, *Il Nuovo Cimento*, 55A, pp. 522-526.

I think that for me it was a very good decision to come here, not only because the human relationships were much better, much more relaxed than in Bologna, but also because here I had space to do what I wanted and I had no great difficulties in doing foundations of quantum mechanics. In Bologna it would have been more difficult (AIP, 2003. My underline).

In the quoted interview, Selleri also stated that:

Already in Bologna before coming to Bari I started saying I did not believe anymore that quantum mechanics was correct. And the reactions were negative. I think that it was also from that point of view very positive that I came to Bari because in Bari everybody was young at the time. Very few people were older than I was, so I had a lot of space so to say, cultural space — yes? — that in Bologna I would not have had, because there were older professors with very different ideas, ideas opposite to mine, especially in theoretical physics. And so I believe that those people would have been a terrible obstacle, very difficult to overcome. Instead nobody like that existed in Bari and here my activity was easier (AIP, 2003).

It is worthwhile to highlight here how in this recollection, Selleri spoke about a Saclay-Orsay-Bari-Bologna bubble chamber collaboration. I would like to remind the reader that Selleri wrote a joint paper in 1963 when he was in Saclay¹⁵³ together with three experimentalists from Bari University. I am guessing that the meeting Selleri talked about in the previous page may have taken place several years before he got to Bari, perhaps at CERN in 1961 at the Geneva Conference of High Energy Physics. I am assuming that it could be also really useful to seek for the beginning of the collaboration between the four above-mentioned universities.

In addition, in November 1965, when at Bologna University, Selleri presented two talks in the 51st National SIF Congress held in Bologna, both held in Italian.

¹⁵³ “On the Universal Coupling of the ρ -Meson”, (Daudin, Jabiol, Kochowski, Lewin, Selleri and Mongelli, Romano, Waloschek 1963. I introduced it at page 81 of this dissertation.

The first one was a joint one, written by G. Russo, J. Chahoud, and F. Selleri, *Un modello dinamico per la produzione di deutoni in urto p-p*, whereas the second, presented by Selleri alone, was entitled *Alte energie senza assorbimento*¹⁵⁴.

It is likely that Selleri, at the conference, came into contact again with several particle physicists from Bari, who had presented several works. It seems clear that the collaboration among the different institutions such as Saclay, Orsay, Bologna, Bari, and Florence was very active, thus contributing to this conference with five articles.

Going back to the grounds for Selleri arrival at Bari, my colleague Dr. Flavio del Santo, working in Wien Universität at Institute for Quantum Optics and Quantum Information, has recently sent me the following very appealing statement concerning Selleri's switchover from Bologna to Bari as described by Prof. Herbert Pietschmann in his recollections:

“And he [Selleri] shifted to Bari and the reason is, he said, if in the faculty he makes a request and he gets the majority, this would not help because as soon as the faculty is over Zichichi¹⁵⁵ would call the ministry and tell him he wanted to do it differently. So that's the reason why he left Bologna”. (Conversation between Dr. Del Santo and Prof. H. Pietschmann).

But another possible key to Selleri's shift to the University of Bari could be found between the folds of my interview with Dr. Michele Barone. Concerning the motivation that led Selleri to leave the University of Bologna, Barone has explained that¹⁵⁶:

Franco Selleri and Silvio Bergia graduated in physics in the same year and session. They both attended the same competition, yet Bergia was the winner. Franco, at the moment, was working on his peripheral model, really appreciated by the physicist community, and his shift to FQM was yet to come.

¹⁵⁴ A dynamic model for the production of p-p impact deuterons and High energies without absorption, respectively.

¹⁵⁵ Antonino Zichichi (1929-): http://www.ccsem.infn.it/em/zichichi/short_bio_it.html. Accessed 24 September 2020.

¹⁵⁶ Michele Barone has known this information by Silvio Bergia himself.

As a reaction to the unjust outcome of the competition, and with the hope of being able to get to the professorship, Franco accepted the proposal by Fabio Ferrari¹⁵⁷ to reach him in Bari (Interview with Michele Barone, Bitonto (BA), 1 June 2019)¹⁵⁸.

And, regarding the relationship between Selleri and Zichichi, Barone told me, among other things, that:

Well, of course there were some quarrels. Zichichi was close to USA, was representing the establishment; Franco did not even drink Coca Cola, for instance. But, beyond that, nothing! Zichichi played a remarkable role for physics in Italy, doubling the budget of INFN when he was President, for instance. And Zichichi has appreciated Franco's research, including those of FQM. He has given some funding to support research on Bell's inequality. And Franco was surprised by this (Interview with Michele Barone, Bitonto (BA), 1 June 2019).

I found a really controversial letter that Selleri sent to Aldo Cossu, the Rector of University of Bari, concerning a lot of internal problems he and his group on FQM were experiencing in the Institute of Physics. And Selleri quoted that Zichichi took a stand on his behalf! Moreover, in a letter to *Physical Review*, dated February 1997, Selleri, submitting a paper for publication, stated:

I have been told that you welcome suggestions of possible referees. Well in this case three good names are: Prof. Philippe Eberhard, Prof. Antonino Zichichi, Prof. Matts Roos (Selleri to *Physical Review*, February 1997-FSPA. My underline).

So, as often happens, it could be useful, in order to reach an almost convincing conclusion, try to analyze all possible sources about any matter. At Bari University, being the first Theoretical Physics Professor, Selleri dealt with PP at first.

¹⁵⁷ Fabio Ferrari was the Director of the Institute of Physics at University of Bari in the period 1968-1972.

¹⁵⁸ Michelangelo Merlin has played the main outstanding role for the birth, development and absolute breakthrough of the Institute of Physics in Bari. He proposed to Ferrari to call physicists for theoretical physics chairs.

His first paper totally written in Bari and published on *Nuclear Physics* when Selleri belonged to the new University was: “On the npd Vertex Functions”¹⁵⁹, (D’Auria, Predazzi, Selleri 1968). The authors stated that:

It is shown that a reliable calculation of the npd vertex functions must take into account the effects on the form factors describing the $N\pi N$ vertex with a virtual nucleon. These form factors are known with good accuracy in the region of interest. An explicit calculation shows that their effect is rather important and that the sum rules expressing the normalizability conditions of the deuteron wave-function are much better satisfied, in the anomalous region approximation, when such form factors are taken into account (D’Auria, Predazzi, Selleri 1968, pag. 645).

“Elastic Unitarity and Phase Equality of Scattering Amplitudes”¹⁶⁰, (Giovannini, Predazzi, Selleri 1968) was issued on *Physical Review*. The authors stated that:

Recently Törnqvist suggested that $\pi\pi$ scattering below the inelastic threshold is compatible with the assumption that the phases of the $\frac{1}{2}$ and $\frac{3}{2}$ isospin amplitudes differ by $n\pi$. We show in the scalar case that this phase relation cannot hold over the whole angular interval; for elastic unitarity would then, in general, force the two amplitudes to have equal moduli also (Giovannini, Predazzi, Selleri 1968, pag. 1660).

1.9-THE FIRST TURNING POINT: THE ANGULAR-MOMENTUM NONCONSERVATION. THE ISSUE AT STAKE

At Bari University, Selleri produced outstanding papers concerning a new subject which was really remarkable for his future shift to FQM: the angular momentum nonconservation, a “Tentative Theory of Elementary Particles”, as he named it in one of his many lists.

The relevant papers, all written in two years, are here reported:

1968 Relativistic Invariance without Angular Momentum Conservation

1969 The Physics of Spin $\frac{1}{2}$ K mesons

1969 The Multi-Regge Model and Strange Particle Production

¹⁵⁹ D’Auria R., Predazzi E., Selleri F. (1968), “On the npd Vertex Functions”, *Nuclear Physics*, B8, pp. 645-653.

¹⁶⁰ Giovannini A., Predazzi E., Selleri F. (1968), “Elastic Unitarity and Phase Equality of Scattering Amplitudes”, *The Physical Review*, 173 (5), pp. 1660-1663.

1969 Noninvariant Vacuum and Relativistic Quantum Field Theory

1969 Two-Body Processes and Regge-Pole Model

1970 Angular Momentum Nonconservation Scheme

“Relativistic Invariance without Angular-Momentum Conservation”¹⁶¹, (Selleri 1968c), appeared on *Il Nuovo Cimento*. In it, Selleri stated the main topic of his research on this subject, creating his theory and searching for confirmations, while creating and proposing the necessary experiments. I would like to underscore the use of kaons here, too. Even if it sounds a very long citation, conversely it looks like a deep and impressive work, with almost a whole tentative theory still introduced:

We show that, contrary to a rather widespread belief, relativistic invariance does not necessarily lead to angular-momentum conservation. Noticing that there is no empirical evidence for such a conservation in weak interactions, we show that a new classification of strange particles is possible in which the K-meson has spin $\frac{1}{2}$, the Λ and Σ have spin 1 and the Ξ has spin $\frac{3}{2}$. Our scheme can be proved or disproved with simple experiments.

It is usually thought that angular-momentum conservation is implied by relativistic invariance. If this were true one would not need direct experimental proofs of angular-momentum conservation, for the many existing verifications of relativistic invariance would then imply it. In fact we will show that it is not so, by considering a simple example. Pictorially we can describe our mechanism for J-violation as the emission of a particle with fixed spin, but with zero four-momentum (spurion¹⁶²). From the point of view of the remaining observable particles participating in the considered reaction, some angular momentum disappears, but strict conservation of energy and momentum is valid. Furthermore, the transition rate can be made an invariant essentially because the notion of zero four-momentum is relativistically invariant (Selleri 1968c, pag. 678. My underline).

¹⁶¹ Selleri F. (1968), “Relativistic Invariance without Angular-Momentum Conservation”, *Il Nuovo Cimento*, 57A, pp. 678-688.

¹⁶² “Spurion” appeared in a joint paper by H.P. Dürr and W. Heisenberg, published on *Zeitschrift für Naturforschung*, 16a, 726 (1961). In another joint paper published on *Il Nuovo Cimento* in June 1965, “On the ‘Spurion’ theory of strange particles”, they recalled their first work, stating: “The strange particles can be represented in the nonlinear spinor theory of elementary particles by making use of a degeneracy of the ground state with respect to isospin. ‘Spurions’ carrying isospin may then be detached from the ground state and attached to the nucleons or bosons to form the strange particles” (*Il Nuovo Cimento*, 37, 4, pag. 1447).

[...] The spins of some elementary particles have not been measured directly. But have been deduced, assuming angular-momentum conservation, from the total angular momentum of their decay products.

This is true for the strange particles, K-meson, Λ , Σ and Ξ hyperons.

If we suppose instead that angular-momentum conservation is violated in strange-particle decays with the rule $\Delta J = \frac{1}{2}$, we are led to the following possible new scheme:

- K has spin $\frac{1}{2}$,
- Λ has spin 1,
- Σ has spin 1,
- Ξ has spin $\frac{3}{2}$ (Selleri 1968c, pag. 680).
-

[...] In our scheme it would therefore be possible to keep the point of view that weak interactions of hadrons always violate $\mathbf{J}$, in addition to $\mathbf{T}$.

In our scheme the Gell-Mann-Nishijima formula [...] continues, of course, to be valid.

We wish next to show that the strangeness selection rules in strong, e.m. and weak interactions can easily be included in our model and that in some cases a better understanding of them is possible (Selleri 1968c, pag. 682).

[...] Direct measurements of the spins of the strange particles can easily prove or disprove our model. For instance, the K spin could be measured, apart from technical difficulties, in a Stern-Gerlach experiment, assuming, of course, that besides spin the kaon has also a magnetic moment (Selleri 1968c, pag. 685).

[...] Other experimental methods to detect a possible spin of the K-meson could consist of observations of asymmetries in the decay of polarized K beams. Such asymmetries were actually looked for (and not found) in the early days of strange-particle physics. This evidence is, however, not conclusive, firstly because one does not know if the beam he is studying is polarized or not, and secondly because most of the K decays probably lead to a $J=0$ final state which is isotropic anyway.

Furthermore, a demonstration of the type of statistics that K-mesons and hyperons obey would be strong indirect evidence for or against this scheme.

Our scheme leads to an entirely new point of view in particle physics. For instance, the quark model should probably be abandoned.

Perhaps the weakest point of our model lies in the success of SU_3 . It could happen, however, that this new point of view improves the understanding of SU_3 as well. For instance, the eight mesons π , η , K, K_{bar} could form an SU_3 multiplet in which the degeneracy is broken by the spin of K and K_{bar} , besides the well-known isospin breaking of the symmetry. This would indicate a connection between spin and hypercharge (Selleri 1968c, pag. 686. My underline).

[...] In conclusion our theory presents the following advantages and disadvantages on the traditional one:

ADVANTAGES:

- There is a greater parallelism between elementary particles and the excited states of atoms in which higher masses (i.e. lower binding energies) correspond to higher angular momenta.
- Once accepted the idea of J-violation it is nice to think of weak interactions as generated by some sort of $\mathbf{T} \cdot \mathbf{J}$ coupling.
- The $\Delta T = \frac{1}{2}$ rule become rigorous. The order of magnitude of the rate for the decay $K^+ \rightarrow \pi^+ \pi^0$ can easily be understood.
- Strangeness conservation in e.m. interactions is achieved without the minimality assumption, mainly is a consequence of J-conservation in photon interactions.
- The (rigorous) $\Delta S = 1$ rule becomes a consequence of the $\Delta T = \frac{1}{2}$ rule. So we have one selection rule less.
- The difficulties of interpretation of the decays $K_{\mu 3}$, $K_{e 3}$ are very probably eliminated.

DISADVANTAGES:

- As far as we can see, the only bad point of our scheme is in the successful predictions of SU_3 .

All together there seem to be many more positive than negative features in our proposal. Anyway, a good experiment will probably get rid of our scheme (Selleri 1968c, pagg. 686-687).

Part of this paper was prepared at the Istituto di Fisica Teorica of the Turin University. The author is very grateful to S. Fubini, E. Predazzi and M. Verde for the kind hospitality extended to him [...].

NOTE ADDED IN PROOFS

The difficulties connected with SU_3 are conceptually solved in the paper “The Physics of Spin- $\frac{1}{2}$ K-Mesons”, by the same author. In the same paper it is shown that our present empirical knowledge about the K-mesons generally does not contradict our scheme and in some cases seems even to support it (Selleri 1968c, pag. 687. My underline).

In these very sensitive months, Selleri experienced another period in USA, at the University of Notre Dame, Indiana. Having just started his activity at the University of Bari on 1 November 1967, Selleri received just one and a half months later the following letter, showing firstly Selleri's gladness for the new assignment:

Dear Franco,

It was a pleasure to receive your 14 December letter and to learn that the sun shining on Bari shines on you. We will change your address accordingly on our preprint list and bring you up to date on $\pi^\pm p$ interactions at 8 and 18 GeV/c. we have recently submitted a Letter on the $\rho^- \rho^0$ resonance at 1710 MeV; we also have written up the 3π ($\rho\pi?$) R, S and T resonance data that I was discussing with people at Heidelberg.

We would be most delighted to have you spend the summer with us at Notre Dame. I've discussed this with our department chairman, Charlie Mullin, and it would seem that a Visiting Professorship for 2-½ months at \$5,000 (including travel) could be arranged. If this were agreeable we would need only the usual "curriculum vitae" (with publications) and one letter of recommendation—from the "capo mafioso" or anyone else who is familiar with your work—to initiate the appointment.

[...] It Would be best if you could be here 1 June-15 August..

It might be possible to gather some people interested in this sort of physics for an informal "study week" while you are here.

Regards to your family. Sincerely (University of Notre Dame, Indiana-V. Paul Kenney to Selleri, December, 22nd, 1967-FSPA-My underline).

And later, two other letters to define his Visiting Professorship there in the period 1 June 1968-15 August 1968:

Dear Professor Selleri,

I am very glad to learn from Paul Kenney that you plan to come to Notre Dame this summer.

I should like to confirm that we shall offer you a Visiting Professorship with a stipend of \$5000 (including travel) for the period June 1-August 15, 1968... (University of Notre Dame, Indiana-Charles J. Mullin-Chairman to Selleri, January, 24st, 1968-FSPA).

Dear Professor Selleri:

We were happy to receive your recent letter in which you accepted the position as Visiting Professor in the Department of Physics for the period June 1, 1968 to August 15, 1968.

Once more I want to extend to you a warm welcome to the faculty of the University of Notre Dame...(University of Notre Dame, Indiana-Thomas E. Stewart-Associate-Vice President-Academic Affairs to Selleri, March, 14st, 1968-FSPA).

When he was in USA, he wrote another noteworthy paper, supported in part by the National Science Foundation, and concerning the angular-momentum nonconservation (later published when he came back to Bari),

namely: “The Physics of Spin- $\frac{1}{2}$ K-Mesons”¹⁶³, (Selleri PP 1969), which appeared on *Il Nuovo Cimento* dated 21 March 1969. The proposal managed to find several confirmations. I decided to quote almost the whole paper for the importance it played in a full understanding of Selleri’s theory.

A recently proposed model of elementary particles in which the K-meson has spin $\frac{1}{2}$ and decays, without conserving angular momentum, to states having $J=0$ and $J=1$ is compared with certain number of empirical facts. We discuss the definition of short- and long-lived K^0 ’s and show that the spontaneous decay $K_L \rightarrow \pi\pi$ can take place without violating CP-invariance. The ϕ -mesons are shown not to decay to $K_1^0 K_1^0$ as a consequence of C-invariance, as in the usual scheme, in agreement with experiments. For the same reason $p_{\text{bar}}p$ annihilations at rest into $K_1^0 K_1^0$ from the $^3S_1$ state are forbidden. We discuss the decay of several resonances which can go either to KK_{bar} or to $K\pi$ and show, in particular, that the $K^*(890)$ must be attributed $J=3/2$. A long-standing problem of interpretation of X-ray emission from K-mesic atoms is resolved in our model. The 1690 MeV maximum in the cross-section for the process $\pi^-p \rightarrow K^0\Lambda$ can be attributed to a P-wave final state while in the traditional scheme one has to attribute it, rather unnaturally, to D and F waves. We discuss the decay of the K-meson. We show, in particular, that $K_L^0 \rightarrow l^+l^-$ is CP-forbidden to the lowest order and that the present upper limits for the decays $K^+ \rightarrow \pi^+\gamma$, $\pi^+ l^+l^-$ are just above the expected rates.

In a recent paper (*Nuovo Cimento* 57, 678 1968) we proposed a model of elementary particles in which weak interactions of strange particles, and possibly all of the weak interactions, violate angular-momentum conservation. In this model one is led naturally to assume that the K-meson has spin $\frac{1}{2}$, Λ and Σ have spin 1 and Ξ has spin $3/2$. The spins of nonstrange particles remain unchanged. It was shown that several nice consequences arise from this point of view. Firstly, the rule $\Delta T = \frac{1}{2}$ can be considered as rigorous and the rules $\Delta S = 1$ for hadronic decays of strange particles and $\Delta S = \Delta Q$ for leptonic decays are straightforward consequences of the former rule; secondly, strangeness conservation in e.m. interactions can be understood as a consequence of angular-momentum conservation; thirdly, the phenomenological relationship between spin and isospin is very much simplified. In fact in the traditional scheme one had to state that particles with even strangeness (π , N, Ξ) have spin and isospin either both integer or half-integer, while particles with odd strangeness (K, Λ , Σ , Ω) have one integer and the other half-integer. In our scheme all particles have spin and isospin either simultaneously integer or simultaneously half-integer. As a consequence of these three important results strangeness disappears from the scene as a physical quantity.

¹⁶³ Selleri F. (1969), “The Physics of Spin- $\frac{1}{2}$ K-Mesons”, *Il Nuovo Cimento*, 60A, pp. 291-320.

Its role is merely to provide a labelling for the center of charge of isospin multiplets. This is certainly satisfactory since strangeness is the least understood of the particle quantum numbers (Selleri PP 1969, pagg. 291-292).

Against these and other nice features stands SU_3 . At first sight it seems impossible to accommodate particles with different spin in a multiplet of SU_3 . However, one can conceive a way out. Suppose, for instance, that SU_3 were an exact symmetry if the particles belonging to the same multiplet had the same internal quantum numbers (spin, parity), but that it cannot be exact because there are spin-dependent forces which vary from one element to another of the given multiplet. In this way one could attribute SU_3 breaking to the spin. But it is well known that such a breaking is attributable to hypercharge (M. Gell-Mann, report CTSL-20 unpublished, 1961). Therefore, our model is tenable only if we can show that spin and hypercharge are related. Going back to our classification, we discover that in fact there is such a relation [...] for the baryon octet and for the mesons π, η, K_{bar} [...] It will be shown that a similar relation holds for $\rho, \omega, \phi, K_{bar}^*$ (Selleri PP 1969, pagg. 292-293).

[...] These relations give faith in the possibility that SU_3 can be recovered in our model. Furthermore, it is clear that a symmetry of the SU_3 type can be obtained in our model as well, by assuming the existence of three quarks: a doublet with spin one-half and a singlet with spin one or zero (the author is indebted to L.M. Brown for pointing this out to him). The existence of octets and decuplets of particles with different spin would follow naturally in such a model. This important problem is however not discussed any further in the present paper. What we propose to do, rather, is to analyse experimental facts in particle physics from the viewpoint of our model and to see if anything is known which already excludes our classification. We anticipate that the answer to this question is negative and that, on the contrary, several phenomena can be better understood from our point of view. Therefore, we would like to urge the experimentalists to measure directly the spins of strange particles [...] to settle the matter of J conservation (Selleri PP 1969, pag. 293. My underline).

[...] Conclusions

In the present paper we have discussed several topics of K-meson physics and shown that there is essentially no evidence, in the arguments considered, against our proposal of angular-momentum nonconservation in weak interactions. Of course there are many more problems that should be considered, but limitations of time, space and energy suggested that one important, rich and thoroughly investigated area of particle physics be isolated and analysed. We did so with the physics of K-mesons, disregarding for the moment the hyperons and the leptons.

A priori it could be expected that our scheme could be ruled out by one, or several, of the following subjects: the K_L - K_S doublet, $p_{\text{bar}}p$ annihilations at rest, evidence related to mesons decaying to KK_{bar} or $K\pi$, K-mesic atoms, K-meson decays, strong reactions involving K-mesons and so on. In practice, [...] our model was often equivalent to the usual one and in several cases considerably more successful (Selleri PP 1969, pag. 316).

[...] the list of advantages of our scheme over the traditional one has become rather impressive. In the previous paper we reported the following:

- the rule $\Delta T = \frac{1}{2}$ becomes rigorous and we understand the rate for $K^+ \rightarrow \pi^+ \pi^0$.
- A relationship between isospin and angular momentum is suggested, since $\Delta J = \frac{1}{2}$ goes always together with $\Delta T = \frac{1}{2}$ and $\Delta J = 1$ with $\Delta T = 1$.
- Strangeness conservation in e.m. interactions is a consequence of J-conservation.
- The rule $\Delta S = 1$ becomes a consequence of $\Delta T = \frac{1}{2}$.

Now we can add the following points:

- all the particles have $2T$ and $2J$ either both even or both odd. This is further evidence for a relationship between isospin and angular momentum.
- Since also the rule $\Delta S = \Delta Q$ for leptonic decays of strange particles is a consequence of $\Delta T = \frac{1}{2}$ it follows from the latter three points that strangeness disappears altogether. This is hardly distressing since strangeness was always a very mysterious quantum number.
- It is no longer necessary to invoke CP-violation to explain the decay $K_L \rightarrow \pi^+ \pi^-$.
- We explain easily total and differential cross-sections for the process $\pi^- p \rightarrow K^0 \Lambda$ near threshold. In the usual scheme it was not possible to do so.
- We can easily accommodate a small nuclear shift of the K_a -line from K^- , ^4He atoms.
- A D-wave K^* (890) is better understood from the point of view of the peripheral model.
- We explain much better than the usual scheme the decay angular distribution of $f^0 \rightarrow K_1^0 K_1^0$.
- We predict that a $K_1^0 K_1^0$ decay of the resonances A_1 (1070) and E (1420) should be observed, in agreement with experiments.
- We should have no trouble in fitting the data for the K_{13}^+ and K_{13}^0 decay modes.
- The K-meson has spin $\frac{1}{2}$, the K^* spin $\frac{3}{2}$, Λ and Σ spin 1 and so on. The corresponding Regge trajectories lie therefore higher in the J - t plane than before. This certainly helps in understanding why the cross-section for strange particle production at high energy is not smaller than at low energy.

The above evidence is impressive enough and our scheme deserves at least one timid, but accurate, experimental verification.

Many experiments are possible. The most important ones are listed below:

- A Stern-Gerlach experiment with K-mesons and hyperons.
- A detailed balancing experiment, for instance with the processes $K^- d \leftrightarrow \Sigma^- p$.

- Observation of anisotropy in the decay of polarized K^+ to $\pi^+\pi^0$.
- Observation of a small ((5-10)%) asymmetry in the $\pi^+\pi^-$ decay of polarized K^0 's.
- Observation of hyperfine splitting in K-mesic atoms.
- Double scattering of K^+ , to detect a possible K^+ polarization.
- Observation of $K^+ \rightarrow \pi^+\gamma$.
- Difference in the angular distributions for the charged and uncharged KK_{bar} decays of the ϕ -meson.
- Observation of 1 keV X-rays associated with the decay $K_L \rightarrow \pi\pi$.
- Verification of the kind of statistics obeyed, for instance, by checking whether three lambdas can stay on the same nuclear level in hypernuclei created from Ω^- absorption.
- Confirmation of the present results about K_{13} decays.
- Confirmation of the energy and width of the K_a line from $K^-, {}^4\text{He}$ atoms.

Of these experiments the first seven are decisive pro or against our scheme. The last five can give strong support to our model, but some doubt about their interpretation could perhaps be raised. The important point, of course, is that there are experiments to decide. Apart from this *chi vivrà vedrà*¹⁶⁴.

Part of this research was carried out at the Department of Physics of the University of Notre Dame. The author is grateful to Drs. Mullin and Kenney for the kind hospitality extended to him.

Particular thanks are due to Dr. U. Amaldi for stressing the importance of some of the subjects discussed in the present paper. A suggestion by Dr. L.M. Brown was very illuminating. Stimulating correspondence with Drs. Che, Ferbel, C. Rubbia, E. Segrè and Streater is gratefully acknowledged. The author has benefited from discussions with Drs. Alees, Biswas, Burleson, DeCelles, Finocchiaro, Fitch, Kenney, McGlinn, Okubo, R.F. Peierls, J. Rosen, R.T. Torgerson and many other colleagues. He is very grateful to Dr. R.E. Lanou for reading the manuscript and for useful suggestions.

Note added in proofs

Since the paper was submitted for publication, several new facts have provided further evidence in favour of our proposal. They are: the KK_{bar} decay of the A_2 -mesons, the $K^0K^0_{\text{bar}}$ to K^+K^- ratio for ϕ -meson decay, the angular distribution for $\Lambda(2100) \rightarrow \Xi K^+$, the evidence for diffractive production of $K^*(890)$, the increase with energy of strange-particle production and the difficulties in understanding the properties of heavy hypernuclei in the traditional scheme. These facts will be discussed in a future publication (Selleri PP 1969, pagg. 317-319. My underline).

Selleri shows the knowledge of the seminal theory on Gell-Mann's quarks, addresses the details of the choices inherent in his theory proposal and in

¹⁶⁴ Only time will tell (in Italian in the text).

addition proposes experiments to test it. Moreover, Selleri dealt with the kaons physics, a key topic in his research. From the above paper it emerges how he used them since 1969, making their future use clear in his demand for experiments for the discrimination between QM and LR.

I decided to include Selleri's acknowledgements to put in evidence on the huge number of scholars who knew the work by Selleri on this topic.

In a email sent by Selleri to Michele Barone, dated 25 January 2001, in which he dealt with the asymmetric behaviour of the pion, Selleri recalled the importance of two of his old papers, i.e., "Relativistic invariance without angular momentum conservation", and "The physics of spin $\frac{1}{2}$ K mesons", but, above all, Selleri explained how "in these two articles we talk about the asymmetric behavior of strange particles starting from the K meson but I am sure that A DIFFERENT THEORY with the same conceptual basis could also fix the pion"¹⁶⁵.

I recalled this document because the asymmetry of the pion played a key role in the renewal of Selleri's activity in PP starting the 1983 until the early 2000s and concerning the neutral kaons.

Another paper on angular momentum nonconservation, jointly with O. Murro, with sound conclusions, is "The Multi-Regge Model and Strange-Particle Production"¹⁶⁶, (Murro, Selleri 1969), published on *Lettere al Nuovo Cimento*. Selleri cited his gratefulness to Prof. Cocconi for being able to share ideas with through various letters.

[...] for every strange particle or resonance with given quantum numbers there exists a corresponding nonstrange system with lower mass (Selleri 1969a, pag. 784).

We will show in the following that there is evidence in high-energy processes that exactly the opposite is true, namely that strange-particle production becomes more and more probable with increasing energy. A possible solution of this problem will be discussed (Selleri 1969a, pag. 785).

¹⁶⁵ "in questi due articoli si parla del comportamento asimmetrico delle particelle strane a partire dal mesone K ma sono sicuro che UNA DIVERSA TEORIA con le stesse basi concettuali potrebbe sistemare anche il pione" (Selleri to Barone, 25 January 2001, CAPS in the original text).

¹⁶⁶ Murro O., Selleri F. (1969), "The Multi-Regge Model and Strange-Particle Production", *Lettere al Nuovo Cimento*, 1, pp. 784-788.

Our results can be taken literally to imply that the multi-Regge model is not adequate to explain high-energy production of strange particles. The model of strange particles proposed by one of us gives however a higher spin to strange particles (higher by $\frac{1}{2} \hbar$) and thus a higher position of the corresponding Regge trajectories in the (J-t)-plane (Selleri 1969a, pag. 788).

Selleri wrote another paper on this topic, presented to Bari University and which remained unpublished, though: “Noninvariant Vacuum and Relativistic Quantum Field Theory”, (Selleri Unpublished 1969).

A vacuum state, noninvariant under Lorentz transformations, is introduced in relativistic quantum theory with indefinite metric. This leads to angular momentum nonconservation as far as the pieces of matter (particles...) are concerned. Poincaré invariance is however preserved.

By now it appears rather clearly that the K_1 decay into two pions is a manifestation of CP-nonconservation. Therefore the last attempt to preserve the vacuum symmetry under reflections, which consisted in replacing P with CP, has failed. A vacuum state invariant under rotations, but noninvariant under reflections is however very difficult to conceive as it was stressed particularly by Landau (Nucl. Phys. 3, 127 (1957)). In his words: “It might seem at first glance that non-conservation of parity implies asymmetry of space with respect to inversion. If, however, complete isotropy of space (conservation of angular momentum) is taken into account this type of asymmetry would seem to be extremely strange [...]”. It seems therefore important to study theories in which the vacuum state is noninvariant under the Lorentz group (Selleri Unpublished 1969, pag. 1-FSPA).

[...] in the present note we will show that the use of indefinite metric allows one to use the nonunitary representations of the Lorentz group which are finite dimensional. A simple theory is built in which transitions are possible between two different vacuum states, one with angular momentum zero and the other with angular momentum one half. Transitions between particle states with angular momenta differing by one half become then possible. The transition rates are however fully invariant under rotations and translations in Minkowski's space (Selleri Unpublished 1969, pag. 2-FSPA).

[...] it was observed in previous papers that no proof of angular momentum conservation in weak interactions was available. In the leptonic processes a neutrino enters and its spin is postulated to be $\frac{1}{2}$ in order to achieve J-conservation.

In the hadronic processes the spin of the strange particles has never been measured directly but deduced, assuming J-conservation, from the total angular momentum of their decay products. A new scheme is therefore possible in which the weak processes go, to the first order, according to $\Delta J = \frac{1}{2}$. This implies a change in the spin of the neutrinos and a new classification of strange particles. The phenomenology of elementary particles can be reinterpreted and in some cases better understood by assuming that the K-meson has spin $\frac{1}{2}$, Λ and Σ spin 1 and so on (Selleri Unpublished 1969, pag. 10-FSPA).

Regarding Landau words in these excerpts, Selleri, speaking on angular momentum nonconservation, was considering an anisotropy of space, a related topic we are going to deal with on the segment about his studies on FRT, with the absolute reference frame.

1.10-THE FIRST TURNING POINT: THE ANGULAR-MOMENTUM NONCONSERVATION. POSSIBLE REASONS FOR CHANGING THE COURSE OF HIS RESEARCH.

Soon after the publishing of his paper on angular-momentum nonconservation, “The Physics of Spin- $\frac{1}{2}$ K-Mesons”, Selleri published his first paper on FQM, “On the wave function of quantum mechanics” in *Lettere al Nuovo Cimento*. I am going to explain more in depth the details of this paper further in this study, in Chapter two. It is essential to understand why he decided to change the area of his research. Selleri still continued to publish remarkable papers on angular-momentum nonconservation as well on Regge poles even after his first paper on FQM, though. He wrote a joint paper again with O. Murro, “Two-Body Processes and Regge-Pole Model”¹⁶⁷, (Murro, Selleri 1969a), published on *Lettere al Nuovo Cimento*, and “Angular Momentum Nonconservation Scheme”¹⁶⁸, (Selleri 1970), in a book by *Plenum Press* related to the Proceedings of the Third International Conference on High Energy Physics and Nuclear Structure sponsored by the International Union of Pure and Applied Physics, held at Columbia

¹⁶⁷ Murro O., Selleri F. (1969), “Two-Body Processes and Regge-Pole Model”, *Lettere al Nuovo Cimento*, 2, pp. 863-869.

¹⁶⁸ Selleri F. (1970), “Angular Momentum Nonconservation Scheme”, in *High-Energy Physics and Nuclear Structure. Proceedings of the Third International Conference on High Energy Physics and Nuclear Structure sponsored by the International Union of Pure and Applied Physics, held at Columbia University, New York City, September 8–12, 1969*, Devons S. (ed), New York: Plenum Press, pp. 751-758.

University, New York City, September 8–12, 1969. Selleri in such paper claimed that:

As I have shown in two recent papers (“Relativistic Invariance without Angular-Momentum Conservation”; “The Physics of Spin- $\frac{1}{2}$ K-Mesons”), the phenomenology of elementary particles can be interpreted and, in some cases, better understood by attributing to the strange particles spin values higher than the traditional ones. Thus the K-meson has spin $\frac{1}{2}$, Λ and Σ become spin 1 bosons, K^* (890) has spin $\frac{3}{2}$, and so on.

This scheme is possible because the spin of the strange particles has never been measured directly, but always inferred from the total angular momentum of their decay products, assuming angular momentum conservation in the weak decay. Direct measurement of the spin would be possible. If angular momentum is instead non-conserved during the decay process and the violation shows some kind of regularity (for instance, by taking place according to a $\Delta J = \frac{1}{2}$ rule) a new attribution of the spin-values is possible.

A very important question which has to be answered is whether angular momentum nonconservation can be compatible with Lorentz-invariance. This question was answered positively in the first paper by studying the spurion model according to which the missing angular momentum is carried away by an unobserved spin- $\frac{1}{2}$ “particle” with zero four-momentum. In the present note I will try and answer again the same question in a more complete manner by using a noninvariant vacuum and the indefinite metric. The latter ingredient is important in order to be able to work with the finite-dimensional representations of the Lorentz group, which are non-unitary (Selleri 1970, pag. 751).

I mentioned all the previous papers on angular momentum nonconservation because this topic proved to be crucial for Selleri’s switch from PP to FQM. Firstly, I would like to expose some reactions to his foregoing six papers. Some essential letters that I found in his Private Archive explain so well how much interest was created on PP community by Selleri’s research on this topic.

For this purpose, I enclosed here first a letter by Vittorio Silvestrini at Frascati National Laboratories (FNL), where the peculiarity of the invitation made to Selleri to spend a period at the FNL is well emphasized:

Dear Franco, I am pleased with your intention to spend June and July [1969] in Frascati¹⁶⁹. I told Touschek and Querzoli about it and, like the rest of us, they will both be glad to have you.

This is unprecedented in Frascati (but it won't be the last time: I really hope that also Cabibbo will come when you are here) and so Querzoli will have to solve some bureaucratic difficulties. Even if he can not give you any formal assurance for now, the impression is that these difficulties can certainly be overcome.

[...] I really hope your idea keeps going all the way through¹⁷⁰ (Vittorio Silvestrini-Cnen-Frascati National Laboratories to Selleri, September 14th, 1968-FSPA. My underline).

Silvestrini was surely showing interest and appreciation for Selleri's idea on the angular-momentum nonconservation, both because Touschek was working on the particles and because Selleri's stay at Frascati on June and July 1969 included a 30 lessons refresher course on topics related to elementary particle physics. Another remarkable letter was that by Carlo Rubbia¹⁷¹ to Selleri. Rubbia was favourable towards the theory, still proposing to apply it to the experiments:

I have just seen your paper on the nonconservation of angular momentum. I would like to understand a little better in which way you think to describe the states $K_1 \rightarrow K_2$ within the framework of your theory.

Clearly, although the effects due to the presence of spin are rather small, it should be noted that the sensitivity of current experiments is extremely high. The CP violation especially corresponds to terms about 10^{-3} times the mass difference. In certain circumstances effects of the order of 10% of the CP violation are detectable.

¹⁶⁹ Concerning the relationship between Selleri and the FNL, it should be underlined that he also went there:
- in September 1969, for some impressive and far-reaching lectures on FQM (as I will exhibit in Chapter 2);
- on 4-6 June 1974, for a national conference on *Mutamenti e prospettive della meccanica fonamentale: I fondamenti della MQ*.

¹⁷⁰ "Caro Franco, sono contento della tua intenzione di passare giugno e luglio [1969] a Frascati. Ho parlato della cosa a Touschek e a Querzoli ed entrambi, come tutti noi, saranno contenti di averti. La cosa non ha precedenti a Frascati (ma non sarà l'ultima: spero anzi che anche Cabibbo sarà qui quando ci sarai tu) e quindi Querzoli dovrà risolvere alcune difficoltà burocratiche. Anche se non può per ora darti alcuna assicurazione formale, l'impressione è che queste difficoltà potranno senz'altro essere superate. [...] Spero veramente che la tua idea continui a camminare fino in fondo" (Vittorio Silvestrini-Cnen-Frascati National Laboratories to Selleri, September 14th, 1968-FSPA. My underline).

¹⁷¹ Carlo Rubbia (1934-), the fourth of five Italian Nobel Prize in Physics (Guglielmo Marconi 1909, Enrico Fermi 1938, Emilio Segrè 1959, and Riccardo Giacconi 2002) was awarded the Nobel Prize in Physics in 1984 with Simon van der Meer, "for their decisive contributions to the large project, which led to the discovery of the field particles W and Z, communicators of weak interaction":
<https://www.nobelprize.org/prizes/physics/1984/rubbia/facts/>. Accessed 24 September 2020. See also <https://home.cern/biographies/carlo-rubbia-b1934>. Accessed 24 September 2020.

Moreover, the phenomenon of coherent regeneration and p scattering is in optimal agreement with the theory. If the K_0 and the $K_{0\text{bar}}$ had spins, it is likely that regeneration effects due to spin orbit couplings would be detectable.

I therefore believe that it would be worth reviewing further the experimental work in this field from the point of view of your theory¹⁷² (Carlo Rubbia to Selleri, June, 5th, 1968-FSPA. My underline).

Then, here is a letter sent by Emilio Segrè¹⁷³ to Selleri. Segrè gave some credit to the theory by pointing out the impossibility of finding evidence to the contrary, even though he was not very convinced. And still leaving the door open for an analysis of the theory itself:

We have read with considerable interest your paper where you want to give spin $\frac{1}{2}$ to the K meson. Min Chen is intrigued by your hypothesis, and has written you a letter sometime ago in which he points out the difficulty that it would entail for his measurements of the radiative decay of the K . He has never received an answer from you and I wonder whether you have given any thought to it. We have been trying to find arguments against spin $\frac{1}{2}$ of the K meson. We were considering the radiative and non-radiative decay of hyperons, but as of today we have not concluded anything on it. I personally am somewhat skeptical on the subject but I believe one should think about it. I will be in Italy in the fall and maybe we might talk about it in Rome (Emilio Segrè to Selleri, July, 15th, 1968-FSPA. My underline).

In his answer to Segrè¹⁷⁴, needless to say, Selleri expressed conviction in the soundness of his theory. He also asked for an experiment to be performed in Wien, really pivotal for his theory, claiming that also in Bari some experiments and researches were being performed at the time:

¹⁷² “Ho appena visto il tuo lavoro sulla non-conservazione del momento angolare. Mi piacerebbe capire un po’ meglio in quale maniera pensi descrivere gli stati $K_1 \rightarrow K_2$ nel quadro della tua teoria. Infatti quantunque, come fai notare, gli effetti dovuti alla presenza dello spin sono alquanto piccoli, va tenuto presente che la sensibilità degli esperimenti attuali è estremamente elevata. La violazione di CP in particolare corrisponde a termini circa 10^{-3} volte la differenza di massa. In certe circostanze effetti dello ordine del 10% della violazione CP sono rivelabili. Inoltre il fenomeno di rigenerazione coerente e p scattering è in ottimo accordo con la teoria. Se il K_0 e il $K_{0\text{bar}}$ avessero spin, è probabile che effetti di rigenerazione dovuti ad accoppiamenti spin orbita sarebbero rivelabili. Credo quindi che varrebbe la pena di rivedere più a fondo il lavoro sperimentale in questo campo dal punto di vista della tua teoria” (Carlo Rubbia to Selleri, June, 5th, 1968-FSPA. My underline).

¹⁷³ Emilio Segrè (1905-1989). He was awarded the Nobel Prize in Physics in 1959 together with Owen Chamberlain, “for their discovery of the antiproton”:

<https://www.nobelprize.org/prizes/physics/1959/segre/facts/>. Accessed 24 September 2020.

¹⁷⁴ The original Italian text is in the Addendum 3, p. 956.

Dear Prof. Segrè, I am writing in order to ask you for information about the experiment $K^+ \rightarrow \pi^+\gamma$ we talked about in Wien. This experiment will be decisive for the life or for the death of my theory (and more likely for the second one!) and of course I look forward to the result with suspense. $K^+ \rightarrow \pi + \gamma$ -type decays have been searched here in Bari by Sergio Natali in his X2 experiment film. On the basis of $\Gamma(K^+ \rightarrow \pi^+\gamma) \approx 5 \cdot 10^{-4}$ he waited to find an event and a half and found none. Clearly the result is not very significant. About the value of $\Gamma(K^+ \rightarrow \pi^+\gamma)$ I had an epistolary discussion with Min Chen, but I do not think I was able to convince him that my estimation is correct. His argument is based on a perturbative diagram with a virtual K and he suffers from three serious weaknesses:

- 1) It is not gauge-invariant;
- 2) It requires prior knowledge of the coupling constant $K \pi$ which is significantly different depending on whether it is extracted from $K \rightarrow 3\pi$, $\Lambda \rightarrow p\pi$ or from other processes;
- 3) It is perturbative in the pre-dispersive sense and therefore, strongly doubtful (think for example of perturbative predictions for low energy πN interaction).

My estimate attributes $K^+ \rightarrow \pi^+\gamma$ to the same basic transition that permits $K^+ \rightarrow \pi + \pi^0$ and assumes the denomination of a meson ρ . Therefore, it is up to date with the dispersion relations and it is gauge-invariant. I also need a coupling constant (the one related to $P \rightarrow \pi\gamma$), but this is theoretically appraisable in a precise way (Weinberg-Proceedings of Vienna) because it depends on $P \rightarrow \pi + w$ (obtainable from $w + \pi \pi \pi$) followed by $w + \gamma$ (obtainable from $w \rightarrow l^+l^-$ or from the electromagnetic form factors of the nucleon).

My theory does very well in very different fields, such as the splitting of the A_2 also in the KK_{bar} channel and the increasingly probable production of KK_{bar} pairs at high energies and in cosmic rays. Furthermore, the successes [already] discussed in my work still remain. Finally, given the general situation of elementary particle physics, it seems to me that a revolution is desirable and probable. If it is not this one it will be another, but something big will have to happen!

I would be infinitely grateful if you could write me two lines about the current state of the experiment $K^+ \rightarrow \pi^+\gamma$ (Selleri to Segrè, January, 8th, 1969-FSPA. My underline).

Another bit of evidence came from a letter by John Stewart Bell, who along with Lehmann wanted to test Selleri's ideas. Consequently, Bell asked about Selleri's thoughts on some scepticism he felt regarding the topic of locality.

Dear Dr. Selleri,

Dr. Lehmann, who is now here, became interested in the possibility of testing your ideas on angular momentum conservation.

In a preliminary discussion he asked me if I thought the theory Lorentz invariant, and it seems to me that it is not-in a sense that I will describe.

A general reason for expecting this is that a rotation can be expressed as a commutator of boost, so boost invariance implies rotation invariance.

More explicitly, as I understand your proposal, you replace the conventional invariant amplitude for $K \rightarrow (\text{??})$ by a form $\bar{\Psi} U$, where U is the Dirac spinor for your spin-1/2 kaon, and $(\bar{\Psi})$ is a fixed “vacuum” or “spurion” spinor. The square of this averaged over kaon spin is $\text{Trace } \bar{\Psi} \Psi_{\text{bar}} (m - i\gamma k) = m \bar{\Psi}_{\text{bar}} \Psi - k_{\mu} \bar{\Psi}_{\text{bar}} i\gamma_{\mu} \Psi = m \bar{\Psi}_{\text{bar}} \Psi - k_{\text{vett}} \Psi^{\dagger} \alpha_{\text{vett}} \Psi + k_0 \Psi^{\dagger} \Psi$.

The second and third term here introduce a quite novel dependence of decay rate on direction and velocity of motion of the particle, the Ψ being independent of the state of the particle, unless $\Psi^{\dagger} \alpha_{\text{vett}} \Psi = \Psi^{\dagger} \Psi = 0$, i.e., $\Psi=0$, so that there is no decay.

I am also somewhat concerned about locality in our theory. It seems to me that a decay in one region of space modifies the vacuum (emission or absorption of spurion) over the whole universe, and so changes the conditions for decays in other regions of space. What do you think of these remarks? (John S. Bell to Selleri, February 2nd, 1970-FSPA).

Bell seemed to notice how Selleri’s theory implies a nonlocality. It sounds ironic, because Selleri’s ideas on locality will rule his approach to FQM.

More evidence about this issue comes from a paper that the Italian Physicist Francesco Guerra¹⁷⁵ sent to the 37^o SISFA¹⁷⁶ Annual Conference, held in Bari on 26-29 September 2017¹⁷⁷. I think it should be shared in order to illustrate the great expectation many people had on this matter.

Franco Selleri, a high-class theoretical physicist, has been able to give contributions at the highest level in the most advanced fields of theoretical physics of elementary particles, since his early years. His “peripheral” model, which interprets a relevant part of the proton-proton interaction as due to the exchange of a single meson, immediately found multiple applications, particularly in the production of mesons in the diffractive diffusion, in a series of works carried out in prestigious collaborations, with Ugo Amaldi [Jr.], Francesco Bonsignori, and Ezio Ferrari [...]. In the second half of the 1960s, his proposal concerning the nonconservation of angular momentum would have led to a better understanding of some peculiar aspects of physics of a set of elementary “strange” particles. The proposal was initially accepted with great interest. I remember that in Naples we discussed it with great interest.

¹⁷⁵ Francesco Guerra (1942-): <http://www.icra.it/irapphd/CV/Guerra-CV.pdf>; and https://en.wikipedia.org/wiki/Francesco_Guerra. Both accessed 24 September 2020.

¹⁷⁶ Guerra F. (2017), “Franco Selleri: the courage of ideas”, *Proceedings of the 37^o SISFA Annual Conference, Bari 26-29/09/2017*, Campanile B., De Frenza L., Garuccio A. (eds), pagg. 301-306.

¹⁷⁷ The original Italian text is in the Addendum 3, p. 957.

[...] The proposal by Selleri was physically well founded. The missing angular momentum would have been absorbed by a new form of excitation of the vacuum, called spurion, devoid of energy and momentum, but endowed with angular momentum. Spurion had a noble origin, even proposed by Heisenberg. Unfortunately, Nature had decided not to use this opportunity. The Non-conservation of the angular momentum proved contrary to the experimental evidence, and the proposal fell (Guerra F., “Franco Selleri: the courage of ideas”, paper presented at the 37° SISFA National Congress, Bari: 28 September 2017. My underline).

In my interview with Guerra, still stressing the above-mentioned concept of angular momentum nonconservation, he claimed that:

Actually, one of my most brilliant graduating students even decided to leave me and to work on his thesis with Selleri instead, due to this very issue of nonconservation of the angular momentum (Interview with Francesco Guerra, Rome, 9 December 2017).

It should be said that Selleri also attended an international conference held in September 1969 (three months after his first FQM paper came out) in New York (see page 112 above), talking a lecture about the “Angular momentum nonconservation scheme”. The following year he published a paper by the same title.

So, what had really happened? Why did he decide to move on to FQM?

A decisive role in this switch may have been played by the achievements of Murray Gell-Mann (1929-2019) and by the development of quarks¹⁷⁸:

In 1961 Murray Gell-Mann exploits the patterns of particles of similar mass and spin, but differing charge and strangeness to create a classification scheme [based on the group SU(3)] now called flavor symmetry, for the ever-increasing number of known particles. The scheme predicts a new particle type, the Ω , whose discovery shortly thereafter gives great validity to this idea.

For his earlier work on strangeness, this classification scheme, and the later work on quarks in 1964, Gell-Mann was awarded the Nobel Prize in physics in 1969¹⁷⁹ (abyss.uoregon).

¹⁷⁸ Murray Gell-Mann (1929-2019): <https://www.nobelprize.org/prizes/physics/1969/gell-mann/biographical/>; <https://www.nytimes.com/2019/05/24/obituaries/murray-gell-mann-died-.html>. Both accessed 24 September 2020.

Concerning the origin of the introduction of quarks it is well known that Enrico Fermi was working in Chicago on the diffusion of the meson π , considering the pion as a nucleon-antinucleon bound state. His students count many of the great future scientists, including Gell-Mann himself. In the paper by Maurice Jacob and Luciano Maiani, *L'eredità di Enrico Fermi nella fisica delle particelle*¹⁸⁰ it is well stated the path behind the researches and the difficulties on predictions on strong interactions:

Among Fermi's discoveries [...] we mention [...] the theoretical and experimental study of the pion-nucleon diffusion with the validation of the independence from the charge, or isospin invariance, [and] the manifestation of the famous 3-3 resonance (Jacob, Maiani, pag. 243).

[...] By the end of the 1950s it was already clear that the nucleon was an extended object (with dimensions of about 10^{-15} m) and with some more internal structure still to be explored. The many new particles discovered in cosmic rays also showed that the list of fundamental particles could not be limited to the few involved in the structure of the atom (Jacob, Maiani, pag. 245).

[...] [By the end of the 1940s] the theorists found themselves in great difficulty in formulating verifiable predictions for strong interactions. [...] The theory of perturbations [...] miserably failed with the strong coupling constant of meson theories (Jacob, Maiani, pag. 256).

[...] In their effort to reduce the number of fundamental particles, Fermi and Yang were led to assume a point interaction (of the Fermi type) between the constituents of the π -meson thus avoiding the introduction of quanta of an inter-nucleon field. This hypothesis was later abandoned by Gell-Mann in favor of a vector force.

The elementary quanta of these fields have been called "gluons" because of their role of "gluing" the elementary constituents together.

We know today that the interactions of the constituents show a "hidden" symmetry, called color, which plays a fundamental role in banning the fermion-antifermion bound states and allowing the fermion-antifermion states. Consequently, there is an entire octet of gluons that are related to each other by symmetry.

¹⁷⁹ The Nobel Prize in Physics 1969 was awarded to Murray Gell-Mann "for his contributions and discoveries concerning the classification of elementary particles and their interactions" (MLA style: The Nobel Prize in Physics 1969. [NobelPrize.org](https://www.nobelprize.org/prizes/physics/1969/summary). Nobel Media AB 2019. Wed. 30 Oct 2019. Accessed 24 September 2020. <https://www.nobelprize.org/prizes/physics/1969/summary>). Accessed 24 September 2020.

¹⁸⁰ Jacob M., Maiani L. (2001), "L'eredità di Enrico Fermi nella fisica delle particelle", in *Conoscere Fermi nel centenario della nascita 29 settembre 1901-2001*, Bologna:SIF, pagg. 242-273. (Jacob, Maiani 2001) in the following.

Sakata introduced into the picture the hyperon Λ , which brought the negative unit of strangeness, and extended the Fermi-Yang model to strange particles. What did not work in Sakata's model, and which greatly jeopardized the affirmation of this idea in the late 50s, was the structure of the strange baryons, particles with spin $1/2$, which weakly decay into a nucleon and a pion, discovered in the meantime (sometimes also called hyperons) (Jacob, Maiani, pag. 257).

[...] A crucial step forward was Gell-Mann and Ne'eman's "eightfold way" scheme, based on SU_3 symmetry, and the unitary transformations in a complex three-dimensional space. This symmetry extends Heisenberg's isospin symmetry (SU_2) to strange particles.

In the Fermi-Yang-Sakata model this symmetry is natural because there are three basic constituents, and the SU_3 transformations are nothing more than the unit transformations of P, N, and Λ among them (the fermions are represented by complex fields, and their three-dimensional space is consequently complex).

However, the idea was to put the symmetry on a more abstract basis: assuming its existence and trying to classify the particles according to multiplets of SU_3 [...]. Surprisingly, it was found that mesons adapted to the octet and singlet representations of SU_3 (as in the triplet model) and that known baryons [and this only happened with the eightfold way] could be arranged in octets and decuplets: the latter is a representation of SU_3 never considered before.

[...] The $3/2$ spin resonance decuplet was incomplete when the eightfold way was proposed. The discovery of the missing particle, the Ω , which possessed exactly the predicted properties, convinced physicists that they were on the right track!

[...] Although successfully proven, the eightfold way did not answer the fundamental question: should these particles, baryons and mesons, all be considered elementary? The next step is due to Gell-Mann and Zweig, who independently discovered that the structure of mesons and baryons could be reproduced by assuming them as bound states of a fundamental triplet of spin fermions $1/2$, the quarks, called u, d, and s.

The mesons are bound states made by quark and antiquark, while the baryons are made of three quarks: the first statement requires that the charge of u is greater than one unit of the electrical charge of d and s (similar to the triplet of Fermi-Yang-Sakata). However, in order to reproduce the baryons, the quarks must have fractional electrical charges [...], with $Q(u) = 2/3$, $Q(d)=Q(s) = -1/3$ (Jacob, Maiani, pag. 258).

In the mid-1960s, the idea of quarks as constituents of known hadrons was gaining credit. [...] However, and as just said, it was only with the advent of the quark model that Fermi's theory found its completion.

A π^+ is made of a u-quark and an anti d-quark. Calculating at the quark level with a good level of accuracy is still often impossible today (Jacob, Maiani 2001, pag. 259)¹⁸¹.

¹⁸¹ The Italian original text is in Addendum 3, pp. 957-958.

Another key in this inquiry into the reasons for Selleri's changeover to FQM is the one proposed by Francesco Guerra during the abovementioned interview. I deliberately asked him if he thought the switch to FQM was related to the failure of the angular momentum nonconservation theory. This was his response:

I cannot tell you that. He was certainly ripe for a professorship in the late '60s that he did not win. The Italian academic environment is typically reserved in theoretical physics, to very high level circumscribed groups, which are really powerful. Take Gatto, Cabibbo, Fubini: they are all outstanding theorists in Italy and yet I do not think they accepted the angular momentum issue properly (Interview with Francesco Guerra, Rome, 9 December 2017).

But maybe the main evidence for Selleri's switch from PP to FQM can be found in the remarkable recollections by Michele Barone (who informally speaks in the following about Selleri using his christian name, Franco):

In 1968 Franco had hypothesized the violation of the conservation of the angular momentum (spin) in the case of the decay of the meson K (with an asymmetric distribution $K \rightarrow \pi^+ \pi^0$) and of the hyperons Λ , Σ , Ξ and grouped these violations in a model of elementary particles alternative to that of the quarks and based on the similarity with excited atoms in which high energies correspond to high values of the angular momentum.

He made a model to explain the physics of the K mesons with spin $\frac{1}{2}$ based on the violation of the conservation of the angular momentum in the decay of the kaons by the weak way. This model, which competed with SU(3), explained the strange mesons produced until then better than SU(3).

Neutrino oscillation violates the law of leptonic number conservation.

Franco measured in the reaction $p + p \rightarrow d + \pi^+$ that the π^+ had $S=0$, but since $p + p \rightarrow d + \pi^+ \rightarrow \mu + \nu$ (whose total spin S is supposed to be zero by the V-A theorem) then the sum $T+J$ (isotopic momentum + angular momentum) is zero, so since this sum is not preserved, the spin of the neutrino should be the opposite of what is observed ($+ 1/2$) in order to have the conservation.

In Franco's model this contradiction and also the problem of CP violation in the decay of K was solved.

So the common point resided in a vision of the structure of the elementary particles different from that accredited by the mainstream.

One of the experimental tests that most would have proved the validity of the model was the observation of the anisotropic behavior of K^+ decay in $\pi^+\pi^0$. This had to be measured by observing an asymmetry of 5-10% in the decay of $\pi^+\pi^-$ of polarized K^0 . So the proposed experiment could prove Franco right or wrong.

Romano and Armenise were working at the bubble chamber in Bari in a spin-off from Bologna. In my opinion this is also why Franco came to Bari from Bologna. For the chance of a chair in Bari, certainly. But maybe also for the opportunity of carrying out an experiment with them.

[...] When we saw Franco in the following years, I represented the anchor, the link with the world of elementary particles. So we would talk about the Higgs boson, the mass of the neutrino and so forth. He was always following these things. He had not overlooked or forgotten them.

Franco told me that he esteemed Luciano Maiani, who, by the way, had been director of CERN. Franco told me this: “When I published that article on the violation of the angular momentum [Maiani] told me that I was following an interesting line of development and urged me to continue on that path”.

I figured out later why he went down that way. Franco was perhaps aiming for the Nobel Prize, as the family (to whom he had confided this opportunity) revealed to me later. But Franco abandoned that road, because Gell-Mann’s octet came out, and the same Gell-Mann won the Nobel Prize in 1969. Franco knew Gell-Mann, he had worked with him.

The octet theory had been validated by the discovery of the Ω^- and therefore had become successful. Elementary particle physics, at least in theory, evolved on the basis of group theory. Franco had seen that in this way his idea (that the angular momentum is something related to elementary particles which are still connected) ended up fading away, because the group classification had taken over. And group theory invalidated the non-preservation of the angular momentum, too. I think that also contributed to its abandonment of PP. That is what he told me in the last few years of his life.

[...] To sum up, Franco then did the peripheral model. He was against quarks and the spurion reorganized the particles discovered so far in a different way from the quarks. Gell-Mann however was awarded the Nobel Prize with Ω^- . Franco realized that physics had chosen another path and his model based on spurion would no longer be accepted. Then there was also the negative result with the angular momentum experiment done in Bari (Interview with Michele Barone, Athens, 18 June 2019. My underline).

The last statement revealed by Barone opens up some substantial prospects that require a major integration, concerning several topic, last but not least, that related to the reasons for Selleri’s relocation to Bari!

I am planning to carry out an investigation in the Physics Department of Bari, in the near future, to look into the matter concerning this experiment, accepting the fact that some of the protagonists are deceased.

In confirmation of what I said about the need to investigate further, I found in FSPA a letter sent to Selleri by Adalberto Piazzoli¹⁸², and regarding an experiment proposed by Selleri himself on fluorescence:

Dear Selleri,

Every now and then we discuss among ourselves the feasibility of the fluorescence experiment.

We have collected some technical and experimental data and we decided to write to you for more details about your proposed experiment. We do not mean theoretical details (for now) but experimental, or rather information regarding the drafting of a first project of experience. Since this is a completely new kind of experiment, we can not even ask you very precise questions. Anyway I will try some:

- a) Assuming that the experiment is negative, to what extent does it make sense to push the upper limit of the percentage change in average life?
- b) do you think it is more convenient to measure a long or short average life?
- d) did you mention the fluorescence phenomenon at random? Do you think you can also use the shut-off time of a laser, for example?

In short, we would like to understand the essential requirements of the experiment, also because we would have found an electronic expert in very accurate measurements of width of spectroscopic lines and things like that.

If you are kind enough to write to us on this subject, I think we could try to do a project of experiences¹⁸³ (Adalberto Piazzoli to Selleri, 1 April 1970-FSPA).

I could not manage to discover which kind of experiment was, even if, looking at some statement, I suppose it could be referred to the opportunity to reveal an anisotropy in the behaviour of pions emerging by polarized kaons, within his theory on the nonconservation of angular momentum. But it is just my speculation.

¹⁸² Adalberto Piazzoli (1937-): He researched in the field of elementary particle physics, mainly on resonances, photoproduction of pions, $e^+ e^-$ at Stanford, neutron oscillation at Pavia and Grenoble, Icarus project with Rubbia (currently at Fermilab for the sterile neutrino) and so on. Currently he is Professor Emeritus at the University of Pavia (Personal communication to the author, 20 September 2020).

¹⁸³ The original Italian text is in Addendum 3, pp. 958-959.

In order to test it, I sent an email to Prof. Piazzoli asking for his recollection as well as some confirmation on the above idea. He answered me very politely and with a great availability. I had the opportunity to ask him several questions. In the following I mention his answers. Basically, Piazzoli told me that my speculation was correct, even though he described the fluorescence time of neutrinos, actually. According to Piazzoli,

Selleri was a heretic, it was well known, and he had thought about this experiment, but it remained only at the level of dialogue and nothing more, nothing was ever developed. Selleri also gave a value to Ψ , not only to Ψ^2 . An example that comes to mind to describe Selleri's proposal is to want to shake a carpet under an atomic building. In Pavia we have a tradition of particle physicists and solidists, experts in fluorescence. And there is also the only working reactor in Italy that obviously only produces neutrino beams. The idea was to measure the fluorescence time and then reassess it in the reactor room, with values of the order of 10^{16} neutrinos/s cm^2 . If an effect had been seen, it would not have been to be attributed to Ψ^2 , to the neutrino, whose impact section is 10^{-44} but to Ψ . As said, nothing has been done, I cannot compare places with and without neutrinos. Besides, why use the reactor when the Sun already sends $6 \cdot 10^{10}$ neutrinos/s cm^2 ? I remember a seminar in Frascati by Selleri, quite theatrical. He gave the wrong spin values and the audience was a bit embarrassed. Actually, the strange situation was linked to his proposal of the theory of the nonconservation of the angular momentum. Among other things, he explained how the nonconservation of parity led to the nonconservation of the angular momentum. It seemed to me to be a bit of a stretch¹⁸⁴ (Prof. Adalberto Piazzoli-Personal communication to the author, 2 September 2020. My underline).

There is also an evidence by Michele Barone to his son Gaetano, in which the former recalled to the latter on a possible experiment:

Franco in 1968 had made a model to explain the physics of K mesons at spin $\frac{1}{2}$ based on the violation of the conservation of the angular momentum in the decay of the K due to the weak way. This model, which competed with SU_3 , explained better than SU_3 the strange mesons produced until then. One of the experimental tests that would have proved the validity of the model consisted in the observation of the anisotropic behavior of K^+ decay in $\pi^+\pi^0$. This was to be measured by observing an asymmetry of 5-10% in the decay of $\pi^+\pi^-$ of polarized K_0 . So the proposed experiment can give right or wrong to Franco. I have the articles to which Franco refers:

¹⁸⁴ The original Italian text is in Addendum 3, p. 959.

Relativistic invariance without angular momentum conservation 1968

The physics of spin $\frac{1}{2}$ K mesons 1969¹⁸⁵ (Michele Barone to Gaetano Barone, date unknown, maybe after 26 January 2011-MBPA).

In some personal notes, Michele Barone dealt with the model of angular momentum nonconservation, stating that¹⁸⁶:

Since π^+ has spin zero it should not be sensitive to the influence of an external magnetic field, so the μ^+ distribution should be symmetrical. Instead many experiments detect an anisotropy due to an excess of muons in the opposite direction to that of the incident proton that produced the pion.

[...] Since in the past many other experiments had detected a certain asymmetry, it was proposed [also following the results obtained by Gaetano Barone] to make at Paul Scherrer Institute an *ad hoc* experiment that was refused [using the excuse of systematic errors and also the monochromatic non-guaranteed beam]. Theoretically, if this asymmetry were confirmed, the pion would no longer be regarded as a pseudo-scalar quantity that has no information about the direction of propagation but as a vector, as Yukawa had hypothesized.

In 1968 Franco had suggested the violation of the conservation of the angular momentum (spin) in the case of the decay of the meson K (with an asymmetric distribution $K \rightarrow \pi^+ \pi^0$) and of the hyperons Λ , Σ , Ξ and framed this violation in an alternative model of the elementary particles to that of the quarks and based on the similarity with the excited atoms where high masses correspond to high values of the angular momentum.

[...] According to Franco, the reaction $p + p \rightarrow d + \pi^+$ measured that π^+ has $S=0$ but since $p + p \rightarrow d + \pi^+ \rightarrow \mu + \nu$ (whose total S is supposed to be zero by the theorem V-A) then the sum $T+J$ (isotopic momentum + angular momentum) is zero, so since it is not preserved, the spin of the neutrino should be the opposite of what is observed ($+ 1/2$) to ensure conservation.

[...] In Franco's model this contradiction is solved [and is also solved] the problem of CP violation in the decay of K (Michele Barone notes, 16 April 2014-MBPA. My underline).

In conclusion, I can hazard that the main motivation that prompted Selleri to move from the field of PP to FQM was that mentioned above, i.e., the failure of Selleri theory of angular momentum nonconservation as well as of the thought experiments; as the success, at the same time, of the Nobel-awarded quark model of Gell-Mann.

¹⁸⁵ The original Italian text is in Addendum 3, pp. 959-960.

¹⁸⁶ The original Italian text is in Addendum 3, p. 960.

In the 2003 interview with Freire Jr. (see pages 93-94) about his stay in the USA in 1963-65, which, however, may have been affected by the action of time, Selleri claimed that already in 1965 he understood there were big problems in physics, deeply linked to the abstractedness of QM.

Finally, as my research went on, I found a letter, dated year 2000 in the huge correspondence Selleri had collected in his life, in which he reaffirmed 1965 as a turning point for his awareness:

You say that now elementary particle physics has gone in a dangerous direction, but I feel that this is true at least since 1965, when I was young and could witness fashions in physics being born and die, leaving nothing behind, a lot of brilliant careers excepted (Selleri to Winterberg, 7 January 2000-FSPA. My underline).

1.11-LEAVING (?¹⁸⁷) PARTICLE PHYSICS. AND SWITCHING TO FOUNDATIONS OF QUANTUM MECHANICS

First of all, it ought to be made clear that Selleri's did not entirely abandon any of his former research areas. As a matter of fact he carried on researching on PP again for his work in FQM concerning the *experimentum crucis* to discriminate between QM and LR using kaons at ϕ -factory during the 1990s and 2000s, starting in 1983, though. Moreover, several recollections by Michele Barone revealed a constant focus on all areas at stake.

It is also possible to bear witness to several interrelated connections between PP and FQM, carried forward together as well as the fact that his full transition to FQM seemed still in progress after three years.

As a proof of this, the chart below highlights some of Selleri's participations to international conferences during 1961 and 1970, where it is possible to see that he decided to have talks referred to both areas, still confirming his dilated and further evolving transition over several years:

¹⁸⁷ The question mark in the title is for his future research on kaons and for his will to study the whole areas of physics.

Year	Place	Conference	Selleri's talk
May 1961	Geneva, CH	CERN conference on high energy physics	Quasi-elastic scattering of high-energy protons in the OPE
April 1964	Washington D.C.	Meeting of the APS	Deuteron production in proton-proton collisions
June/July 1964	Boulder, CO	Conference on particles and high energy physics	1) πN collisions below 1.5 BeV/C; 2) On the reaction $p + p \rightarrow D + \pi^+$
June 25 th -July 1 st , 1969	Lund, SWE	International conference on elementary particles	On the wave function of quantum mechanics
September 1969	Frascati, ITA	-	Quantum Theory and Hidden Variables (Lectures)
September 1969	New York, USA	High energy physics and nuclear structure	Angular momentum nonconservation scheme
June/July 1970	Varenna, ITA	Foundations of Quantum Mechanics	Realism and the wave function of quantum mechanics

Even though Selleri was participating (in 1969) to a conference about elementary particles in Lund, yet he decided to talk about his first FQM paper, “On the wave function of quantum mechanics”, just released on 11 June 1969 on the *Nuovo Cimento*. And he continued in researching and publishing papers on PP until 1970.

Quite the same thing will happen when he attended several conferences on FQM having a talk on FRT during the end of the 1990s.

Moreover, in the nine months spent at the University of Nebraska between 1 September 1970 and 31 May 1971, Selleri was in charge of the research in High Energy Theory and for teaching one introductory physics course, namely, Quantum Theory. Both interests of PP and FQM were so still cultivated after 3 years.

Looking at the teaching practise Selleri experienced abroad, there is an unambiguous evidence that PP and FQM are both subjects of his courses:

1964/65: Cornell University: Electromagnetism

1966: University of Ghotenbourg: Elementary particle physics

1970/71: University of Nebraska: Quantum Theory

1971: Universidade de Sao Paulo: Elementary particle physics (FSPA)

According to him, a key moment in his awareness for a transition to FQM was his meeting with a book written by Bernard d’Espagnat:

...perhaps in 1971 or 1972 I was in Sweden. And it was in the library of the physics department of the University of Gothenbourg that I found in French the book by Bernard d'Espagnat, *Conceptions de la Physique Contemporaine* (AIP, 2003).

These evidences need to be well analyzed, though. Selleri spent a period as a visiting Professor in the summer of 1966, in Sweden, as we can see on the list of his lecturing abroad mentioned above. It could mean that he probably got confused with his first and second comeback from USA (1965 and 1971). In the summer of 1971, after being to Nebraska, he went to Sao Paulo.

This hypothesis could be confirmed by the following excerpt taken from a list of his trips abroad for research purposes, as found in several CV made by Selleri himself which is common to different folders in distinct archives:

YEAR	ACTIVITY
1959	INFN Fellow Bologna
1959/61	CERN Fellow Geneva
1961/62	Teaching Bologna University
1962/63	Collaborateur Étranger Saclay
1963	First level professorship
1963/65	Research Associate at Cornell University
1966 (summer)	Visiting professor at Göteborgs Universitet
1965/67	Teaching Bologna University
1967/70	Teaching Bari University
1968 (summer)	Visiting professor University of Notre Dame, Indiana
1970/1971	Visiting professor University of Nebraska
1971 (summer)	Visiting professor Universidade de Sao Paulo
1971/73	Teaching Bari University
1973/76	Teaching Bari University-Director of Bari INFN Section

There is no reference at all to a possible trip to Gothenburg in 1971/72!

I found two letters by Göteborgs Universitet at a time when Selleri was at Cornell. Evidence about the Swedish period is well defined below:

I have suggested to the Faculty of Science that you be invited as visiting professor for the time 01 February-30 June 1966.

... The conditions for you as visiting professor would be following:...

iii) It is understood that you will take an active part in seminar and research;

iv) It will be arranged for you to visit Lund and Stockholm to give seminars and discuss with research group.

... Our research activities are mainly the following:

- i) Group theory in elementary particle physics (Lorentz group and symmetry groups)
- ii) Quantum electrodynamics (of course)
- iii) Field theory, more precisely gauge theories of interaction
- iv) π -mesic atoms (in connection with experiments at CERN)
- v) Solid state physics.

I think that your experience would be very valuable for the activities i), iii) and iv).

(Karl Erik Eriksson to Selleri, 18 February 1965-FSPA)

Five minutes ago I got a telephone message that everything is O.K. with your visit. You will be guest professor at our institute for four months.

(Karl Erik Eriksson to Selleri, 6 April 1966-FSPA)

However, there is a letter sent from Gothenburg by Selleri in October 1972 to the Italian philosopher Ludovico Geymonat.

Unlikely the abovementioned proofs, it seems that Selleri spent some time in Gothenburg, though! And precisely in 1972!

As you can see, in a historical research the certainties often turn out to be equivocal!

Anyway, regarding the d’Espagnat’s book, Selleri pointed out that:

It was a revelation. It was something fantastic to see how many problems were open in quantum mechanics.

[...] Reading that book was a great discovery. There was a new field possible to create and a lot of research to do.

[...] It was fascinating to see that so many possibilities were open. So it was clear that the Copenhagen approach was not unique, was not obligatory. We had a philosophical freedom (AIP, 2003).

The book by Bernard d’Espagnat, as well as the same French physicist, played a key role in Selleri’s activity, being seminal for the shift from PP to FQM. For instance, d’Espagnat was the Director of Varenna Summer School held in 1970 on FQM, organized by SIF on Selleri’s proposal. So, I considered really pivotal try to understand when actually Selleri read the book.

I would like to assume that Selleri discovered the d’Espagnat’s book in 1966 and not in 1971-1972 as he declared to Freire!

I am going to demonstrate my claim.

I found an unpublished work titled “Quantum Theory and Hidden Variables”-Lectures on Frascati National Laboratories, written between June and July 1969, and held on September 1969, in which Selleri, while describing of the importance to create an alternative theory to QM, pointed out that:

[...] The philosophical prejudice in favour of a realistic philosophy is strong in the large majority of physicists. This prejudice did not turn against QM simply because very few people knew its real implications. The book of d’Espagnat¹⁸⁸ should hopefully contribute to give a better comprehension of them (Quantum Theory and Hidden Variables, pag. 10. My underline).

Another remarkable proof is found in “Realism and the Wave-Function of Quantum Mechanics”, his talk held in Varenna at the summer school on FQM, surely prepared before June 1970. In this text the book by d’Espagnat seems to be well known and fully used:

[...] The above definitions of [wave functions of first and second type] follows closely the definition of mixtures of the first and second type given in d’Espagnat’s book (*Conceptions de la physique contemporaine* (Paris)). The most paradoxical aspects of quantum mechanics can be attributed to the existence of wave functions of the second type. It is not this the place to review these paradoxes which can be found in the quoted book by d’Espagnat, or in Jauch’s book, or in many other places (Realism and the Wave-Function of Quantum Mechanics, pag. 401. My underline).

In the same year (1972), Selleri published a paper, “Sull’ideologia nella fisica contemporanea”, appeared on the special issue of *Critica Marxista*¹⁸⁹ fully dedicated to Marxism and Sciences in 1972.

¹⁸⁸ In the original paper there is a note marked with number 4 with the book reference: B. d’Espagnat, *Conceptions de la Physique Contemporaine* (Hermann, Paris, 1965).

¹⁸⁹ Critica Marxista was a magazine founded in 1963 as a theoretical institution of the Italian Communist Party.

I would just like to point out that Selleri quoted the book by d’Espagnat to recommend its use in order to understand the basic concepts of QM, like state vector, wave packet collapse and mixtures of first and second species. As far as I am concerned, it is impossible Selleri used and mentioned this book with such great knowledge of the facts if he had not read it well before. And certainly not in the same year of his trip to Gothenburg. Actually, the work came out in the summer of 1972 while he wrote his letter to Geymonat from Gothenburg in October.

For historical thoroughness, it should be reported that d’Espagnat had also written another book, *Conceptual Foundations of Quantum Mechanics*, whose first edition was published in 1971. Selleri may well have read this one in Gothenburg. It should be stressed, however, that Selleri never mentioned this specific book.

Finally, in an official reports on research programmes dates 1972, Selleri, presenting a project on the research on the historical and philosophical foundations of quantum mechanics, with a group of six collaborators, stated that¹⁹⁰:

The applicant has carried out studies and research on the historical and philosophical foundations of quantum mechanics both in Italy and during two stays in the United States and Sweden (Lincoln-Nebraska 70/71 and Goteborg 66). During his recent stay in Sweden [1972], the applicant gave a course of lectures on the history of quantum mechanics¹⁹¹ (AGAB-Progetti di ricerca-Richieste finanziamenti ricerca scientifica dip di fisica armadio D settore 22 palchetto 4 busta 1. My underline).

So, his studies and researches dates back to 1966, whereas in 1972 he came back to Sweden for lectures on history of QM!

¹⁹⁰ I will introduce it as an example of his activity with a Group on FQM at INFN Section of Bari in the section 2.8.3.

¹⁹¹ “Il richiedente ha compiuto studi e ricerche sui fondamenti storici e filosofici della meccanica quantistica sia in Italia che durante due permanenze negli Stati Uniti e in Svezia (Lincoln-Nebraska 70/71 e Goteborg 66). Durante la recente permanenza in Svezia [1972], il richiedente ha tenuto un corso di lezioni sulla storia della meccanica quantistica” (AGAB-Progetti di ricerca-Richieste finanziamenti ricerca scientifica dip di fisica armadio D settore 22 palchetto 4 busta 1. My underline).

In the light of the above, I renew the conclusion that the knowledge of Selleri on the book by d’Espagnat may have occurred in 1966, thus anticipating the period of his embryonic interest in FQM.

Going back to his move from PP, Selleri devotes himself to FQM after his first paper (Selleri FQM 1969), his remarkable correspondence with Louis de Broglie and the crucial turning point constituted by Varenna 1970 (all these events will be investigated in the next chapter).

As a further proof, in the already mentioned interview with Olival Freire, to his precise question about the date of his shift to FQM, Selleri answered that it happened “[...] Basically in 1970 with the Varenna School” (AIP 2003). Even though I started showing how Selleri’s shift to FQM started before, always underscoring the key feature that his move was almost expanded in three years and never really conclusive (If we think about kaons).

During his activity in FQM, he produced a huge amount of publications and works mainly related to the following topics:

1. the Empty Waves, for the study of the Einstein-De Broglie picture of wave-particle duality;
2. the Einstein-Podolsky-Rosen Paradox, dealing mostly with Bell’s Theorem, its proof and the meaning of the experimental investigations;
3. the probabilistic Local Realism, the most general formulation of our idea still leading to the validity of Bell’s inequality;
4. the Inequalities of Local Realism, showing that Bell’s inequality is just one of an infinite set of inequalities that are consequences of local realism (Selleri, 1991-FSPA)¹⁹².

But, regarding the difficulties inherent in the investigation about the precise date for Selleri’s interest in the FQM interest, the ultimate piece of evidence in this sense can be found in a fundamental letter that Selleri wrote on 11

¹⁹² The document (Selleri, 1991) characterizes an excerpt from a typescript found in FSPA. Selleri prepared it on September 1991, grouping 25 complete papers in the different groups above reported. As Selleri stated: “The present collection has been prepared for the Cesena conference Bell’s theorem and the Foundations of Modern Physics (October 7-10, 1991) and has been made possible by a grant of the Commission of the European Communities (D.G. XII) for which we are very grateful”.

June 1982 from Geneva to the Italian philosopher Ludovico Geymonat, with whom he had had a long correspondence in the 1970s and 1980s¹⁹³.

The following words turned upside down the perspective on this issue, backdating it to before 1958, and almost enveloping the whole thing in a great indeterminacy, without forgetting Selleri's commitment with FQM in 1965 during his lectures at the "Scuola di perfezionamento" in Bologna, after his comeback from the USA (see page 94).

Even though it must be taken into account that the letter is not contemporary to the context narrated by Selleri, but later, as it can always be conditioned and/or misled by memories. Anyway, according to Selleri:

[...] I remember that I had not yet graduated and I held a small talk at a cultural association in Bologna talking about wave-particle dualism. A machine gun shoots corpuscles and does not give rise to interference. A body oscillating on water generates waves and this extended phenomenon gives rise to interference. The electrons that generate interference but that are always observed as corpuscles are instead incomprehensible. Even then I was impressed that physicists were satisfied with a declaration of impotence^{194 195} (Selleri to Geymonat, 3 October 1972-ALG-Archivio Ludovico Geymonat, box 75, folder 792).

To sum up, after this analysis, I am more and more convinced about my assumption that the move from PP to FQM did not involve a clean sweep. Maybe his interest for QM was already born in unsuspected times during his university studies but it actually started in 1966, thanks to his knowledge and reading of the book by d'Espagnat (which occurred in that year, as I have shown above), which made things easier in Selleri, who in 1965, during the

¹⁹³ I would like to thank my colleague, Dr. Fabio Lusito, at Bari University, for showing me this seminal letter as well as the one dated 1972, that I mentioned at page 100 in this dissertation. And, above all, the Archivio Geymonat and the Biblioteca del Museo di Storia Naturale e dell'Acquario Civico di Milano, specifically in the person of its Director, Dr. Marina Santi Amantini, for giving me the opportunity to quote several excerpts thanks to their permission. Both letters are stored at Archivio Ludovico Geymonat (ALG), box 75, folder 792).

¹⁹⁴ "[...] Ricordo che non ero ancora laureato e tenni una piccola relazione a un'associazione culturale bolognese parlando del dualismo onda-corpuscolo. Una mitragliatrice spara corpuscoli e non dà luogo a interferenze. Un corpo che oscilla sull'acqua genera onde e questo fenomeno esteso dà luogo a interferenza. Incomprendibili invece gli elettroni che generano interferenze ma che sono sempre osservati come corpuscoli. Già allora ero sbalordito che i fisici si accontentassero di una dichiarazione di impotenza" (Selleri to Geymonat, October 3rd, 1972-Archivio Ludovico Geymonat (ALG), box 75, folder 792).

¹⁹⁵ The "declaration of impotence" is Selleri's title through which he defines the explanation of the wave-particle dualism (better, duality) made by the Copenhagen interpretation of QM.

advanced course held in Bologna, had used the book by Bogoliubov, encountering considerable difficulties. Moreover, it was developed in almost three or four years, from 1968 (the date of his first degree thesis as advisor in QM), to 1971, passing from the period 1970-71 spent in Nebraska (where he read a number of remarkable books on FQM) and with the contribution of all the elements already explained in the foregoing pages.

1.12-A DEEP, FAR-REACHING INTERPLAY BETWEEN SELLERI AND SOCIAL AND POLITICAL OCCURRENCES

In this section I would like to underscore and bring forward how Selleri's scientific activity was strictly interrelated to the social and political environment, by mentioning a pivotal intervention on 155^o meeting of the SIF Executive Board held on 2 March 1970, included in the SIF Bullettin Number 75. Selleri, already oriented in the research on FQM, after being disappointed due to the Nobel Prize which had been awarded to Murray Gell-Mann and the lack of success of the angular momentum nonconservation, wrote a crucial, astonishing letter, *La piramide azteca della fisica teorica delle particelle elementari*¹⁹⁶. In the following SIF meeting there was a debate which sprang up from the contributions by Fabri and by Baracca and Bergia. The three interventions are all found in the ASIF. Maybe, Selleri's intervention was the first concrete implementation of a certain discomfort in the world of research activity in elementary particle physics¹⁹⁷.

Indeed, concerning the importance of Selleri's first awareness on this topic, after the Fabri and Baracca and Bergia replies, in the following years there was the publication of several works such as:

¹⁹⁶Selleri F., *La piramide azteca della fisica teorica delle particelle elementari* (The Aztec pyramid of elementary particles theoretical physics), 155^o meeting of the SIF Executive Board, March 2nd, 1970, SIF Bullettin n. 75, pagg. 13-17).

¹⁹⁷ An analysis on the role of social and political environment on physical research in 1970s, which I will introduce in the next chapter, has been made by (Baracca, Bergia, del Santo 2017).

- *Ideologia e fisica contemporanea (Ideology and contemporary physics)*, written by Selleri himself and published in a joint work titled *Sul Marxismo e le scienze (On Marxism and science)* in 1972 on the journal *Critica Marxista-Quaderni* n. 6;

- *La Spirale delle Alte Energie (The High Energy Spiral)*, published by Angelo Baracca and Silvio Bergia in 1975;

- *L'ape e l'architetto (The bee and the architect)*, edited by Giovanni Ciccotti, Marcello Cini, and Giovanni Jona-Lasinio in 1976.

I am reporting down below the whole Selleri's letter¹⁹⁸ in which there is a thorough analysis of the social and political reasons underlying the crisis involving elementary particle physics at that time. A crisis still perceived as a raw nerve by several scholars of the time, actually:

One thing that has long worried the writer is how the very high number of theoretical physicists of elementary particles could be reconciled with the poor results achieved, especially in view of the fact that in many "developed" countries every year's best scholars have been devoted to this activity. In the last 10-15 years some of the most interesting topics have been the following, in chronological order:

- The method by Tamm and Dancoff
- the static model
- the dispersion relations
- the Mandelstam representation
- Regge's poles
- SU_3 and other symmetries
- Veneziano's model.

Each of these and many other topics, once gone, left little or nothing to be remembered. The last three are still under discussion, but at least for two of them (Regge's poles and symmetries) most of the hopes have already been dashed. As a result, the scientific field to which most efforts have been devoted has led to an understanding of elementary phenomena which is close to zero. How was this possible? A whole series of secondary problems revolve around this main problem.

1) What determines the sudden flaming appearance of a new subject in the horizon of Physics, if then, invariably it is reduced to a pile of lukewarm ash?

¹⁹⁸ Due to the length of the text, in order not to weigh even further the notes, the original version in Italian has been inserted in Addendum 3, pp. 960-964, together with the full contribution of Fabri and from Baracca and Bergia, which are at pages 964-968, as showed later.

- 2) Why are the respected and revered leaders of World Physics always the same people or only increase by one unit by direct co-optation by the existing leaders?
- 3) Why is deductive research (axiomatics, mathematical physics) held in high regard while inductive research (phenomenology) despised?
- 4) Why is no one willing to admit the failed state of this kind of research? Conversely, most people today talk about the continuous progress of Science?

These and other questions find a natural answer if we see the high energy theory physicists as an Aztec pyramid that tends to grow and preserve in the best conditions its upper part (that of the great leaders).

The tendency to grow is annually characterized by the entry, among the group researchers, of young Italian postgraduates (PhD in America etc.), and as the number of university students and the physics institutes are constantly rising, it follows that the entry among the theoretical ranks of young graduates will keep rising if the fascination exercised by elementary particles will not diminish.

With this mechanism every year a new step, larger than the previous one, will be added to our Aztec pyramid. It is clear that at some point this structure will become so big that the great leaders (those of the first step) covered with prestige, money and responsibility will no longer have time to think. They will only have time to apply the drawing principle. Looking into the crowd of their followers they will be able to decide from time to time which research work rise to the honours of the great news in order to:

- a) create a new trend and then control the thought of those in the next levels of the structure;
- b) share with the author the credit of the new “discovery”.

Of course, if a great physicist, let us hypothesize, a Californian one, promoted to his level an obscure physicist, an Englishman, for a two-dimensional representation of the amplitudes of diffusion, it will matter little if later such representation proves to be false and useless. From that event on they will both be sitting at the top of the structure, their position being justified by the mere fact of having been successful, of having stolen the scene for a certain period of time. Clearly, now we have the answer to question 1), and partly to 2), posed at the beginning. A further element that helps to understand point 2) is that potentially important discoveries made by elements of the pyramid that are not entirely conditioned are simply ignored by the top level (and therefore by the whole structure). There are plenty of examples of this, but I will mention just one. SU_3 was first discovered by three Japanese researchers, but it is certainly not to them that the biggest credit for SU_3 goes. Although these remarks are important to understand the reason behind 2), I do not think they definitely solve this problem, for such reason I will deal with the issue further on in this dissertation. As far as question 3) is concerned, it is obvious that the disregard of the inductive process is functional for the stability of the structure because a scarce effort in this field will make it unlikely that new regularities will be discovered by people not directly controlled by the bosses.

Consequently, it is very unlikely that new bosses will be nominated with the consequent redistribution of scientific power. Probably the validity of 3) is largely due to the affirmation of quantum mechanics in the formulation of the Copenhagen School. This theory has led to dramatic conclusions of metaphysical scope, such as the impossibility to postulate the separate existence of two systems that have interacted in the past.

It is safe to say that inductive reasoning, which starts from the properties of elementary objects, becomes very difficult when these objects cannot be thought as existing, while deductive reasoning, which starts from the axioms of quantum mechanics and relativity, is much safer.

Then, what has been stated at point 3), becomes fundamental to the stability of the pyramidal structure of high energy theoretical physics; moreover, its validity is imposed by quantum mechanics. This theory certainly requires a thorough examination, yet this is not the place to do so.

The last point which should be mentioned before getting to the main issue is the one indicated with 4) at the beginning of this writing. It is obvious that if the pyramid has to get bigger and bigger, young people need to be attracted to the subject and therefore they need to be told that progress is being made and that the field is fascinating. Perhaps now is the time to say that it is not necessary to think of high-energy theoretical physicists as a caste of mystifiers who want to increase their privileges and power and who systematically lie to achieve this result. I have no doubt that the vast majority of the leaders and all the physicists on the various levels are in perfect good faith. So, it must be a cultural phenomenon that leads to the deviations discussed, if the present analysis is correct. A phenomenon which has the same characteristics of the medieval approach according to which the earth was at the center of the universe. Going back to 4) it is clear that affirming the continuous progress of theoretical physics helps theoreticians to justify to themselves and to others their very existence. There are at least two types of reasoning in support of this progress. First, it is said that, for instance, even if the static model has not led to anything good in itself, it has nevertheless made it possible to discover dispersion relations and so forth. Secondly, it is claimed that, even if a topic does not give results, it is always important to have studied and discarded it so that the error will not be repeated in the future. The weakness of this approach is evident. If theory A did not lead to valid results yet allowed the discovery of theory B, one cannot take B in partial justification of A when B itself did not lead to valid results. Moreover, having spent fifteen years proving false all the theories proposed A,B,C,D,... does not seem a bright way to do research. At this point, many things are coming to the surface, yet there is still one big question. Can the top level of the symbolic pyramid be so powerful that the discoveries made by elements who are not at the top levels of the structure and not among the chosen ones be overlooked without any serious risk? Of course there are some checks, and they are very strict.

They range from the need to be successful (and thus to draw attention to oneself), which translates into a rapid adaptation to the trend imposed from above, to objectively mystifying language (for example: quantum mechanics does not apply to certain observables such as electrical charge, but nobody says so, they say instead that there are “rules of superselection”), to the imperative to be modest and recognize their inferiority over the great leaders, in order for them to be able to assert their superiority over those of the lower steps.

Nevertheless, it is hard to understand why no one has ever had the courage to show that what is being done now is wrong and that the path to choose is another. To further clarify this point, let us return to the hypotheses we started from. The only criteria to explain the problems posed at the beginning are those of the continuous enlargement of the pyramid and the stability of the upper step.

As you can see I do not accept the view that the rational interpretation of natural phenomena is the basic criterion that determines the evolution of physics (at least of the theoretical one of high energies). We can then achieve a global understanding of the situation if we assume that at the beginning we chose the wrong track and followed it up to now. It is now very difficult to realize that we are on false ground because we have strayed too far from the right path (which I somehow believe to exist). Therefore, this analysis ends with the apocalyptic vision of the whole, an ever growing, aztec pyramid building up at an uncontrolled pace reaching the sky of Nothingness while the light of the Truth has already disappeared below the horizon.

This metaphor explains why so little is understood of the elementary phenomena and also definitely provides the solution to problem n. 2). The “discoveries” coming from the lower layers of the pyramid can be neglected by the leaders, provided it is not convenient for them, simply because, in the thought of the leaders, none of them can be objectively important, being obtained from a fundamentally false culture.

SELF-CRITICAL REFLECTION

Of course I think the above description is basically correct. However, it is clear that the description given is exaggerated, limited, and that many other points need to be carefully analysed. This writing has the only purpose of providing a reflection about this matter, generated by the dialectical contrast between the prevailing optimism and the pessimistic vision introduced.

INABILITY TO GENERALIZE

The above analysis cannot be applied to experimental high energy physics. Certainly, experiments have been conditioned by theoretical thought, because today, a self-respecting experimentalist, must be aware of the latest theoretical trends and based on these must decide what research to do. But the results obtained have an objective validity that transcends the theoretical ideas that gave rise to them. Moreover, an experimental physics of advanced technology, such as that of high energy, involves the acquisition of methodologies and collateral knowledge (computers, instrumental techniques, etc.) and a

technological spin-off probably beneficial on the economic level. I do not know enough about other fields of research to judge whether the ideas put forward can be applied to them as well. I believe that something of this kind may be generally valid, but that theoretical high energy physics is a borderline case.

A CONSTRUCTIVE HINT

If the presented analysis is correct, the only way to find the right path seems to be a return to the origins of elementary particle physics.

It seems therefore appropriate to study the ideas and experiments of the immediate post-war period and, above all, the search for different starting points compared to those historically chosen (*La piramide azteca della fisica teorica delle particelle elementari*, 155^o meeting of the SIF Executive Board, March 2nd, 1970, SIF Bulletin n. 75, pagg. 13-17).

In 1994 Selleri, thinking of a possible project with Hadronic Press, i.e., a book on his scientific production entitled *Quantum physics in space and time* (which was never published, though), quoted *La piramide azteca della fisica teorica delle particelle elementari* and literally stated that the work represented a source of “historical interest for my transition” from PP to FQM.

In this work, Selleri’s tone is ironic, sometimes epic and colourful, and, I dare say, a bit frustrated. But also really straight to the point and well centered on a reality that cannot be ignored. Even though he decided in the letter not to investigate thoroughly on QM (also because he is at the beginning of his career in this subject), he already focuses on its some critical aspects. Moreover, the debate with other interventions by Fabri and Baracca and Bergia¹⁹⁹ showed well how the subject touched several raw nerves, being also a current topic of discussion within SIF itself at that time.

In fact, E. Fabri²⁰⁰ sent his own intervention on this issue, written in Pisa on 9 June 1970, at the 158th meeting of the Executive Board, included in the SIF Bulletin N. 77²⁰¹.

¹⁹⁹ I will report the whole contributions by Fabri and by Baracca and Bergia in the Addendum 3.

²⁰⁰ Elio Fabri (1930-): <https://www.sif.it/static/SIF/resources/public/files/uomini-quarks/fabri.pdf>. Accessed 24 September 2020. There is also a devoted page on Facebook.

²⁰¹ Due to the length of the text, in order not to weigh even further the notes, the original complete version in Italian has been inserted in Addendum 3, pp. 964-966.

Fabri introduced a precise and acute analysis of Selleri's issues, providing a major contribution to the debate. He started accepting the opportunity given by Selleri:

Selleri's writing [...] wants to stimulate a reflection and maybe a discussion of the state of theoretical physics of elementary particles. Personally I believe that the stimulus should be welcomed and I would like to bring my contribution to the debate.

Then, he began analyzing the poor results obtained by the research on PP in the face of so many forces at play:

The two basic phenomena that Selleri reveals (continuous growth of the pyramid, research dictated by short-lived fashions) I believe cannot be denied.

Probably the statement about the failed state of the research will not find much acceptance and maybe needs to be better articulated. [...] However, the basic assessment seems acceptable to me, considering that:

a) No idea of a really valid general theory arose after electrodynamics, now 20 years old, and this is everything but satisfactory in its basis, although it makes predictions of incredible accuracy;

b) All attempts to ground a theory of elementary particles on a general basis either turn out to be sterile, or face very serious difficulties, or lead only to "negative theorems", of non-existence, impossibility and so on;

[...] The attitude of many theorists can be called "head in the sand": you know the situation but you continue to work in the usual way, always hoping that someone gets the good idea. This, however, does not eliminate the sense of frustration and the need to justify oneself in the eyes of other physicists: from here, at least in part, derives official optimism. After all, this sense of inferiority is also evident in Selleri's speech, who considers experimental physics strangely immune from his criticisms.

Obviously Fabri shared some of Selleri's points of view but had a few differences, too:

Concerning the sociological aspects of the matter, my analysis of the causes is quite different from Selleri's. The principle of "stability of the top step" does not seem essential to me

[...] I see it rather as an effect. In reality it is the whole structure (the pyramid) that seeks a balance that ensures its persistence. [...] it is society as a whole that promotes or blocks a growth.

[...] Then there is specialization, which is a by-product of the need for selection: not being able to keep up with everything, the researcher defends himself by specializing. [...] It is evident that this also goes in the sense of the conservatism of ideas: the super-specialist does not dare to leave his field, and does not dare to judge what he cannot understand. Therefore, you have to rely on the mechanism of co-optation from above. [...] Finally, continued growth leads to a heavy constraint on working methods. If the researcher has chosen a highly studied field he cannot work calmly, under pain of being beaten over time. If he takes refuge in a little explored field, he has a complicated life and risks being ignored even if he has said something new. It is understandable that the many, who do not feel like Einstein, will choose the first alternative. The one to emerge will not be the one who is more knowledgeable, but who is more ready and has more intuition.

Then, Fabri dealt with an unusual theme, namely the motivations for the attitude of a social system towards advanced research, showing as the only appropriate example, the US society:

Research can have, for an advanced capitalist society such as the USA (the only one that is worth examining, given the obvious subordination of our research to that of the USA), some useful functions: prestige, preparation for technological developments, and ideological coverage. But it can also involve the danger of the birth of culturally revolutionary ideas (in the sense of the Copernican revolution).

It is therefore important to protect oneself from this risk; and this is achieved, much better than with an dull ideology, by encouraging in the researcher patterns of behaviour that promote his identification with the social structure: race to success, extreme division of tasks, teamwork seen as renunciation of individual responsibility, and so on. The mechanism of interaction between research and society rewards those researchers who best conform to the scheme, by giving them power. The leaders' behaviour towards the rest of the pyramid is therefore determined: they were chosen because they had guaranteed to behave in the desired way.

Concerning the role of experimental physics, Fabri's approach definitely diverged with Selleri's:

[...] I do not see anything to improve the situation of experimental physics. I do not think the argument of the objective validity of the experimental results is correct. [...] It seems to me that experimental research is often brought about by an ever-growing logic. [...] The argument concerning the technological fallout is null and void. [...] Conversely, it is often technology that strongly influences research.

[...] Even in large machines [on elementary particle research] the large experiment eats the small. Even the condition of the experimental researcher does not seem to me any better than that of the theoretician. Actually, I find it even more alienating. The theorist may be frustrated, but he is often aware; instead the experimentalist can instead find satisfaction in the external aspects of his work [...] and thus forget the purpose that the official rhetoric would still assign him: the Knowledge of Reality. To the point of reversing the values; and of fighting in defense of the system that has swallowed him up.

Eventually, Fabri tried to come up with some solutions, even if they look like some solutions veined with pessimism and resignation:

[...] It seems to me that Selleri identifies in QM the primary origin of our present troubles: in its “dogmatic” character and in the low consideration that alternative proposals have received . [...] I quite agree with the facts (less with the philosophy) but I do not consider them very relevant to the issue.

[...] what should be put on trial is not QM in general but the second quantization and the field theory, whose difficulties and contradictions are so well known that we have learned by now “to live with them”.

I confess, however, that I do not have too many illusions. Of course, a new Heisenberg would be welcomed with open arms (with achieved victory); but in the meantime, if the above analysis is correct, how many possible Heisenberg have been discouraged from studying difficult and unpopular subjects and diverted towards more conformist goals? However, there is yet something we can do: each one of us has some opportunities, albeit small, in our power (thesis topics, work reviews, assignment of scholarships; not to mention the most important ones, but within the reach of a few) in which we inevitably take sides for or against the current trend. If all those who at least partially share these considerations will want to use their power to support the “difficult” research, maybe we could still see something new under the sun (Fabri, 9 June 1970-ASIF).

In order to bring this debate to a conclusion, I am adding below the contribution by A. Baracca²⁰² and S. Bergia, dated 12 December 1970 at

²⁰² Angelo Baracca (1939-): Italian Physicist graduated at Milano, he worked at Bologna and has been professor at Firenze University.
See https://it.wikipedia.org/wiki/Angelo_Baracca; <https://www.pressenza.com/it/author/angelo-baracca/>;
<https://www.radioradicale.it/soggetti/8507/angelo-baracca>. All accessed 24 September 2020. Almost all pages on Internet deal with Baracca commitment as Essayist specialised in issues related to civil and military nuclear power, pacifist and environmental activist, and columnist.

164th meeting of the SIF Directive Board and included in the SIF Bulletin Number 82²⁰³:

The letters by Selleri (Bulletin 75) and Fabri (Bulletin 77) gave rise to a debate which deserves to be resumed, deepened and widened [...].

Faced with the dissatisfaction that several researchers who are beginning to become significant show for some features of current research, [...] we consider it very important that physicists from different disciplines and, hopefully, scientists working in different fields of research begin to communicate with each other on the issues addressed in those letters. Selleri and Fabri, in our opinion, have found the right point to start from.

[...] Selleri has tried to give a first political-sociological approach to a situation that he considers unsuccessful; Fabri has taken a step forward in the analysis of the internal logic that supports and conditions research.

[...] the physicists of the sector, lying in an undisputed scientific vision, [have] devoted very little attention to methodological and epistemological issues.

[...] In the absence of a theory, particle physics today proceeds through models. These are usually presented either as techniques of approximation “drawn from theory” or as attempts of relativistic extension of techniques developed in the theory of potential. The assumptions of the model soon fall into the limbo of the irrelevant problems; therefore, almost immediately we renounce in practice to demonstrate anything (this is exactly what happened for the poles of Regge), to do what the same experts of the rules of this game often call the “kitchen”, adding some poles or cut or modifying here and there the treatise (i.e. ultimately adding some parameters). [...] But the most serious consequence on the methodological level involves the very meaning of experimentation. [...] Rather than solving real problems, often times new and fictitious problems are posed when the prediction is not verified, and the model itself provides a quick answer “explaining” the result with the addition of some parameters. (Baracca and Bergia, 12 December 1970-ASIF).

The foregoing interventions at the meeting of the SIF Executive Board show how Selleri and other scholars were strictly engaged to the social and political events of the period and how these were intertwined with the trends and patterns of their scientific research and debate.

²⁰³ The original complete version in Italian has been inserted in Addendum 3, pp. 966-968.

As a further confirmation, in the letter which follows, dated 1998, Selleri stated the political origin of his commitment to FQM:

[...] I can tell you, about myself, that for almost thirty years I have engaged in a battle for the radical renewal of physics. Many have laughed, although they do it so much less now. I would just like to add that my choice did not spring up from an arrogant attitude, but from a deep compliance with the mass criticism against scientism that emerged with the student movement in the late 1960s and early 1970s²⁰⁴ (Selleri to Marotta, 21 December 1998-FSPA. My underline).

Further evidence of the key role played by both social and political environments in Selleri's beliefs and switch from PP to FQM could be found in another letter that Selleri himself sent in 1972 (when he had already turned to FQM), to Ludovico Geymonat. Selleri revealed with a profound and painful analysis (and self-analysis), the pattern of his switch to FQM. Several excerpts, written in a clear and simple way, as it was in the character of Selleri, provide an appraisal of his activity in PP as well as the outstanding role of politics in his physical choices:

[...] I am a former elementary particle physicist who saturated his mind for ten years with complex angular momentum calculations, with the analytical properties of the S matrix, and other similar junk.

Then the influence of the student movement first and later the PCI (The Italian Communist Party) led me to a self-critical reflection and a crisis. You can't imagine the sense of liberation that one feels in going out of oneself, looking at what one is and what one has done in a new way, seeing its enormous narrowness, and at the same time the anger for having spent so much energy within a very narrow cultural tunnel without light [...].

In short, capitalism has to pay for this too, and I have committed myself over the last two years to trying to fill the enormous cultural gaps that I have, because of the compulsory sterile super-specialization that I had and was forced to. I was stroke by lightning (in the USA!) when I read a Marxist-Leninist treaty published in the Soviet Union.

²⁰⁴ The letter is sent to Gerardo Marotta, a lawyer at the Istituto Italiano per gli Studi Filosofici (Italian Institute for the Philosophical Studies): "Posso dirle, di me, che da quasi trent'anni ho ingaggiato una battaglia per il rinnovamento radicale della fisica. Molti hanno riso, anche se ora lo fanno molto meno. Vorrei solo aggiungere che la mia scelta non è nata da un atteggiamento arrogante, ma da un'adesione profonda a quella critica di massa dello scientismo che si affermò con il movimento studentesco alla fine degli anni '60 e all'inizio dei '70" (Selleri to Marotta, 21 December 1998. My underline).

Everything became clear in the light of the contradiction between materialism and idealism! So there is no doubt that dialectical materialism fascinates me, even though I am unable to face the criticism [...] maybe only [for] a matter of ignorance²⁰⁵ (Selleri to Geymonat, 3 October 1972-ALG-Archivio Ludovico Geymonat, box 75, folder 792. My underline).

This is clear evidence (as mentioned on pages 127-131 in this chapter), that the contact with the precious source constituted by d’Espagnat’s book, took place in 1966 in Gothenbourg and not some years after, because Selleri needed (as he explains in the above letter) at least of two years of research to fill his gaps, before starting to research on FQM in 1969, at the time of his first paper in FQM, of his lectures in Frascati and so forth. And these are all occurrences in which he already dealt with d’Espagnat book in his citations. In addition, concerning the fundamental finding of the Marxist-Leninist text in the USA, it probably took place in Nebraska in 1970-71, his last well-established period of continuous permanence in the USA²⁰⁶.

As a confirmation of my last statement, I found in the SIF Bulletin an abstract sent by Selleri for a speech at the SIF National Congress in 1971 entitled *La contraddizione materialismo-idealismo nella meccanica quantistica*²⁰⁷. His lecture was to be held on 29 October 1971, but on SIF Bulletin is possible to read “summary received late”.

I consider this anecdote as a confirmation of the fact that Selleri had just read the book in Nebraska in 1971 and immediately wanted to talk about it, but being in San Paolo, Brasil, in the summer of 1971, he probably did not manage to send his paper on time.

²⁰⁵ “[...] Io sono un ex-fisico delle particelle elementari che si è saturato per dieci anni di calcoli con il momento angolare complesso, con le proprietà di analiticità della matrice S, e altre analoghe porcherie. Poi l’influenza del movimento studentesco prima e del PCI²⁰⁵ poi mi ha portato ad una riflessione autocritica e ad una crisi. Non puoi immaginare il senso di liberazione che si prova nell’uscire da sé stessi, guardare a quello che si è e si è fatto in modo nuovo, vederne l’enorme limitatezza, e insieme la rabbia per aver speso tante energie entro uno strettissimo corridoio culturale senza luce [...]. Insomma il capitalismo deve pagare anche per questo, e mi sono impegnato negli ultimi due anni a cercare di riempire le enormi lacune culturali che ho, a causa della sterile superspecializzazione cui mi avevano e mi ero costretto. Un altro colpo di fulmine è stata la lettura (in USA!) di un trattato di marxismo-leninismo edito dall’Unione Sovietica. Come diventava tutto chiaro alla luce della contraddizione materialismo-idealismo! Così non c’è dubbio che il materialismo dialettico mi affascina, anche se non so rispondere alle critiche [...] forse solo [per] una questione di ignoranza” (Selleri to Geymonat, 3 October 1972-Archivio Ludovico Geymonat (ALG), box 75, folder 792. My underline).

²⁰⁶ Selleri’s stay in Nebraska covered the period 1 September 1970-31 May 1971.

²⁰⁷ “The materialism-idealism contradiction in quantum mechanics”.

Also because the other period spent by Selleri in USA, namely at Notre Dame in Indiana, is fully connected to his work on PP (see pages 104-105 of this chapter).

Another confirmation about the importance of Selleri's stay in Nebraska lies in two letters. The first one, written in 1984, is part of a pivotal correspondence with Wheeler. The latter wrote to Selleri congratulating him for the book *Die Debatte um den Quantentheorie*²⁰⁸:

Dear Professor Selleri, Professor Roman Sexl did me the honor and gave me the great pleasure of presenting me with a copy of your Die Debatte um den Quantentheorie when I saw him at San Antonio on feb. 1. Last night I finished reading it and want to thank you for the service you have done the whole community by this study of the leaders, their ideas and where the subject has gone since the golden days.

Among points I found especially illuminating or interesting or both are Jordan's position on the existence of an external world (page 29); the positions of Christian Bohr and Niels Bohr on teleology (page 116); Einstein's view of positivism (page 114); the references to William James (whom Bohr loved to read) (page 121); the ideas of our wonderful colleague, Eugene Wigner, with those position on consciousness, however, I do not always agree (page 123); Einstein's letter to Born, about what he called a spooky action at a distance (page 136); and what could perhaps be called "Dirac's defeatism" (pages 136-137). Congratulations and warm good wishes (Wheeler to Selleri, 6 February 1984-FSPA).

Selleri, in his answer, after his warm acknowledgements, underlined his valuable time spent in Nebraska:

[...] I have been working hard for many years on this kind of problems starting essentially in 1971 when a year spent at the università of Nebraska gave me the opportunity to read the original papers on quantum theory (Selleri to Wheeler, 23 February 1984-FSPA. My underline).

The second one was written in a 1990 letter, in which Selleri spoke about one of his preferred written books, *Quantum paradoxes and physical reality*. He claimed that:

²⁰⁸ This is one of the most remarkable books written by Selleri, translated in other seven languages and really appreciated all over the world.

[I have here] a copy of my book that came out this year, *Quantum paradoxes and physical reality*. [...] It is twenty years of work since I started writing it in 1970 in Lincoln, Nebraska. I am satisfied with it, in the sense that it is certainly the best I can do to present my – sometimes called “heretical” – ideas in physics. The use of the concept of heresy says a lot about the current state of physics, not being a joke, but sometimes a serious and deliberate classification. I try to stick to certain great classics for which heretic/orthodox does not exist, while true/false is all that matters²⁰⁹ (Selleri to Erina (?), 13 July 1990-FSPA. My *Italics* and underline).

So, Nebraska played a key role in Selleri’s full awareness of Marxism-Leninism as well as in the opportunity for him to read a lot of key texts by the great physicists in QM and to write a seminal book in his FQM research. Finally, I wish to draw the attention of the reader on the fact that Selleri’s attitude and reasons for his leaving the PP area was common to other scholars as well. Especially remarkable is the following evidence by Francesco Calogero²¹⁰, reported below, in which he indicates several faults and some shortcomings on the so-called sociological approach in PP:

[...] at a certain point I began to have a sort of rejection for particle physics as it was done at the time, based on the rules of dispersion, then with the so-called representation by Veneziano. More and more I had the impression that a solid theoretical framework did not exist and that things that were considered interesting were to some extent determined by fashions rather than by a real correspondence with experimental reality or with the real existence of a solid theoretical framework. so at some point I decided to leave particle physics. I think this happened at the end of the 1960s, when I wrote a work together with other colleagues on the Veneziano model itself, and I found it absolutely futile (Bonolis, Melchionni 2003, pag. 78)²¹¹.

²⁰⁹ “[ho qui] una copia del mio libro, uscito quest’anno, *Quantum paradoxes and physical reality*. [...] sono 20 anni di lavoro dato che cominciai a scriverlo nel 1970 a Lincoln, Nebraska. Ne sono soddisfatto, nel senso che è certamente il meglio che io sappia fare per presentare le mie – talvolta chiamate “eretiche” – idee in fisica. L’uso del concetto di eresia la dice lunga sullo stato attuale della fisica, non trattandosi di uno scherzo, ma talvolta di una seria e deliberata classificazione. Io cerco di restare attaccato a certi grandi classici per i quali eretico/ortodosso non esiste, mentre vero/falso è tutto ciò che importa” (Selleri to Erina, 13 July 1990-FSPA. My *Italics* and underline). The recipient’s name is not understandable.

²¹⁰ Francesco Calogero devoted himself to Pugwash becoming its general secretary in the period 1989-1997 and receiving in this role the Nobel Peace Prize in 1995 together with Josef Rotblat as founder of Pugwash as well as to Pugwash itself as an organization “for their efforts to diminish the part played by nuclear arms in international politics and, in the longer run, to eliminate such arms”: <https://www.nobelprize.org/prizes/peace/>. Accessed 24 September 2020. For more detailed information, see (Bonolis, Melchionni 2003, pag. 89-91).

²¹¹ The Italian original text is in Addendum 3, p. 968.

[...] What I did not like, at that stage, about elementary particle physics, was that the criterion which established if something could be true or false was no longer valid, instead, the most important criterion was whether it was fashionable or not. This attitude still partly exists for the theoretical physics of elementary particles; it is partially coupled with the fact that, as experimental elementary particle physics requires enormous funds, the research is accompanied by a somewhat ridiculous advertising campaign, whereby at any given moment we are told that we are on the verge of a discovery by which everything will be understood, whereas we are still very far from it. Every small discovery is presented as a great achievement, [...] small compared to the overall architecture and to the fact that there are still a great number of unanswered questions and unresolved issues, or, better, issues that we do not yet know how to address.

I prefer not to be there, in this picture, because this aspect of a continuous advertising, propaganda, disturbs me; just as it disturbs me that the things that are considered important are more so because they are “fashionable” and not because they are actually aimed to the understanding of nature, i.e., to the development of physics.

[...] There is nothing wrong with searching for the final theory, what is wrong is in making people think you are close to the final theory when in fact you are far away from it. [...] you are really a long way away from getting a coherent picture of elementary particle physics. There are a number of open questions: [...] there are so many elementary particles that you have no idea why they exist. You have some recipe to put them together, some formulae that can connect them to some extent; but there is no real theory to solve all the many issues related to the phenomenology of elementary particles (Bonolis, Melchionni 2003, pagg. 84-85. My underline)²¹².

Moreover, Calogero again highlighted again the sociology of PP, recalling Selleri's view as introduced in *La piramide azteca della fisica teorica delle particelle elementari*.

[...] at a certain point, I came to a phase of saturation with regard to particle physics, which was in crisis in those very years. However, in particle physics, for sociological reasons, there is never the acknowledgement that there is a period of crisis, but on the contrary there is always a great fanfare, linked to the fact that this physics on the experimental side is very expensive, therefore it needs to appear always on the brink of great developments to justify the enormous expenses. At that time [at the end of the 1960s], as many now admit, there was a phase of weariness and a real crisis of the theory. I completely distanced myself from it.

²¹² The Italian original text is in Addendum 3, pp. 968-969.

What annoyed me then, and does so even now, is this fashion trendy side of a physics in which to a large extent the interest of theoretical arguments is determined by fashion. Around this fact there is also a whole sociology of those who manipulate trends, of those who promote themselves and their friends on the basis of a mixture of scientific, sociological and power aspects. [...] Blending marketing with theoretical physics seems to me a bit desecrating; I could see, however, that it was done, and it seems to me that it is still being done today, more in particle physics than in other areas (Bonolis, Melchionni 2003, pag. 102. My underline)²¹³.

Finally, another piece of evidence of a PP as an almost closed path with very few outlets, is provided by Carlo Bernardini:

[...] The K mesons, the weirdness, the mixing kept the centre stage, but we did not have enough power. The “theory”, apart from SU(3), the quarks and the new selection rules, had taken paths of little interest, although a crowd of specialists had been extensively working on problems related to Tullio Regge’s representations, to the analyticity of amplitudes, to dispersion relations, to sum rules, to the axiomatics of the fields, to bootstrap, to Pomeranchuk’s theorem, to the models of ‘evasion and conspiracy’, to Veneziano’s amplitudes, to Chew’s ‘nuclear democracy’ and so on. Frankly, seen in retrospect, the whole thing seemed like lurking in the dark, in a jungle of somewhat gratuitous mathematics (Bonolis 2008, pag. 371. My underline)²¹⁴.

1.13-ANY LEGACY IN PARTICLE PHYSICS, THOUGH? A QUICK SUMMARIZATION AND BEYOND

Much has been said about elementary particle physics from a historical point of view, with numerous evidences of oral history and impressive works, true paramount masterpieces on the evolution of research in PP, cosmic rays and particle accelerators. The field of elementary particle physics always runs very fast and currently there are bigger and bigger projects, which makes it a bit more complicated to weave a historical vision of very recent events. Perhaps the announcement of the discovery of the Higgs boson at CERN in 2012 could be a subject of research in this regard.

²¹³ The Italian original text is in Addendum 3, p. 969.

²¹⁴ The Italian original text is in Addendum 3, p. 969.

Currently Selleri's role in PP is obviously linked to topics that are now largely outdated.

The outcomes reached by Selleri between the 1958 and the 1970, namely, the Peripheral Model, the OPE model, and the angular momentum nonconservation have been replaced by many others.

Nevertheless, Selleri's work received great appreciation and interest in those years, see for instance the endorsements he acquired during the competition for his full professorship (See pages 31-35).

But, as Selleri rightly pointed out in his letter titled *La piramide azteca della fisica teorica delle particelle elementari* (see pages 135-138), and also analyzing the debate that followed and that I reported in the previous pages (Fabri, Baracca and Bergia, Calogero, Bernardini) the PP sector has often been considered in crisis and full of examples of roads that led to nothing. Many discoveries presented as novelties of absolute value have passed without a trace.

It should be recognized that a certain legacy of Selleri's research in PP can be found in the fact that his results have been taken into account and have also been the object of attention by great contemporary scientists. I have shown many testimonies for his work, think of Erasmo Recami (page 67) or the letters by Bell, Rubbia, Bethe, Puppi, Cocconi, Sivestrini, and Segrè; or Barone and Guerra recollections. As said, his studies have certainly been surpassed very soon and today little or no trace remains. However, it is fundamental, in the history of science, to trace the paths that have not continued or have not led to definitive results, for their importance in widening the debate and because of a number of partial results that have been part of international research on the subject. On the other hand, for Peripheral Model and OPE there was a great response but also a very fast development, with some variants already presented while Selleri was in the USA, just three years after his work. And this is an indirect confirmation of the validity of his work.

The model of angular momentum nonconservation proved to be an excellent alternative to SU_3 and the other models present at the time. And it was only undermined by Gell-Mann's Nobel Prize-winning octet. It is not known how real was Selleri's expectation even for this award, as he confided to his family and as I was told in interviews with Angela Pizzi and Michele Barone. And this, however, can be a simple vision or a personal exaltation as well as an attitude fueled by some response to his work, which were really existing, actually.

A very different discourse can be made in the case of the resumption of Selleri's studies in PP. It took place from 1983 onwards and continued, albeit apparently not continuously, until the early years of the 21st century. It concerned the asymmetry in pion emission by neutral kaon pairs as an application of Einstein-Podolski-Rosen paradox to the case of PP, an experimental ground for discrimination between quantum mechanics and local realism. In chapter two, I will dedicate a paragraph specifically to this topic. Within Selleri's FQM legacy, I will also introduce the meaning of his research on kaons, showing how the topic is very current and how it is also being done and proposing research on what Selleri started. From the historical point of view the topic is very probative and far from solved!

I conclude on the possible legacy on PP by entering another remarkable recollection by Michele Barone which (recalling what has been stated by Guerra at pages 117-118 and 121) underscored the importance of Selleri's research on angular momentum nonconservation and the possible reaction of the scientific community to the Selleri's decision to judgmentally investigate on FQM, as a further confirmation that the figure of Selleri at the time of PP research and the transition to FQM was the subject of attention by the scientific community, even with mixed feelings:

[...] who had exercised a great fascination not only on me but on everyone was Franco. At that time (I think it was 1968) the bubble chamber group of Bari was doing an experiment on an idea that Franco had proposed on the violation of the angular momentum conservation in the decay of some strange particles.

His name was the major one, because if the result had been positive it would have been a great discovery. Although as students of the first two years we didn't know him, his name hovered.

Franco was seen as a character regarded, due to some articles he had written with Bonsignori on the peripheral model, as the *enfant prodige* of the Italian theoretical physics, on which many had focused their hopes. And then the peripheral model had had many experimental confirmations, it had been tested with energies from MeV to GeV. They were analyzing in Bologna the 600 MeV results of a synchrotron that was in the USA. In those years the idea to make bigger accelerators was coming up at CERN. He was the first to understand that the theoretical model that was becoming established thanks to the influence of the U.S. school, Gell-Mann, etc., was a model that did not solve at the root the contradictions or the weakest points of what had been acquired until then, but that tended to jump at the bottom of these difficulties, going to new and higher energies and focusing on new resonances that could be discovered.

Then, in my third year of university I had chosen his complementary Quantum Theory, I still have the handouts he had done in chapters, typed. And he criticized the FQM. In those years, I realized this later, doing this was seen very badly.

The fact that 20 years later someone took them out made him immediately branded. At the Institute, it was not seen well. Meanwhile, the experiment on the nonconservation of angular momentum had not yielded good results. Even Fabio Ferrari (who had brought him to Bari from Bologna, with whom he was very close friends, they both knew each other and lived in Vigna del Re, in Palese) was disappointed by Franco's passage to the FQMs, for what was seen as a turnabout by Selleri.

Ferrari was so disappointed that I remember that in the corridor where Franco was, Ferrari had the studio a little further down. As soon as they met, they did not even say hello to each other (Interview with Michele Barone, Bitonto (BA), 1 June 2019).

In his interview to Freire jr, Selleri himself gave a confirmation on some possible disruption in his career due to his transition from PP to FQM, even though he pointed out how the situation in Italy was really much better than in other countries:

You see for example I became a full professor in 1980 and my career was delayed about ten years. I could have become full professor in 1970, so ten years before. My career was delayed because I switched to quantum mechanics. I have no doubts that if I had stopped to do research in 1970, considering the success of the one-pion-exchange model I would have got a chair in 1970. The fact is that doing something so dangerous like foundations of quantum mechanics made people also here more careful, but they did not really stop me. Actually, later, they gave me a chair.

I had no doubt, knowing the situation in countries like the Soviet Union, United States and France, that I would have been destroyed there as a scientist. There was no tolerance for the foundations of quantum mechanics (AIP 2003. My underline).

Despite these events, the recognizing of Selleri's work in PP remained evident as well as the influence he had on several scholars and on the debate of that time. His research on PP will come back again starting the 1983, when he began researching on neutral kaons produced at a ϕ -factory, in order to use a source with very high efficiency, really useful to test between QM and LR.

CHAPTER 2

SELLERI AND THE

FOUNDATIONS OF QUANTUM

MECHANICS (1969-2003)

MEPHISTOPHELES: “By this I recognise a most learned lord!

What you can’t feel lies miles abroad,

What you can’t grasp, you think, is done with too.

What you don’t count on can’t be true,

What you can’t weigh won’t weigh, of old,

What you don’t coin: that can’t be gold”.

(Johann Wolfgang von Goethe²¹⁵, Faust, Part II Act I Scene II:

The Emperor’s Castle: The Throne Room, 4917-4922)²¹⁶

2.1-FOREWORD

The second chapter deals with Selleri’s activity on FQM. It is definitely his most well known and best studied period, with several scholars that investigated his work, a huge confirmation of his remarkable role in the international scientific community. Among these sources, it sounds really intriguing to quote (Jammer 1974), (Nutricati 1998), (Freire Jr 2004), (Tassani 2013) (Freire Jr 2014), (Baracca, Bergia, Del Santo 2017), and so forth. The last two references made an impressive work, analyzing the FQM in the period between 1960s and 1990s and focusing specifically on Italian peculiarity. Of course there will be ample excerpts from these texts throughout the whole chapter, as well as some reference to them during my explanations.

However, despite what has been reported, I believe that the researches I have carried out in several archives and in Selleri’s personal private archive, never investigated before, and presented in this chapter together with several oral history testimonies collected during the various interviews, can help to give a broader and more varied view of Selleri’s role within the international scientific research and the historical and socio-political times in which he worked.

²¹⁵ Johann Wolfgang von Goethe (1749-1832). See <https://www.britannica.com/biography/Johann-Wolfgang-von-Goethe>. Accessed 24 September 2020

²¹⁶ See, for instance, always accessed 24 September 2020: https://www.poetryintranslation.com/PITBR/German/FaustIIActIScenesItoVII.php#Act_I_Scene_II.

In this chapter, after a tentative introduction on the birth and development of the QM and its diverse interpretations, I will mention the early Selleri's contributions. Then, his impact on the scientific community is explained also in the light of the deep social and political upheavals of the time. Specifically, I will show Selleri's intertwining with the social-political events in the 1960s and 1970s, going briefly to 1980s and 1990s. Moreover, I will analyze Selleri's participation in the Steering Committee of the Italian Physical Society, his role in Varenna 1970 Summer School on FQM, as well as a description of the School itself. In another part of this long and multifaceted chapter, I will introduce the essential FQM themes on which the debate was then developed (Hidden Variables, EPR Paradox, Bell's Inequalities and Bell-Type Experiments, Empty Waves, and so on), the Selleri's contributions to each of them and the contemporary reaction of scientific community to his work. His research on FQM covered at least 35 years with several books and up to 125 English papers on the above groundbreaking topics. They are all subjects that have been at the heart of the current mainstream research programmes on FQM all over the world. However, at the end of 1960s they were not so developed and studied, thus placing Selleri among the forerunners of the field.

In parallel with his research in the aforementioned themes, I will consider a glance on Selleri's professorship, on his popularization activity as well as his great role in the organization of several conferences all over the world, giving also a look at some outstanding books he wrote, reviewed by several scholars. Then, I will analyze the pivotal research on kaons as a discrimination between the outcomes reached by Quantum Mechanics and Local Realism, the varied reasons linked to Selleri's passage from FQM to FRT, and finally, analyzing a possible legacy in the FQM field.

2.2-FOCUSING ON THE DEBATE ON QUANTUM MECHANICS AND ITS DIFFERENT INTERPRETATIONS

What is mentioned in this paragraph is the result of a substantial consultation of numerous papers and books, all listed in the bibliography as “General Literature”. Obviously, also in this case, as in chapter one, this is only a tentative introduction, definitely not thorough at all. The description of the genesis and development of the quantum theory and QM goes beyond the scope of this work. There are thousands of papers and books that fully describe its evolution as well as the social and political landscape that entered in the diverse choices by the various main characters.

Quantum Mechanics (together with the theories of Special and General Relativity) characterized the twentieth century, fully immersed and linked in a double-stranded with its protagonists with a series of epochal events which I recalled in the Introduction of this dissertation (see page 2).

By the end of the 19th century, following a large number of results found on the atomic scale and concerning the behaviour of matter and electromagnetic radiation, classical theory had become incapable of explaining in an organic way the new picture that was being outlined. Starting from the work of several scientists, it was possible to build a theory which instead shed light on the new emerging framework. In less than thirty years, scientists such as Max Planck, Arnold Sommerfeld, Albert Einstein, Max Born, Erwin Schrödinger, Niels Bohr, Wolfgang Ernst Pauli, Werner Heisenberg, Louis De Broglie, Pascual Jordan, Paul Adrien Maurice Dirac and so on developed the current context of quantum theory and QM.

There are several authors who have introduced some periodizations of the origin and development of quantum theory. Always taking into account the arbitrariness of these temporal classifications, which are never definitive, at least three diverse moments can be found:

- I) the old quantum theory, developed in the period 1900-1925, which includes among its various protagonists Max Planck, Albert Einstein, Niels Bohr, Arnold Sommerfeld, Arthur Compton, and Louis de Broglie;

- II) quantum mechanics, built in 1925-1927 thanks to the contributions, among others, of Louis De Broglie, Erwin Schrödinger, Werner Heisenberg, Max Born, Pascual Jordan, and Niels Bohr;
- III) Relativistic quantum mechanics or quantum field theory, grown at the end of 1920s until the 1950s, with the researches by Paul Adrien Maurice Dirac, Wolfgang Ernst Pauli, Sin-Itiro Tomonaga, Richard Feynman, and Freeman Dyson.

Another useful periodization for quantum theory is exposed in the *Compendium of Quantum Physics. Concepts, Experiments, History and Philosophy*, edited by D. Greenberger, K. Hentschel and F. Weinert²¹⁷ and full of several contribution by diverse scholars. There was an early period, going from 1900 to 1913, among the Planck's work on blackbody radiation and the application by Bohr to the spectrum of hydrogen atom. But,

how did quantum theory develop in the intervening years? One may conveniently distinguish two themes: a quantum theory of matter, often in equilibrium with a Maxwellian electromagnetic field; and the more radical theory of light quanta introduced by Albert Einstein [...] in 1905 and pursued almost exclusively by him for [...] nearly twenty years, until Arthur Compton's experiments made them inescapable [in 1924 and 1925] (Greenberger, Hentschel, Weinert 2009, pagg. 617; 620).

The great work on hydrogen atom made by Niels Bohr and its meaning is exposed in the 2000 book by Abraham Pais, *The Genius of Science. A Portrait Gallery*²¹⁸:

Bohr's work of 1913 caused a veritable explosion of activity in quantum physics, in Europe as well as in the United States. As he wrote to Rutherford, 'The whole field has from a very lonely state suddenly got into a desperately crowded one' (Pais 2000, pag. 19).

²¹⁷ (Greenberger, Hentschel, Weinert 2009) in the following. It provides short introductions to most of these interpretations, including the following: algebraic QM, Bohm interpretation, Bohmian mechanics, consistent histories, Copenhagen interpretation, GRW theory, hidden-variables models of quantum mechanics, ignorance interpretation, Ithaca interpretation, many worlds interpretation, modal interpretations, orthodox interpretation, probabilistic interpretation, and transactional interpretation, even if a quote should also be made for the stochastic interpretation and the ensemble interpretation.

²¹⁸ (Pais 2000) in the following.

From 1914 to 1922, a second phase followed, with several applications of Bohr-Sommerfeld atomic model, that became inaccurate really soon and limited for several spectroscopic anomalies and for multi-electrons atoms. Indeed, between 1923 and 1925:

[...] various problems emerged, however, that turned out not to be treatable within the framework of Bohr's and Sommerfeld's quantum theory, despite the relentless efforts of the Sommerfeld school in Munich and competing groups in Göttingen, Copenhagen, and Leiden. The spectrum line intensities of the Zeeman and Stark multiplets, for instance, could not be calculated satisfactorily, nor did many of the heavier atoms seem to follow the patterns of hydrogen-type atoms. The model could thus not be extended further and an impasse seemed to have been reached. Worse still, persistent anomalies surfaced pointing to aggravating discrepancies between theory and experimental data (Greenberger, Hentschel, Weinert 2009, pag. 613).

Pauli and Heisenberg contributed to give a “crystal clear answer” to an “increasing frustration and mounting uncertainty about the further trajectory that emboldened physicists to venture down unconventional paths” (Greenberger, Hentschel, Weinert 2009, pag. 614). As they stated:

[...] In late 1924, Wolfgang Pauli [...] started to toy with the idea of a “mechanically unaccountable duplicity” or ambiguity [...], a strange precursor to the idea of spin (which only emerged at the end of 1925, too late to rescue the old quantum theory from its internal problems, but of crucial importance for later quantum mechanics).

[...] Pauli had grown up in the ‘anti-metaphysical’ context of *fin-de-siècle* Vienna. Actually, he was the god-son of Vienna's foremost apostle of phenomenalist thinking, the physicist-philosopher Ernst Mach. Pauli was thus the first to stop referring to electron orbits, perhaps reminding himself of what Mach had always asked when someone in his presence talked about atoms as something immediately given: “Hab's aans g'sehn?”, “Have you ever seen one?”. [...] So, Pauli decided to scrap [the] “metaphysical” concept [of the electron orbits] and to concentrate on observables [...].

[...] For a while Heisenberg remained sceptical about this radical suggestion and tried other avenues [...], but he failed to reach closer agreement with the observed intensities of spectrum lines. In June 1925 he gave up and decided to implement Pauli's demand for “a profound modification of the classical concept of motion”. In describing the state of a mechanical system, he consistently only used observable oscillation frequencies and amplitudes and represented them by an integral of quantities in quantum theory.

As Max Born was quick to point out, Heisenberg was applying a type of mathematics totally new to him: matrix algebra. [...] Thus matrix mechanics was born and with it the first step toward a new generation of theories all somewhat equivalent to each other, also including Schrödinger's wave mechanics, Born's and Wiener's operator mechanics and Dirac's q-algebra. They are subsumed under the label QM.

[...] Rather than being fully incommensurable, the old quantum theory and the new quantum mechanics were more intricately related to each other. But many years were needed before this was fully understood (Greenberger, Hentschel, Weinert 2009, pagg. 615-616).

The new QM became highly developed and followed. According to the Italian physicist Emilio Segrè, which was awarded the Nobel Prize in 1959:

The study of quantum mechanics from the publication of Planck's paper in 1900 to Bohr's in 1913 had engrossed relatively few physicists. On the other hand, from 1913 to the completion of the theory in 1928, it absorbed the entire strength of the new generation of physicists. (Segrè 1980, pag. 175).

In Robert P. Crease and Alfred Scharff Goldhaber's book, *The Quantum Moment: How Planck, Bohr, Einstein, and Heisenberg Taught Us to Love Uncertainty*²¹⁹ is described the passage from the first quantum theory to QM, how the critical moment was addressed, and how physicists faced with a strange, puzzled birth of new concepts which ended up altering all of our pre-existing views of reality as well as changing our language in approaching it:

[...] from 1900 to 1925, scientists gradually came to see that the quantum did not fit smoothly into the Newtonian world, and clashed with assumptions about space, time, and causality. But many hoped to find a way to tame their unruly child and find it a place within the elegant Newtonian universe, as they had found place for so much else since the *Principia*'s publication. During this period, the public viewed quantum theory as akin to relativity; terribly important but incomprehensible to all but the brightest scientific lights, who were excited about it for reasons far too complicated to understand.

Then everything changed. In an astonishingly short period, 1925 to 1927, hopes to tame the quantum were dashed by a new theory with foundations fundamentally different from those of classical physics, and whose capstone was the uncertainty principle (Crease, Goldhaber 2014, pag. 23).

²¹⁹ Hereafter referred to as (Crease, Goldhaber 2014).

[...] Physicists, it would turn out, were starting off on the wrong foot. They were starting with a classical approach to things like action and phase space, then trying to find the right rules to adapt it to the quantum realm. At the end of its first quarter-century, it was a conceptual mess. Jammer called it ‘a lamentable hodgepodge of hypotheses, principles, theorems, and computational recipes rather than a logical consistent theory’. Many scientists still hoped the quantum would go away. The quantum’s most disturbing implication was that the subatomic world was thoroughly schizophrenic. Quanta were wavelike [...] but they were also like particles - then called *corpuscles* - [...]. Neither waves nor particles were baffling by themselves; the fact that quantum things behaved like both was (Crease, Goldhaber 2014, pag. 115).

[...] Then, in 1925-27, the whole map - in fact, two whole maps - were found, [...]. The two mappers - Werner Heisenberg and Erwin Schrödinger - had not tried to adapt the classical map but had started over from scratch. [...] If the first quantum revolution involved the astonishing discovery of an alien creature appearing in Newtonian territory - with more and more instances of this alien creature cropping up - the second quantum revolution of 1925-27 involved the even more astonishing discovery: we have been living in alien territory all along! (Crease, Goldhaber 2014, pag. 118).

Concerning the birth and development of QM, Virgile Bresson aimed at reporting in his degree thesis on the causal interpretation of QM²²⁰, a periodization due to Olival freire Jr. A **metaphysical period** is first introduced, a period that Freire gets started in the 1920s with the birth of QM and continued until 1950. It corresponded to the birth of the controversy with the great debates based on *Gedankenexperimente*, including the Einstein, Podolski, and Rosen (EPR) paradox, against the “spooky action at a distance” and with Schrödinger’s cat paradox about the measurement problem and the overlapping of states. It is called metaphysical because it showed well the perception of these issues by the community of physicists. Indeed, the debate in this period lacked experimental applications and took place outside the laboratories, and physicists saw the debate as a matter for the philosophers of science. It was the golden age of the Copenhagen interpretation that took the support of the large majority of physicists, taking advantage of the fact that,

²²⁰ Bresson V., *L’interprétation causale de la mécanique quantique: biographie d’un programme de recherche minoritaire (1951–1964)*, PhD dissertation. (Besson 2018) in the following. I will draw some concepts for the next pages, until 157.

as said, physicists did not pay attention to paradoxes, considering them not physical. And, of course, for its simplicity of operation.

Then, there was the presence of a **transitional period**, characterized by a resumption of interest in FQM by a growing number of physicists, with the departure of several alternative interpretations to QM, which, however, fail to counteract its hegemony. The most famous was that of Bohm's hidden variables of 1952. It motivated John Bell to reflect on the locality in the early 1960s. According to (Crease, Goldhaber 2014):

[In 1952] Bohm paid a price for introducing hidden variables; he had to introduce nonlocality into the theory. "That means that what you do here" - Bell points to the desk in front of him - "has immediate consequences in remote places" - pointing out the window to Mt. Blanc. "And that was *extremely* odd". It is what Einstein called "spooky action at a distance". Bell began to brood about whether you could create a hidden variable theory without spooky action at a distance [namely, a local one] (Crease, Goldhaber 2014, pag. 208. *Italics* in the original text).

[...] Bell found hard to avoid the conclusion that hidden-variable theories had to be nonlocal, and ended the paper ["On the Problem of Hidden Variables in Quantum Mechanics"] with that suggestion. [...] in his second paper, "On the Einstein-Podolsky-Rosen Paradox" he said, "I tried to imagine what hidden variables there might be that would avoid the nonlocality of Bohm [...] and I found that I couldn't do it". A remarkable thing then happened. Instead of looking to disprove the impossibility of a hidden variable theory that would preserve locality, he began to look for its opposite. [...] "I started looking for the proof of impossibility. And I found it" (Crease, Goldhaber 2014, pagg. 208-209).

I decided reporting this statement on Bell because his work has been maybe the most influential on FQM in 20th century, and Selleri worked a lot on it, being often cited precisely for his research on Bell's inequality.

Together with Bohm's theory, there was also Hugh Everett's theory of relative states, who proposed an original solution to the problem of the reduction of the wave packet after the measurement; and the quarrel between Wigner and Rosenfeld about the measurement problem.

All these approaches began to undermine the so-called “monocracy of Copenhagen school”, a definition introduced by Jammer to describe the absolute role played by Bohr, Heisenberg, Pauli, Born, and Jordan interpretation. It must be said that in this phase the reaction against the promoters of alternative interpretations was brutal by supporters of Copenhagen interpretation. The story of Everett can give a factual idea²²¹.

Finally, the **period of legitimation of the dispute and the acquisition by the community of physicists** came. It started in the 1970s and continues to these days. It is based on the fact that physicists saw the opportunity of experimentally testing some characteristics such as the locality (following Bell and his inequality which dictate the conditions that any local quantum theory must pursue). Here, mentioning Freire, philosophy entered the laboratories. The strong moment was given by Aspect’s experiments that in the 80’s seemed to show that it was necessary to give up the locality. Research on the FQM became highly regarded by the community after Aspect’s experiments. This third period was legitimated by the SIF 1970 Summer School held in Varenna.

Another periodization inherent only to the QM was submitted by the Italian physicist Augusto Garuccio:

After the development of quantum mechanics, there was a first phase, lasting about 30 years (1935-1965) and based on Bell’s inequality and locality, in which it was discussed whether or not the QM was complete. Then a second phase, with another 30 years of attempts to verify Bell’s inequality with the first experiments in the 1970s, up to those of Aspect et al. in 1980-1982, with at least 2-3 sub-phases of experiments. They continued until the 1990s with nuclear decays, atomic falls in the 1980s, Parametric Down Conversion, I and II type in the 1990s, with the latter marking an epochal change. It is easier to have entangled photon sources and start thinking about how to use them: imaging, teleportation, quantum computing, etc.. If you want, here comes the third phase of research, that of technological application, which until now has been a phase of study but is now going towards its realization (Interview with Augusto Garuccio, Bari, 1 July 2019).

²²¹ See (Freire 2014), Chapter 3, pages 75-139.

Today, the QM is a prominent topic of discussion in different environments and presents technological developments at the heart of the scientific agenda of governments and institutions worldwide.

For instance, in (Crease, Goldhaber 2014) it is showed how QM is currently so deeply rooted in our society that quantum concepts are used in our everyday language to fully express some concepts of everyday life.

The authors hold a course at the University on “quantum moment”, still analyzing the cultural impact produced by quanta, showing how currently it definitely sounds as a mainstream term:

The word *quantum* is applied to everyday life in plays, poetry, movies, paintings, fiction, music, philosophy, numerous company names, and “pop science” approaches to psychology and neuroscience. Heisenberg’s uncertainty principle, Schrödinger’s cat, parallel worlds, and other concepts and images of quantum mechanics show up on T-shirts and coffee cups, in cartoons, novels, poems, and movies (Crease, Goldhaber 2014, pag. 3. *Italics* in the original text).

Our course came about because the two of us often discussed our bafflement with the way scholarly and popular language is sprinkled with references to “quantum this” and “quantum that”, sometimes in the form of genuine insights, at other times of meaningless buzzwords designed to impress or dazzle. Initially, we assumed the cultural impact of the quantum was largely inconsequential, and that we would mostly find terms and images used by charlatans to impress the gullible, and artists with a dash of scientific literacy who appropriated the cultural authority of science. We found that quantum language and images are indeed often used in humorous, pretentious, wacky, and exploitative ways. But we also noticed that the quantum appeared to provide important new terms and ways of thinking about human beings and the world that have become second nature (Crease, Goldhaber 2014, pagg. 5-6. My underline).

[...] Quantum language and imagery is especially metaphorically appealing because it reflects difficulties we face in describing our own experiences [...] (Crease, Goldhaber 2014, pag. 126).

[...] Updike²²² suggests [...] that the quantum [...] has already changed us spiritually. It has provided a range of novel and helpful images to interpret our experiences, and thereby give us a better grip on the world, on a scale comparable to Newtonian

²²² John Updike (1932-2009) was an American novelist, poet, short-story writer, art critic, and literary critic. One of only four writers to win the Pulitzer Prize for Fiction more than once Updike published more than twenty novels, more than a dozen short-story collections, as well as poetry, art and literary criticism and children's books during his career: https://en.wikipedia.org/wiki/John_Updike. Accessed 24 September 2020. See also <https://www.nytimes.com/2009/01/28/books/28updike.html>. Accessed 24 September 2020.

mechanics. By heightening our sensitivity to the gaps, inconsistencies, warps, and bubbles of experience, quantum language and imagery have made the world itself different (Crease, Goldhaber 2014, pag. 127).

The QM is considered the best explanation for the unusual behavior of the subatomic world nowadays. It is certainly a powerful theory, and it had contributed to produced a wealth of practical applications for our daily lives. In the 2003 book by Jim Al-Khalili, *Quantum. A Guide for the Perplexed*²²³, there is a whole series of practical applications linked to quantum principles, such as, for instance:

[...] a certain property of electron wavefunctions - their obedience to the Pauli exclusion principle - explained how electrons arrange themselves in energy 'shells' in atoms, which led to an understanding on how metals conduct electricity. This, in turn, led to the study of semiconducting materials and the invention of the transistor, from which grew the microchip, the computer and the Internet.

Furthermore, CD and DVD players make use of a corresponding property of photons that gave us the laser, a device with all sorts of industrial, medical and research [...] applications.

The phenomenon of quantum tunnelling gave us nuclear power and will hopefully lead one day to [...] nuclear fusion.

Yet another strange quantum property of certain materials that are cooled to near absolute zero gave us superconductors, which might one day provide us with the ultimate solution to energy conservation: power cables with no electrical resistance (Al-Khalili 2003, pagg. 203-204).

[...] There are far too many applications of quantum mechanics in medicine [...] ranging from X-rays to laser surgery. I will therefore briefly discuss just two. One utilizes the quantum mechanical spin of nuclei [NMR] while the other involves the use of antimatter to map the brain [PET] (Al-Khalili 2003, pag. 220).

Today, seventeen years after this book, a lot of hypothetical landscape are currently for daily use. In addition, it is possible to deal with a future possible technological application for superposition, entanglement and decoherence²²⁴, thinking of tracking a single atom, introducing quantum

²²³ Hereafter referred to as (Al-Khalili 2003)

²²⁴ Quantum entanglement is a physical phenomenon that occurs when a pair or group of particles are generated, interact, or share spatial proximity in a way such that the quantum state of each particle of the pair or group

cryptography, quantum imaging, teleportation, quantum computing and so forth.

Just as an example, the wave-particle duality was strongly rejected by the founding father Bohr and still carried out by Einstein practically single-handedly from 1905 to 1924 and more. However, thanks to it, we crossed from the light microscopes to the Scanning Probe Microscopy, which combines STM and AFM and could be used for imaging surfaces but also for moving molecules and individual atoms²²⁵.

QM is only one of the many interpretations that have occurred in history, perhaps the most widely accepted, but certainly not the ultimate theory of everything, as many still believe today. In fact, the methodological and ontological level inherent in it continues to be the subject of great debate even today.

As well explained by Maximilian Schlosshauer in his book *Elegance and Enigma. The quantum interviews*²²⁶, QM gave rise to many puzzles, being still unsolved in its ontological features:

[Quantum mechanics], a theory whose formalism can be written down on a napkin whilst attempts to interpret it fill entire libraries. A theory that has seen astonishing experimental confirmation yet leaves us increasingly perplexed the more we think about it. How can we know so well how to apply this theory but disagree so vehemently about what it is telling us?

cannot be described independently of the state of the others, including when the particles are separated by a large distance. The topic of quantum entanglement is at the heart of the disparity between classical and quantum physics: entanglement is a primary feature of quantum mechanics lacking in classical mechanics.

See, https://en.wikipedia.org/wiki/Quantum_entanglement#:~:text=Quantum%20entanglement%20is%20a%20physical,inclusing%20when%20the%20particles%20are. Accessed 24 September 2020.

The term *decoherence* is used in many fields of (quantum) physics to describe the disappearance or absence of certain superpositions of quantum states. Decoherence is a consequence of the unavoidable interaction of virtually all physical systems with their environment. [...] Decoherence then explains within quantum theory why macroscopic objects seem to possess their familiar classical properties. No additional classical concepts are required for a consistent quantum description. Decoherence explains, for example, why particles appear localized in space (hence there is no need for an additional particle concept). Contradictory levels of description (classical and quantum) are no longer needed, instead a consistent description in terms of a universal wave function can be pursued. See https://link.springer.com/chapter/10.1007/978-3-540-70626-7_48#:~:text=The%20term%20decoherence%20is%20used,physical%20systems%20with%20their%20environment. Accessed 24 September 2020.

²²⁵ This enhancement passed through the Transmission Electron Microscopes (TEM) in 1931; the Scanning Electron Microscope (SEM) at the end of Second World War; the Scanning Tunnelling Microscope (STM) in the early 1980s, and eventually the Atomic Force Microscope (AFM).

²²⁶ Hereafter referred to as (Schlosshauer 2011).

[...] [Quantum mechanics takes] the game of abstraction and counterintuitiveness to a whole new level. Grounded in a stunningly elegant formalism [...], quantum theory is not only unabashedly silent on questions of ontology but it also gives rise to puzzles that have inspired and been appropriated by audiences ranging from scientists and philosophers to New Age transcendentalists and Marxists (Schlosshauer 2011, pag. 19).

The most recent historiography has deeply analyzed what, since Jammer 1974 onwards, has always been called the “Monocracy of the Copenhagen school”, either “Copenhagen spirit” (from Pauli’s words, feature on which John L. Heilbron pulled out an interesting work, “The earliest missionaries of the Copenhagen spirit”²²⁷) or even “Copenhagen interpretation” (Think of “Who Invented the ‘Copenhagen Interpretation’? A Study in Mythology”²²⁸ by Don Howard) and so forth, basically uncovering a mystified vision.

This misleading view of a compact front is subsequent and not at all contemporary and can be attributed to Heisenberg in the mid of 1950s²²⁹. Actually, the conclusion is that the founding fathers showed a lot of differences between them, not being a monolithic block at all. Think, for instance, about the great quarrels between Bohr and Heisenberg. Moreover, already during the construction of the fundamental details of the QM there was a huge objection from great scientists such as de Broglie, Schrödinger, Einstein, which continued for a long time, with the EPR paradox of 1935, and continuing with Bohm in 1952, Everett in 1956, Bell in 1964-1966 and so on. And they have shown enormous differences between them too, with the result that no interpretation has fully succeeded in being a real alternative to the standard interpretation (or Copenhagen one, or orthodox interpretation, or QM, or whatever you want to call it). Each scholar showed differences of choice and view on the various interpretations of QM strictly related to their physical beliefs as well as their social, political and philosophical ones.

²²⁷ (Heilbron 1988) in the following. Also known as Heilbron, J.(2001): “The earliest missionaries of the Copenhagen spirit”, in Galison, P., Gordin, M., Kaiser, D. (eds.) *Science and Society. The History of Modern Physical Science in the Twentieth Century*, New York Routledge.

²²⁸ Howard, D. (2004), “Who invented the ‘Copenhagen interpretation’? A study in mythology”. *Philos. Sci.* 71, 669–682. (Howard 2004) in the following.

²²⁹ “Bohr never used the term ‘The Copenhagen interpretation’, nor did anyone else until Heisenberg in 1955” (Kumar 2009, pag. 276).

Think for instance of Marxists like Rosenfeld and Bohm who have been on both sides of the barricade, in defense and against standard interpretation, respectively.

In this regard, Selleri stated in his interview with Olival Freire Jr., held in Bari on June 2003 that:

[...] I knew that [Rosenfeld] was considered to be a Marxist, and that is very difficult to understand. I mean how could he use the Marxist approach to justify the Bohr and Copenhagen philosophy which is basically in line, as told by Max Jammer, with the Kierkegaard and Høffding philosophies. These people belonged to the right wing of Hegelianism, which is something very different from Marxism, which in principle should be the left wing. Kierkegaard basically says that there is the thesis and also the antithesis, but no synthesis is ever reached. According to him synthesis does not exist, it is impossible. So it's difficult to understand the position of Rosenfeld given that Bohr's complementarity is also a negation of syntheses, such as the one between space-time description and causality, or the one between wave and particle (AIP 2003).

Hegel was perhaps the main philosophical reference for Selleri as I will show later, being also the Marxism a derivation of Hegelianism in his belief.

But now let us see how the different interpretations that have occurred can be represented in a concise and by no means exhaustive way. As far as I am concerned, among the hundreds books and papers, I took into account in this description mainly (Al-Khalili 2003), and (Greenberger, Hentschel, Weinert 2009) in no particular order and with direct and indirect references²³⁰. Since the beginning, it sounds clear that many scholars well illustrate the plethora of interpretations, not accepting the so-called “monocracy of Copengahen school”.

[...] it would be extremely dishonest and arrogant of me to pretend that all is rosy in the quantum garden. Many physicists [...] feel strongly that there is no problem. They would argue that drawing attention to the conflicts between different interpretations of what is, after all, a perfectly well understood, logically consistent, and successful mathematical theory is unnecessary and pointless.

²³⁰ Another meaningful reference is surely (Freire 2014).

[...] Let me begin with a strong but nevertheless accurate statement: No one interpretation of the quantum formalism has been proven to be any better than the rest, other than on aesthetic grounds or personal taste. This has therefore suggested to many that it is a futile exercise to argue the relative merits and shortcomings of the various interpretations. Worse still, many believe that there is no true interpretation and that they are all equally valid ways of thinking about what is going on. This view is embodied in the widely quoted 'shut up and calculate' interpretation, which suggests that since it has proven (so far at least) impossible to find the right interpretation, it is a waste of time talking about it. Let the philosophers worry about such issues while physicists get on with using the quantum mechanical formalism to learn about nature.

For over half a century most serious physicists frowned upon issues of interpretation. They argued that quantum mechanics is unique among scientific theories in that, while it has tremendous predictive power, all that it can comment upon, by definition, are the results of measurements. That should be all we need concern ourselves with, and not worry about requiring a unique interpretation in order to make progress. Such a pragmatic, or 'instrumentalist', view is rooted in the philosophy of 'logical positivism' that happened to be popular in Europe at the time that quantum mechanics was born (Al-Khalili 2003, pagg. 119-120).

It goes without saying that the first interpretation I am going to illustrate is the **standard interpretation** (also known as **QM** or the **Copenhagen interpretation** or the **complementarity interpretation**). From the plurality of names to define it, we can already see that it is not a monolithic block. According to (Jammer 1974):

The Copenhagen interpretation is not a single, clear-cut, unambiguously defined set of ideas but rather a common denominator for a variety of related viewpoints. Nor is it necessarily linked with a specific philosophical or ideological position. It can be, and has been professed by adherents to most diverging philosophical views, ranging from strict subjectivism and pure idealism through neo-Kantianism, critical realism, to positivism and dialectical materialism (Jammer 1974, pag. 87).

Hence, a sort of rear and subsequent identification, probably aimed at giving an idea of greater strength and compactness. Indeed, within the standard interpretation are included several views, like for instance, the probabilistic interpretation of QM. According to (Greenberger, Hentschel, Weinert 2009):

[It is] based on Born's 1926 papers and von Neumann's formal account of quantum mechanics in Hilbert space. According to Max Born, the quantum mechanical wave function Ψ does not have any direct physical meaning, whereas its square $|\Psi|^2$ is a probability.

[...] The probabilistic interpretation holds for all quantum theories. i.e., for non-relativistic or relativistic quantum mechanics as well as for quantum field theory. In general, the probabilities for transitions between two quantum states are calculated from the density matrix of a quantum system (Greenberger, Hentschel, Weinert 2009, pag. 485).

The standard interpretation is currently the favoured and most taught interpretation, often identified as the one and only QM, and still considered the simplest one.

In providing recipes for carrying out calculations in quantum mechanics, it is the ultimate tool of the pragmatist and is known simply as the Copenhagen interpretation. Unfortunately, it has nothing to say about some of the deepest quantum mysteries and instead simply side-steps many of the issues (Al-Khalili 2003, pagg. 120-121).

There is actually a plethora of versions of this interpretation which, however, have a number of cornerstones in common. For instance, the measuring apparatus is completely bound to a quantum system and it is impossible (better, meaningless) to try to know something about the quantum system being measured and the macroscopic measuring device, which follows the rules of classical mechanics, even if it is composed of atoms. Moreover, the observer plays a key role. Before a measurement, no one property could be defined as real or existing, but

it must remain suspended in a superposition until we decide what we wish to measure. In this way, certain properties of the quantum system are only endowed with reality at the moment of measurement. Before that, they cannot even be said to exist in a definite classical sense until we measure them. Only the results of measurements are real!

[...] by giving the observer such a privileged status, the Copenhagen interpretation denies the existence of an objective reality that exists in the absence of observation (Al-Khalili 2003, pag. 126, 128).

Another cornerstone is the “collapse of the wavefunction”, a notion firstly introduced by Heisenberg in 1929:

The act of measurement brings about a sudden jump in the state of the measured system from a combination of potential properties to one actual outcome.

[...] the Copenhagen interpretation says nothing about how the process of wavefunction collapse takes place. A strange divide exists in time - before and after measurements - for the quantum system being measured. Before we look, it evolves according to the Schrödinger equation [...] [with] no uncertainties or probabilities. But as soon as a measurement takes place we must abide by a different set of rules which bring in the quantum probabilities (Al-Khalili 2003, pagg. 126-127).

According to the 2009 book by Manjit Kumar²³¹, the rapid spread and acceptance of the “spirit of Copenhagen” was due to three factors:

The first was the pivotal role of Bohr and his institute. [...] Bohr had managed to create an institute with [...] the sense that anything was possible. [...] While [Einstein] usually worked alone, or later with an assistant who carried out the calculations, Bohr fathered many scientific children. [...] Secondly, around the time of Solvay 1927 a number of professorships became vacant. Those who had helped create the new physics filled nearly all of these. [...] Lastly, despite their differences, Bohr and his younger associates always presented a united front against all challenges to the Copenhagen interpretation (Kumar 2009, pagg. 276-277).

It is worth adding to the last statement that, starting from 1930, Heisenberg, Pauli and Dirac, three relevant founding fathers of QM interpretation, published three seminal and widely disseminated books, which surely contributed to publicize and spread their interpretation.

To sum up, QM sounds really a counter-intuitive theory and his founding fathers like Bohr, Heisenberg and Pauli rejected as metaphysical all different attempts to explain how nature really behaves, simply taking into considerations the outcome of measurements. Surely the standard interpretation of QM lead to a huge number of paradoxes, mainly associated with wave-particle duality, wave function collapse, quantum nonlocality, and

²³¹ *Quantum. Einstein, Bohr and the Great Debate About the Nature of Reality.* (Kumar 2009) in the following.

relativity quarrels. The main ones are known as Schrödinger's cat, de Broglie's box, Wigner's friend, Wheeler's retarded choice and so on, and Selleri deeply described all of them in his books.

I decided to analyze the standard interpretation with a huge amount of citations, including one to (Al-Khalili 2003) because we will see how Selleri criticized this interpretation on very similar grounds but already set it out at the end of the 1960s.

Another interpretation is the **de Broglie-Bohm interpretation** (also referred to as **causal interpretation** or **hidden-variables interpretation**)²³².

Louis de Broglie was maybe the first to develop a serious, well built alternative to the standard interpretation, the so-called "double solution", in which he resumed the dual aspect of matter which he had proposed in his 1924 thesis. According to (Bacciagaluppi, Valentini 2009)²³³ and (Besson 2018), in 1927 de Broglie presented at Solvay conference in Bruxelles only a partial view, i.e., the pilot wave. He made the assumption of a wavefunction as a real physical wave. It had to be guide the particles along their paths. The strong opposition of the follower of Copenhagen interpretation along with the fact that maybe de Broglie could not fully develop his theory, made his attempts futile. Bohr, Heisenberg, Pauli and the others were

[...] completely dismissed of any approach that attempted to revert back to 'realism' as being too pictorial and quite wrong in the quantum domain (Al-Khalili 2003, pag. 129).

But, in 1952 David Bohm exposed in two papers²³⁴ an extension of de Broglie's idea, even though it was nonlocal and it could not be applied to relativistic case.

²³² See also https://en.wikipedia.org/wiki/De_Broglie%E2%80%93Bohm_theory. Accessed 24 September 2020.

²³³ Bacciagaluppi G., Valentini A. (2009), *Quantum theory at the crossroads. Reconsidering the 1927 solvay conference*,

²³⁴ Bohm D. (1952), "A Suggested Interpretation of the Quantum Theory in Terms of "Hidden" Variables, I", *Physical Review*, 85(2), pagg. 166-179. Bohm D. (1952), "A Suggested Interpretation of the Quantum Theory in Terms of "Hidden" Variables, II", *Physical Review*, 85(2), pagg. 180-193.

Anyway, Bohm shown that Bohr, Heisenberg, Pauli and the others in 1927 behaved at least hastily and rushedly, and surely with a dogmatic attitude.

It is worthy mentioning that the de Broglie-Bohm interpretation showed no difference with standard interpretation as regarding the mathematical framework as well as measurable predictions. But they strongly differ in explaining the double slit experiment, in the underlying physics as well as in the describing language of formalism.

As features of this interpretation, there is a wavefunction seen as a real physical presence; a quantum potential which propagates throughout space and guides the quantum particles. Indeed, it is a really strange kind of potential, neither local nor attenuated by distance.

Anyway, a possible explanation for the lack of dissemination and success of this interpretation can be so explained:

[...] why has the de Broglie-Bohm interpretation not caught on? Its supporters still represent a minority among physicists. The usual accusation is that the de Broglie-Bohm interpretation is only successful because it is based on the same mathematical theory as the Copenhagen interpretation.

[...] Other physicists find the extra assumptions about the existence of the quantum potential unnecessary, and are uncomfortable with its, very real nonlocality (Al-Khalili 2003, pag. 132).

[...] Disturbing the quantum potential in one location instantly modifies it throughout the whole of space! And while this nonlocality cannot be used for faster than light signalling, due to the built-in unpredictability of how it will affect any given particle it guides, it is still this aspect of the de Broglie-Bohm view that most people find distasteful (Al-Khalili 2003, pag. 131).

It should also not be overlooked that the standard interpretation was born first and directly from the founding fathers and that it has always been strongly supported and advocated by influential physicists like Bohr, Heisenberg, Pauli, Rosenfeld and so forth, who have always stood together against any alternative to their vision.

Then, we have the **relative states interpretation**, introduced by Hugh Everett III in 1956. Like the de Broglie-Bohm interpretation, it is free of the typical concerns of the foregoing interpretations, namely the measurement problem as well as the bohmian nonlocality. Moreover,

while decoherence fits in comfortably enough in the first two interpretations, it seems [...] to find a home most naturally in Everett's approach or its modern-day variants. [...] But never forget that quantum weirdness must show up in some guise or another. Here, it is in the outrageous requirement that we must inhabit one of an infinite number of parallel universes! (Al-Khalili 2003, pag. 134).

Basically, the Everett's relative states interpretation was

a reaction to his belief that the standard von Neumann-Dirac collapse formulation of quantum mechanics could not be consistently applied to systems which, like the universe, contained observers. Everett proposed solving the quantum measurement problem by dropping the collapse postulate from the standard formulation of quantum mechanics then deducing the empirical predictions of the standard collapse theory as the subjective experiences of observers who were themselves treated as physical systems described by the theory. [...] Everett's goal then was to deduce the appearance of the statistical predictions of quantum mechanics with the collapse postulate, as physical records in the memory of the observer, from pure wave-mechanics without the collapse postulate (Greenberger, Hentschel, Weinert 2009, pag. 363).

Everett abandoned physics research after the lack of support by other physicists and the strong rejection by the "Crusader knights" of the Copenhagen interpretation with the consequent withdrawal by his own advisor, John Wheeler. He did not want to hear anymore about QM. After a few years, the Everett's work back in vogue as **many-worlds interpretation**, thanks to the contribution of Bryce DeWitt in the late 1960s. As stated in (Al-Khalili 2003), the basic idea of this new interpretation is the following:

When a quantum system is faced with a choice of alternatives [...] then, rather than the wavefunction entering a superposition, we think of it, and the whole Universe along with it, as splitting into a number of realities equal to the number of options available. These different worlds/universes/branches will be identical to each other apart from the different option chosen by the particle [...].

The universes overlap, only in that region where interference is taking place, until decoherence sets in. This then causes them to separate into non-interacting independent realities. [...] There is no measurement process any more and the wavefunction never needs to 'collapse'. [...] We, as observers, will also split and so only ever see the outcome of our branch. But there will be other copies of us in parallel universes for whom the alternative outcomes are realized (Al-Khalili 2003, pag. 136).

The aforementioned interpretation introduced by Bryce DeWitt is even known as **multiverse interpretation**, **many-histories interpretation** and **many-minds interpretation**. There is also a variation presented by the physicist David Deutsch in which

all possible universes already exist and do not have to wait until faced with quantum alternatives to split. Deutsch has also proposed a test of his interpretation [...] to see if wavefunction collapse take place or not, since in the many-worlds interpretation it does not (Al-Khalili 2003, pag. 136-137).

As I am displaying, there are a lot of different interpretation, as a further confirmation of a real problem hitherto unresolved. Some of them are currently considered as outdated or defeated by the mainstream vision of the existing theory, even though some of them, according for instance to Selleri, would require further analysis, notwithstanding the following words by Al-Khalili:

Some interpretations have, over the years, been discredited or dismissed, either as being too unlikely or because they were ruled out by new experimental results. For instance, relatives of the de Broglie-Bohm approach, known as local realism theories, were scratched off the list following the experimental results of Alain Aspect [et al.] in the early 1980s [...]. Similarly, hardly anyone still takes seriously the notion of consciousness being a requirement for collapse of the wavefunction. One of the early approaches was the statistical interpretation - interestingly the one favoured by Einstein - which assumes that quantum mechanics only makes statements about whole collections of measurements (of identical quantum systems) rather than on an individual measurement [...]. Since experiments can now be performed on individual systems, even individual atoms, this interpretation requires major clarification by its small group of supporters if it is to survive (Al-Khalili 2003, pag. 137. My underline).

The **statistical interpretation** surely deserves a more accurate description, at least for its wide acceptance in URSS until the end of 1950s, as well explained by Silvano Tagliagambe in his Addendum to chapter XIV in the 1972 Geymonat work, *Storia del pensiero filosofico e scientifico, Volume VI-II Novecento*²³⁵, and in the Chapter 10 of (Jammer 1974). The standard interpretation (called at the beginning Bohr's complementarity interpretation) was strongly rejected in URSS on the basis of the idealistic philosophy behind it, hence totally incompatible with dialectical materialism.

According to Tagliagambe²³⁶:

The substantial difference between complementarity and the theory of quantum sets consists in the fact that, according to the concept of complementarity, the cause of the statistical character of the majority of the statements of quantum mechanics is the alteration, uncontrollable in principle, of the state of the object; whereas, according to the theory of quantum sets, modern quantum mechanics had, from the beginning, to do with real statistical sets. [...] the theory of statistical sets considers that it can dismiss the conception, defined by it as idealistic, of the wave function as characterization of the state of an individual atomic particle: the wave function is preserved, in such theory, as a statistical characteristic, and the square of the modulus of this function characterizes the probability with which the coordinate of the particle can be found. The error of the so-called "physical idealists", an error that leads to the attempt to destroy the materialistic theory of causality, would lie precisely in the fact that they illegitimately confuse statistical laws with those of individual phenomena (Tagliagambe 1972, pagg. 735-736).

Another analysis came from (Jammer 1974), in which the statistical interpretations (also called a statistical ensemble interpretation) is seen as:

an interpretation according to which the state vector provides a description not of the individual system but of an ensemble of identically (or similarly) prepared systems. [...] Einstein [in 1927] proposed a statistical interpretation [...] to avoid the conceptual difficulties which arise if the reduction of a wave packet is described in terms of an interpretation which associates wave functions with individual systems.

²³⁵ (Tagliagambe 1972) in the following.

²³⁶ The original Italian text is in the Addendum 3, p. 970.

[...] all through his life Einstein adhered to the statistical interpretation of the existing formalism even though he hoped that someday a “complete” theory of microphysics would be available which, though established on a conceptual basis different from that of quantum mechanics, would contain the latter as a statistical approximation

[...] The thesis that quantum mechanics predicts the relative frequencies of the results of measurements performed on an ensemble of identically prepared systems is sometimes called the “Einstein hypothesis” in contrast with the “Born hypothesis”, according to which quantum mechanics predicts the probability of the result of a measurement performed on a single system (Jammer 1974, pagg.440-441).

[...] In Soviet Russia, where, [...] Bohr’s complementarity ideas were interpreted as an endorsement of idealistic philosophy and hence as incompatible with dialectical materialism, the statistical interpretation was more favorably accepted as an alternative to the Copenhagen view (Jammer 1974, pag.444).

It deserves to be recalled how the URSS approach to QM was also addressed by Selleri in an in-depth review of Osmelianovski-Fock’s text as well as in articles on newspapers and in two periods he and Augusto Garuccio spent in URSS, too²³⁷.

According to (Jammer 1974), the situation in the Western world sounded different:

In the Western countries, at that time, the statistical ensemble interpretation had very few committed adherents. One reason was undoubtedly the great authority of Bohr, Heisenberg, and the other prominent protagonists of the Copenhagen interpretation enjoyed in the world of quantum mechanics. To understand the second, less obvious reason, we should recall that in those years von Neumann’s impossibility proof of hidden variables was still generally regarded as absolutely conclusive. Since, it was argued, it is an incontestable fact that individual physical systems exist in nature as objects of experimental research, it would be reasonable to expect that a statistical interpretation is only the first step toward a theory that describes the behaviour of individual systems; but such a theory, to confirm with the spirit of the statistical standpoint, would be necessarily be a hidden variable theory whose logical validity had been disproved (Jammer 1974, pagg. 447-448).

²³⁷ I will introduce it later in the chapter.

Recently, it is worthy mentioning also the **transactional interpretation** of QM and the **consistent histories interpretation** of QM, where the ‘history’ is a sequence of quantum events at successive times.

The former is due to John Cramer, is explicitly nonlocal, requiring, however, an instantaneous connections across time. Moreover, it is also relativistically invariant, so it can be used both with the relativistic wave equation and the Schrödinger equation. In addition,

it uses the retarded (Ψ) and advanced (Ψ^*) wave function solutions of these equations in a “handshake” that provides a rationale for understanding the formal structure of quantum wave mechanics and for treating quantum wave functions as physically present in space. In fact, the advanced-retarded transactions are “visible” in the quantum wave-mechanics formalism (Greenberger, Hentschel, Weinert 2009, pag. 795).

The latter was introduced by Griffiths in 1984, further developed by Omnès in 1987, and it could be described in this way:

It is essentially identical to the decoherent histories approach of Gell-Mann and Hartle that first appeared in 1989. [...] In essence, what the consistent histories approach does is to introduce probabilities into quantum mechanics in a fully consistent and physically meaningful way [...] [namely assigning] probabilities to both microscopic and macroscopic states of affairs, using the same formalism for both, without any reference to [acts of] measurement. Actual laboratory measurements can then be discussed in purely quantum terms using the same principles that apply to any quantum process. Hidden variables play no role in [this] approach, which employs the standard quantum Hilbert space. And there is no such thing as a classical world or classical measuring apparatus lying outside the quantum domain. [...] Copenhagen quantum mechanics is a “black box” description in which a macroscopic preparation procedure is followed by a macroscopic measurement outcome, and what happens in between cannot be discussed in terms of microscopic physics if one wants to avoid paradoxes. The consistent histories approach opens the box without generating paradoxes, and thus extends Copenhagen to allow a consistent discussion of microscopic [...] quantum physics in probabilistic terms (Greenberger, Hentschel, Weinert 2009, pag. 117. My underline).

For the sake of completeness, some interpretations based on **dynamical reduction theory** should also be introduced. Maybe it should be fairer in this case to talk about different theories, rather than diverse interpretation of QM.

The most famous one is surely the GRW theory, proposed by Ghirardi, Rimini and Weber in 1986. Another was proposed by Roger Penrose who suggests that gravity can do the job of collapsing the wavefunction. According to Al-Khalili, the approaches based on dynamical reduction theories,

require an extra something that causes the spontaneous collapse of the wavefunction all by itself (in the absence of measurements) every now and again. While the theory can be made to agree with all known observations, the real physical mechanism that would bring about this collapse is still unknown (Al-Khalili 2003, pag. 139).

Eventually, it deserves to take into account also the Ithaca interpretation of QM and the modal interpretation of QM. The former,

was proposed by David Mermin as an attempt to understand quantum mechanics by supposing that the only proper subject of physics is correlation between observables. Further, while correlation are taken to have physical reality, that which they correlate is not. [...] Mermin's presentation of the Ithaca interpretation starts by taking quantum mechanics without the collapse postulate as given. He then seeks to infer what physical reality must be in order for this theory to be taken as providing a complete and accurate physical description (Greenberger, Hentschel, Weinert 2009, pag. 325).

Instead the latter,

seek to solve the measurement problem within no collapse quantum mechanics and to account for the nonlocal correlations in quantum mechanics in EPR-and Bell-type scenarios in a way that might be compatible with special relativity. Various modal interpretations have been proposed, from the mid 1970s onwards, by Van Fraassen, Kochen, Healey, Dieks, Bub and others. These versions are quite different from each other (Greenberger, Hentschel, Weinert 2009, pag 394).

It sounds really intriguing to see, within the classification of the different interpretations, how they faced one of the cornerstones of quantum paradoxes: the double slit experiment.

It was considered a pure abstraction and an impossible thing to achieve in practice by Feynman. Conversely, as highlighted by Giorgio Lulli in his 2013

book *L'esperimento più bello. L'interferenza di elettroni singoli e il mistero della meccanica quantistica*²³⁸, Pier Giorgio Merli, Gian Franco Missiroli and Giulio Pozzi at the University of Bologna and CNR were the first to carry out the experiment in 1974. They published it on *American Journal of Physics* in 1976. But they decided to go beyond the experiment. Indeed:

involving Lucio Morettini, researcher and movie director; and Dario Nobile, head of the LAMEL-CNR research group, which included the same Merli, made a documentary on this experience, entitled "Interference of electrons". This was an immediate success, winning the first prize in the Physics sector at the Science Film Festival in Brussels in September 1976²³⁹.

But the three Italians were not mentioned, and instead the authorship of the experiment was assigned for some time to the work of Tonomura et al. published in the same *American Journal of Physics* in 1989, 13 years after the Italian experiment. It was thanks to the researcher John Steeds, who saw the film, that the right paternity emerged.

The experiment has been extensively studied and discussed, as well as its paradoxical results and its explanation goes beyond the goal of this work²⁴⁰. It is a confirmation of wave-particle duality for atoms in the quantum field. Here instead I wanted to report very briefly how some of the diverse interpretations have addressed it, using the source (Al-Khalili 2003) previously mentioned:

Copenhagen interpretation: since we can never know what goes on behind the quantum curtain without affecting the results, it is a waste of time asking the question and we can only talk about what we *can* see. [...] Thus, not only does the Copenhagen interpretation not explain how the atom goes through the two slits, it states categorically that even to ask such a question is meaningless and we should restrict our comments to the interference pattern on the screen (the measurement).

²³⁸ (Lulli 2013) in the following.

²³⁹ "Coinvolgendo Lucio Morettini, ricercatore e regista; e Dario Nobile, responsabile del gruppo di ricerca del LAMEL-CNR di cui faceva parte Merli, girarono un documentario su questa esperienza, intitolato "Interferenza di elettroni". Questo ebbe immediato successo, vincendo il primo premio nel settore Fisica al Festival del Cinema Scientifico di Bruxelles nel settembre 1976" (Lulli 2013, pag. VIII).

²⁴⁰ An explanation of the double-slit experiment is exhibited, for instance, in Lederman L.M., Hill C.T. (2011), *Quantum physics for poets*, Amherst: Prometheus Books, or in (Al-Khalili 2003, pagg.1-17).

The Copenhagen interpretation succeeds in banishing logical contradictions and inconsistencies by only allowing those questions to be posed which concern the result of measurements (Al-Khalili 2003, pagg. 122;126. *Italics in the original text*).

de Broglie-Bohm interpretation: the atom remains as a localized classical particle throughout and, as such, only ever goes through one of the slits. However, the quantum potential's influence is spread over both slits and it guides the atom along a definite yet uncontrollable and unpredictable path etched out by the pattern of its distribution. [...] The pattern that builds up on the screen is now due to the way the quantum potential behaves on the other side of the slits. The shape of the quantum potential can be thought of as causing each atom to move along a definite, but unpredictable, path. It is only when many atoms have gone through that the true shape of the quantum potential emerges in the pattern of hits on the back screen. The interference pattern disappears when we try to spy on the atoms because we disturb the quantum potential. In the same way, closing one of the slits alters the quantum potential everywhere, causing a change in the possible trajectories of the atoms going through the other slit. Disturbing the quantum potential in one location instantly modifies it throughout the whole of space! In the de Broglie-Bohm interpretation we no longer have to think of atoms as spread-out when they go through the two slits; they are definite, localized particles at all times. But it is important to point out that we can still only calculate probabilities for finding the atom in a certain location, using the wavefunction in the same way as [Copenhagen interpretation] (Al-Khalili 2003, pagg. 129;131-132).

many-worlds interpretation: all possible realities co-exist. The atom goes through a different slit in each universe and the two universes overlap only at the level of the single atom. In each universe, the atom feels the presence of its parallel self which has gone through the other slit. The superposition, and hence interference, is the result of superposition of universes (Al-Khalili 2003, pag. 135).

transactional interpretation: while this interpretation does not help with explaining the central mystery of how the atom manages to go through both slits at once, it does attempt to explain how the atom knows before it goes through the slits whether or not it is being watched. On arriving at the screen, or in a detector close to one of the slits, the atom sends a signal backwards in time that tells the advancing wave how to behave: either to go through both slits and interfere or to go through only one of the slits.

consistent histories interpretation: the atom remains as a particle but it explores all possible paths simultaneously, however unlikely their route. Summed together, all paths cancel each other out leaving just the physical path taken by the atom. But the way the paths cancel depends on what options are available; if both slits are open, more paths are available and the cancellation is different (Al-Khalili 2003, pag. 139).

In conclusion, QM, with all its successes, is still under discussion today, at least in the fields related to the history of science and philosophy of science,

while among physicists, although there is a greater awareness of the importance of history within the development of their discipline, there remains a broad approach linked to the famous “shut up and calculate”, as well as the “publish or perish”, which today, in my opinion, could also be integrated with a “create quantum technology or die”, given the advanced implementation phase for the QM area and the billion euro allocated by the EU for only practical applications of QM.

We have seen a first phase in which quantum theory was introduced and a second phase in defining QM, which seemed wrongly to be the result of a common front and which left no room for further objections, despite the work of great scientists. Anyway, starting in the 1950s and again in the 1970s and 1980s, QM has gone through a phase of crisis of method and ideas with different alternative solutions that came to be presented.

Then, a plethora of alternative interpretations blossomed at that time, certainly due to a widening of interest in FQM, seen as a field of study in which trying to clarify the many dark points of QM at a philosophical, methodological and ontological level. Selleri fits into this dialectical and problematic context, being both one of the promoters of this mass interest in FQM and one of the most prolific and critical authors of mainstream concepts.

Today we can experienced the following landscape, well expressed by Schlosshauer statement:

And so it came to pass that the last two or three decades have witnessed a stunning renaissance of quantum foundations. Today we find ourselves in the fortunate situation where occupation with quantum-foundational matters is no longer inconceivably far from reaching mainstream status. As an indication perhaps, already a good decade ago *Physics Today* [...] ran what seemed like a torrent of articles and opinion pieces on the interpretation (and apparent inconsistency) of quantum mechanics.

[...] To this day, the traditional controversies continue to provide most of the intellectual fuel that keep foundationalists’ engines humming: the meaning of quantum states, the issue of indeterminism, the interpretation of quantum probabilities, the measurement problem, and the question posed by the EPR-Bell scenario.

[...] Finally, [thanks to Bell Theorem as well as Clauser, Horne, Shimony, Holt (CHSH) theorem and] with experimental techniques reaching stunning levels of refinement, our inquiries are no longer confined to the ivory tower of gedankenexperiments, theoretical examination, and philosophical contemplation.

[...] The field of quantum foundations is back with a vengeance. It's here to stay, and its future looks brighter than ever (Schlosshauer 2011, pagg 20-21. *Italics* in the original text).

According to (Freire 2014), all these diverse interpretations sound really useful to science:

[...] tolerance towards diversity may be more helpful to science than strict adhesion to the dominant views. Meanwhile physics gains from the existence of such a diversity of interpretations (AIP 2003).

The above-mentioned quotations refer to several works written in the 20th and 21st century. Now it will be seen how Selleri had already dealt with these problems since the late 1960s.

2.3-INTRODUCING SELLERI ON FOUNDATIONS OF QUANTUM MECHANICS. EARLY PAPERS AND THEMES: 1969-1971.

In the previous chapter I widely analyzed, among diverse topics, the reasons behind the change that Selleri experienced from his research on PP to FQM, roughly in the year 1966-1971, with the breakthrough surely linked firstly to when he read the book of Bernard d'Espagnat, which happened in 1966 and not in 1971-1972 as I tried to demonstrate; and secondly with the period 1970-71 he spent in Nebraska, where he had the opportunity to read a lot of significant books and papers on FQM and on Marxism-Leninism.

Now, at last, I would like to introduce his activity in FQM. Selleri's research on FQM covered more than 40 years, with a deep, intertwined relationship with his social, political, philosophical, historical, and popularizing activity.

As part of the controversy towards the underlying motivations of QM (and certainly not for its results) as well as the different interpretations to microscopic phenomena, born at the embryonic level with the de Broglie's theory of the double solution in 1923-24 and the pilot wave in 1927 and resumed from the 1950s with Bohm, up to the following and current plethora

of interpretations, Selleri surely fitted with his contributions. As can be seen from his letter to Ludovico Geymonat in 1982 (here reported in chapter 1, page 133), Selleri had shown interest in the strange and paradoxical behaviour of the microphysical world as explained by QM very early in his life, when he was still studying at Bologna University, having already held a talk on wave-particle dualism.

In an excerpt taken from the interview with Freire Jr, Selleri highlighted the enormous problems in physics in the middle of 1960s, connecting them to the conceptual abstractness in QM interpretation and in its paltry understanding.

2.3.1-EARLY PAPERS. THAT IS, THE MAIN CORNERSTONES OF HIS APPROACH ALREADY REVEALED

(WITH A SHORT GLANCE ON EMPTY WAVES INSIDE)

The early Selleri's papers show better his clear attitude regarding a deep critic to QM and its strong subjectivism, his acceptance of realism and locality, the role of a realistic philosophical approach in FQM and a continuous proposals of interesting experiments.

I decided to mention here his first four works in FQM, which soon clarified the main topics of his research agenda.

His first paper on FQM was "On the wave function of quantum mechanics"²⁴¹ (further referred to as Selleri 1969a), received at *Lettere al Nuovo Cimento* on 10 May 1969 and therein published on 11 June 1969, subsequent and prior to other key papers on PP, notably on angular momentum nonconservation. (Selleri 1969a) was a really brief paper, only three pages long, but still rich in content. In the following, some excerpts are introduced. Firstly, he started showing his adherence to hidden variables theories by reminding the diverse ones already exposed in the past, including those of de Broglie and Bohm-Vigier; then he proposed his first substantial stance, the opportunity to do experimental check once a physical reality for the wave function has been adopted:

²⁴¹ Selleri F. (1969), "On the wave function of quantum mechanics", *Lettere al Nuovo Cimento*, 1 (17), pp. 908-910.

No physical phenomenon is known which disagrees with the predictions of quantum mechanics (Q.M.). Nevertheless, several physicist found it very hard to accept this theory as basically correct.

[...] Several hidden-variable theories have been proposed, starting in 1926. They arose mainly as a reaction to the abstractedness of the quantum-mechanical axioms and regarded Q.M. itself not as a final description of phenomena but as a theory similar to classical thermodynamics which provides relations for statistical averages over unknown (“hidden”) parameters. Such are, for instance, the Bohm-Vigier theory, and, in some sense, the theory of the double solution of de Broglie.

[...] Furthermore an explicit model theory with hidden variables has been constructed by Bohm and Bub.

[...] It is the purpose of the present note to point out that it should be possible to check experimentally these theories [...] if we adopt the point of view that there must be some sense in which the wave function itself acquires physical reality for any hidden-variable theory (Selleri 1969a, p. 908-909. My underline).

Then, Selleri introduced the claim currently recognized as “empty wave” (or “quantum wave”):

[...] Even though the theories of hidden variables are not completely developed, an important shift of philosophical attitude can be noticed: **particles and waves are now objectively existing entities**. The need for such a shift from a purely logical standpoint has been strongly invoked by philosophers of science [Bunge in 1967]. Once accepted, however, it poses immediately an epistemological problem: what does it mean in practice that waves and particles exist objectively, or, in other words, which are the predictions of the new theories which could not be obtained from Q.M.? This problem is particularly acute for the wave function. In fact a successful hidden-variable theory must state that, even though the waves exist, all of the energy, the momentum, the angular momentum, the charge, and so on are strictly associated with the particle. What is it, then, an entity which exists but has not associated with it any observable physical quantity?

[...] A better solution of the epistemological problem can be found if we notice that, even without any physical quantity associated with it, the wave function could give rise to physically observable phenomena. **In fact we do not only measure energies, momenta and so on. We also measure probabilities**, e.g. the lifetime on an unstable system. **The wave function could acquire reality, independently of the particles associated with it, if it could give rise to changes in the transition probabilities of the systems with which it interacts** (Selleri 1969a, p. 909-910. My underline and bold).

Hence, Selleri was pointing out that we measure energy changing processes but also probabilities. So, the empty wave could reveal its presence by modifying decay probabilities of an unstable system!

In a mail sent on August 1994 to the publishing house Hadronic Press for the institution of a book with his work, Selleri introduced some kind of abstracts of his articles. As regards (Selleri 1969a), which he submitted in a note titled *F. Selleri's papers proposing experiments for the detection of empty waves*, he stated:

To the question: "What is it an entity which exists objectively, but has not associated with itself any physical quantity of the usual type (energy, momentum)?" this paper answers: "The wave function becomes observable, independently of the particles associated with it, if it gives rise to changes in the transition probabilities of the (metastable) systems with which it happens to interact". This is the central idea which will be developed in the later papers (FSPA-My underline).

Now I would like to digress a little with a brief, thorough non exhaustive introduction on the history on empty waves. They were quite a new concept, although in History of Physics it already existed something similar as the Einstein's *Gespensterfelder*, the theory presented by Bohr, Kramers, and Slater (BKS in the following), and the de Broglie's double solution and pilot wave theories. Nevertheless, despite Selleri's empty waves approach referred to Einstein and de Broglie ideas, it sounds rather different, actually, especially for the ability to build an evidentiary experiment.

With regards to Einstein's approach, it could be useful referring to a paper titled *Gespensterfelder*²⁴² that Selleri himself published in 1984.

Here are showed some excerpts in which, besides the description of Einstein's approach to the issue as well as the discarding of this idea by the vast majority of physicists, Selleri felt the need to explain the same concept of Einstein "ghost fields" talking of empty waves. As a consequence of his research on dualism and his realistic view regarding the electromagnetic

²⁴² (Selleri 1984) in the following. It appeared in the proceedings of an international conference he contributed to organize, held in Perugia in April 1982.

radiation, Einstein “was forced to admit the existence of something real (in the sense that it was assumed to propagate causally in space and time) but containing neither matter, nor energy-momentum” (Selleri 1984, pag.101). Then Selleri stated that:

[...] A problem coming immediately to one’s mind within Einstein’s philosophy is the following: if the localized particle carries all the energy and momentum in which sense can the wave be considered real? This problem was felt so acutely by Einstein that he referred to these waves as *Gespensterfelder* (ghost fields); an object without energy and momentum is in fact unable to exert a pressure when hitting other bodies, which means that it does not have that quality which makes us call real a normal object.

[...] The difficulties associated to the conception of an empty wave (here and in the following these two words are taken as equivalent to “wave propagating in space and time but devoid of energy-momentum”) have led many people to discard the idea as a scientific impossibility: if the wave is empty it cannot induce changes into physical objects; hence it cannot be observed; therefore it does not make any sense to postulate its existence. This is probably one of the reasons of the rejection of Einstein’s dualism from the large majority of the physicists [...] (Selleri 1984, pagg. 101-102. My underline and *Italics*).

In another part of the abovementioned 1984 paper, Selleri, while presenting some possible experiments which could in principle allow to settle the question that: “It is the wave and not a localized particle-like structure which induces stimulated transitions in atoms” (Selleri 1984, pag. 108), clearly underlined that it refers only to energy-momentum conserving processes, because:

[...] the BKS-idea of nonconservation is untenable after so many atomic and subatomic processes have always shown a complete consistence with the conservation laws (Selleri 1984, pag. 108).

Bohr, Kramers, and Slater spoke in their BKS theory about a virtual radiation field, or a virtual wave, which could only produce some stimulated transitions between distant atoms, without carrying neither energy nor momentum. In this way the atomic transitions could violate the conservation laws for individual events, which assumed only a statistical mean value.

The theory was completely surpassed by the Compton-Simon and the Bothe-Geiger experiments and by Wolfgang Pauli, who crushed the article²⁴³.

The de Broglie's theory of double solution and pilot wave was presented in 1923-24. It provided that quantum objects consist of a physical wave propagating in a real three-dimensional space and a singularity which stand in for particle and belongs to the wave. Later, in 1927, he proposed in his paper "Wave mechanics and the atomic structure of matter and of radiation"²⁴⁴ the so-called pilot wave theory, the first known example of a hidden-variable theory. According to (Bacciagaluppi, Valentini 2009), in the Solvay conference in 1927, de Broglie:

[...] proposed the [nonrelativistic] many-body pilot-wave dynamics for a system of [N] particles, with the total configuration guided by a wave [function] in configuration space, and not just (as is generally believed) the one-body theory in 3-space (Bacciagaluppi, Valentini 2009, pagg. X-XI).

[...] de Broglie's ideas about particles as singularities of 3-space waves are mentioned only briefly. [...] No use is made of the principle of the double solution. [...] de Broglie simply postulates the basic equations of pilot-wave dynamics, without trying to derive them from anything else (Bacciagaluppi, Valentini 2009), pag. 74; pag. 76).

The official historiography explained how in Solvay 1927, de Broglie's work was strongly rejected by the group of scholars related to Copenhagen interpretation (specifically Pauli and Kramers, even if normally it is cited only the former), contributing to push him to the partially renunciation of his ideas until 1952. But in (Bacciagaluppi, Valentini 2009, pag 248), the author show how de Broglie "contrary to a widespread misunderstanding" answered well to Pauli, not so well to Kramers; that the French physicist had a very wider idea on the issue; and then in Solvay 1927 he did not submitted the double solution theory but the pilot wave idea.

²⁴³ Bohr tried again to refute the energy-momentum conservation during his studies of the β -decay but he failed again after the discovery of neutrino.

²⁴⁴ A deep analysis of this underestimate work has been well developed by Bacciagaluppi and Valentini in their 2009 work, *Quantum theory at the crossroads. Reconsidering the 1927 solvay conference*. (Bacciagaluppi, Valentini 2009) in the following.

As reconstructed by (Bacciagaluppi, Valentini 2009) and (Besson 2018), it is worth reminding that de Broglie considered the pilot wave as a provisional solution. Also because, before 1927, he refuted the configuration space, truly thinking to a three-dimensional real space for describing quantum phenomena, still rejecting the Schrödinger equation defined in the 3N-configuration space. Which, however, must be said, is a simpler solution for explaining a many particle system.

In 1993, thanks to the work of Holland and Bohm and Hiley²⁴⁵, the “pilot-wave theory - as originated by de Broglie in 1927, and elaborated by Bohm 25 years later - is now accepted as an alternative (if little used) formulation of quantum theory” (Bacciagaluppi, Valentini 2009, pag. 32).

John S. Bell reviewed the de Broglie’s work in two papers, namely “Louis de Broglie’s guiding wave” and “Note for a history of the pilot wave”.²⁴⁶

After at least 25 years after de Broglie’s “defeat” at 1927 Solvay Conference, in the years between 1951 and 1954, Jean-Pierre Vigier and David Bohm made a deep work in order to rebirth the idea of the pilot wave. The French physicist published his papers in 1951²⁴⁷, while Bohm released his two pivotal works in 1952²⁴⁸ in which he introduced his causal, deterministic and nonlocal approach to quantum phenomena. Both scholars in 1954²⁴⁹ proposed a model related to the hydrodynamics interpretation, also to answer to 1927 Pauli’s objections to de Broglie. According to Freire 2014:

In 1954, Bohm and Vigier were able to prove that under certain general conditions any function could become a solution of the Schrödinger equation.

²⁴⁵ Holland P.R. (1993), *The Quantum theory of motion: An account of the de Broglie-Bohm causal interpretation of quantum mechanics*, Cambridge: Cambridge University Press; and Bohm D., Hiley B.J. (1993), *The undivided Universe: An ontological interpretation of quantum theory*, London: Routledge.

²⁴⁶ The latter unprecedented work has been discovered by Angela Laurora, during her researches for her 2014 PhD dissertation titled *John Bell e le ricerche sul paradosso di Einstein, Podolski e Rosen*.

²⁴⁷ Vigier J.P. (1951), “Introduction géométrique de l’onde pilote en théorie unitaire affine”, *Comptes Rendu Hebdomadaires des séances de l’Académie des Sciences*, 233, pp. 1010-1013. (Vigier 1951) in the following.

²⁴⁸ Bohm D. (1952), “A Suggested Interpretation of the Quantum Theory in Terms of “Hidden” Variables, I”, *Physical Review*, 85(2), pp. 166-179.

Bohm D. (1952), “A Suggested Interpretation of the Quantum Theory in Terms of “Hidden” Variables, II”, *Physical Review*, 85(2), pp. 180-193.

²⁴⁹ Bohm D., Vigier J.P. (1954), “Model of the causal interpretation of quantum theory in terms of a fluid with irregular fluctuations”, *Physical Review*, 98, pp. 208-216.

To achieve this, they used an analogy between Bohm's approach and the hydrodynamic model suggested by Erwin Madelung in 1926²⁵⁰, which embedded microscopic quantum particles in a subquantum medium with random fluctuations. Thus, the “molecular chaos”, an idea Bohm had abandoned after his discussions with Pauli, came back into his work with Vigier (Freire 2014, pag. 43).

For Bacciagaluppi, and Valentini, de Broglie's theory resemble the Bohm's one of 1952, being the former based on a first order/velocity-based dynamics, whereas the latter is founded on a second order/acceleration-based dynamics. And “de Broglie's first order approach to the theory of motion seems more naturally cast in terms of a dynamics in configuration space” (Bacciagaluppi, Valentini 2009, pag. 86).

Bohm derived his theory by Schrödinger equation, whereas de Broglie's double solution was inserted in a relativistic framework. Bohm tried to answer to the critics, still promising a further relativistic completion of the theory. Bohm-Vigier collaboration was therefore involved in the research by the former for this relativistic widening of his theory and in the work of the latter in the relativistic path for a while. Indeed, according to (Bresson 2018), Vigier collaborated a lot with de Broglie and worked on General Relativity in a field close to the unitary theory of Marie-Antoinette Tonnelat. His publication, (Vigier 1951), was asked by Bohm via Schatzman:

Vigier's work is in line with [Marie-Antoinette] Tonnelat's work in unitary theory. Starting with the antisymmetric metric tensor introduced by Schrödinger in 1948, Vigier proposes a geometric method to find the Lagrangian fields of the particles of Klein-Gordon (spin 0), Dirac (spin $\frac{1}{2}$) and Proca (spin 1). This work was the subject of a first publication at the CRAS presented by Louis de Broglie in March 1951 [Vigier 1951] [...] It is very likely that Bohm would be leading this article that Schatzman passed on to him. [...] Vigier's research took an unexpected turn when he became aware of Bohm's theory. Vigier was immediately enthusiastic about the theory²⁵¹ (Bresson 2018, pag. 74).

²⁵⁰ And picked up by Takehiko Takabayashi (Bresson 2018, pag. 129).

²⁵¹ “Les travaux de Vigier s'inscrivent dans ceux de [Marie-Antoinette] Tonnelat en théorie unitaire. À partir du tenseur métrique antisymétrique introduit par Schrödinger en 1948, Vigier propose une méthode géométrique pour retrouver les lagrangiens des champs des particules de Klein-Gordon (spin 0), de Dirac (spin $\frac{1}{2}$) et de Proca (spin 1). Ces travaux font l'objet d'une première publication aux CRAS présentée par Louis de Broglie en mars 1951 [Vigier 1951].[...] C'est en toute probabilité cet article que Bohm animera que Schatzman lui

Always according to (Bresson 2018, page 97), Vigier developed a research program which included an alignment of the theory of the double solution towards nonlinear equations with general conditions on the solutions, valid whatever the type of equation. And Vigier also hoped that the double solution theory could be a way to gravity and quantum theory.

The research program by Bohm-Vigier was characterized by causality and nonlocality, with a nonlocal quantum potential, defined in the configuration space.

Indeed, the description of quantum phenomena in the real space faced many theoretical problems, due to the intrinsic nonlocality of QM. And Bohm was always less hindered by nonlocality respect to de Broglie and the first Vigier.

Going back to Selleri's early papers, maybe a further rise of the issue started precisely with (Selleri 1969a), but in a diverse approach, always realistic and causal but local. In a special 2013 issue of the Journal *Isonomia*, fully devoted to the 75th Selleri's birthday, the Italian philosopher of science Gino Tarozzi, in his contribution²⁵², always talking about the problem of considering as real a wave carrying neither energy nor momentum, deeply claimed that:

Franco advanced a new realistic interpretation of the wave function of quantum mechanics based on the introduction of a new concept, that of empty wave (to which will be preferred later on that of quantum wave), which can be considered as an attempt to synthesize three²⁵³ different conceptions of the dual nature of microscopic objects, historically supported by the main founding fathers of quantum theory²⁵⁴ (Tarozzi 2011, pag. 233).

transmette. [...] les recherches de Vigier prennent un tournant inattendu quand il prend connaissance de la théorie de Bohm. Vigier est immédiatement enthousiasmé par la théorie [...]” (Bresson 2018, pag. 74).

²⁵² “Oltre la fisica normale. Realtà della funzione d’onda e delle proprietà fisiche prevedibili nell’interpretazione di Selleri della meccanica quantistica”, in Tassani I. (2013) eds., *Oltre la fisica normale. Interpretazioni alternative e teorie non standard nella fisica moderna*, Isonomia (a special issue published for Selleri’s 75th birthday);. (Tarozzi 2011) in the following.

²⁵³ Einstein, de Broglie and BKS.

²⁵⁴ “Franco avanzò una nuova interpretazione realistica della funzione d’onda della meccanica quantistica basata sull’introduzione di un nuovo concetto, quello di onda vuota (alla quale sarà preferito in seguito quello di onda quantistica), che può essere considerato come un tentativo di sintesi rispetto a tre diverse concezioni della natura duale degli oggetti microscopici, sostenute storicamente dai principali padri fondatori della teoria quantistica” (Tarozzi 2011, pag. 233).

However, Selleri spoke very diversely on his “new concept”. In (AIP 2003), referring to (Selleri 1969a), he stated to Freire:

In 1969 for me conceiving the idea of that little paper was a revelation, because I had been educated in believing that the double slit experiment is something that cannot be explained by any intuitive approach. Instead I basically rediscovered something that of course was very well known to de Broglie, Bohm, and other people, I discovered that real waves associated with real particles in principle explain everything. That was the so called dual approach. Real waves and real particles propagating together. If you have that, you can understand the double slit experiment without any difficulty, even though many important books claim that it is a phenomenon going beyond the human reason. In that paper there was also an idea concerning neutrinos and the possibility of using them to discover the reality of the waves (AIP 2003. My underline).

In (Selleri1969a), Selleri also suggested an experiment on the opportunity of detecting the Empty Waves using neutrinos, soon pointing out his detailed confidence of the remarkable importance of experiments to check different theories in order to discriminate between them:

An experiment [...] can be the following: a continuous beam of neutrinos traverses a piece of matter in which unstable entities (nuclei, excited atoms or molecules) are contained. The lifetime of these entities is measured in such conditions and compared to the lifetime of the same entities in the absence of any passing beam. If a difference is observed, its only logical explanation is that it is due to the action of the wave function, since the neutrinos are extremely weakly interacting particles and only a few of them, at most, can have interacted in the piece of matter with presently available neutrinos intensities (Selleri 1969a, p. 910. My underline).

Another peculiarity about (Selleri 1969a) is that, being his first paper in a new topic of physics, Selleri decided to send the preprint of his article to Louis de Broglie²⁵⁵, as we can see from the answer by De Broglie himself to Selleri, dated 24 February 1969, quite early before the publication of the paper²⁵⁶:

²⁵⁵ Selleri experienced a long correspondence with Louis de Broglie, and the following letters I found and used are those collected in FSPA.

²⁵⁶ Unlike Selleri’s interview to Freire (AIP 2003) in which he talked to the Brazilian historian about having received this letter after the publication of his article.

I received with great interest the article “On the Wave function of Quantum Mechanics” because I saw that you were coming up with ideas very similar to mine²⁵⁷ (Louis de Broglie to Selleri, 24 February 1969-FSPA).

In another letter, dated 11 April 1969 (one month before sending the paper for publication), the French physicist answered to the proposal concerning the above-mentioned experiment, underlining his preference for photons, which Selleri will use in the following:

The experience you imagined to prove the existence of the [empty] wave [...] would be very interesting to prove the existence of this very weak wave that would transport the particles, but it would probably be very difficult to do with neutrinos. On the contrary, the improvement of the images for the apodization process is common in laboratories and I think that it already brings the advantage that the sustained photons are transported by a very weak wave that undergoes an absorption of a classical type when passing through a medium as I pointed out in one of the notes I sent you²⁵⁸ (Louis de Broglie to Selleri, 11 April 1969-FSPA).

To lend faith to another letter that de Broglie sent to Selleri on 13 April 1971, the two have just met. Selleri went with Vigier, who acted as a liaison between the two²⁵⁹. In this letter de Broglie analyzed the Selleri’s proposal of the above experiment, being quite skeptical:

[...] I was very pleased to meet you during the visit you made to me with Mr Vigier and to enter into a relationship with you. I have studied the work that you have transmitted to me, but I think that the experience you propose would not be convincing. Indeed, let us first of all put forward a currently “orthodox” point of view, those who admit [...] that the photons which arrived behind Young’s screen under riparted “in the potential state” throughout the region between Young’s screen and plane P where the interferences can be observed. For them, the photons are “everywhere present” in this whole region.

²⁵⁷ “J’ai reçu avec un très grand intérêt l’article “On the Wave function of Quantum Mechanics” car j’ai vu que vous arriviez à des idées très voisines des miennes” (Louis De Broglie to Selleri, 24 February 1969-FSPA)

²⁵⁸ “L’expérience que vous avez imaginé pour prouver l’existence de l’onde [...] serait très intéressante pour prouver l’existence de cette onde très faible qui transporterait les particules, mais elle serait sans doute très difficile à faire avec des neutrinos. Au contraire, l’amélioration des images pour le processus de l’apodisation est courante dans les laboratoires et je pense qu’elle apporte déjà la preuve que les photons sont transportés par une onde très faible qui subit une absorption d’un type classique en traversant un milieu ainsi que je l’ai signalé dans un des notes que je vous ai envoyées.” (Louis De Broglie to Selleri, 11 April 1969. FSPA).

²⁵⁹ Although in a letter of 1976 it seems that de Broglie must still meet Selleri, according to the word of the French Physicist: “I know you will come in early April and it will be a great pleasure to make your acquaintance” (de Broglie to Selleri, 18 March 1976. FSPA).

It is only when a photon manifests its presence at a point on [plane] P that it is located at that point. Of course I think this opinion is not acceptable, but it is still generally accepted. So the orthodox say that photons pass through both Young's slits and that your experiment proves nothing.

If, on the contrary, we adopt the point of view of the double solution theory, we must say that each photon passes through one of Young's slits and we introduce what I call the "guidance trajectories" which are defined at each point by the Poynting vector. If the photon regularly follows a guiding trajectory, your reasoning would prove that a photon which has passed through one of the Young's slits must necessarily arrive on the half of the plane P which faces this slit and your experiment would be conclusive²⁶⁰ (Louis de Broglie to Selleri, 13 April 1971-FSPA).

Then de Broglie explained his approach which revealed his link with the Bohm and Vigier's theory of the levels of organization of nature, with a materialistic vision of the subquantum levels of nature²⁶¹, which for Selleri is unknown or absurd, given that the statement of the French Physicist is marked in pen by Selleri with question marks:

??

However, it is certain that the photon does not regularly follow a guidance trajectory, but continuously undergoes random perturbations of subquantum origin which cause it to pass from one guidance trajectory to another²⁶² (Louis de Broglie to Selleri, 13 April 1971-FSPA).

²⁶⁰ "[...] J'ai été très heureux de vous rencontrer pendant les visites que vous m'avez faites avec M. Vigier et d'entrer ainsi en relations avec vous. J'ai étudié le travail que vous m'avez transmis, mais je pense que l'expérience que vous proposez ne serait pas convaincante. En effet, plaçons-nous d'abord un point de vue actuellement "orthodoxe", ceux qui l'admettent [...] que les photons qui sont arrivés derrière l'écran d'Young sous ripartis "à l'état potential" dans toute la région comprise entre l'écran d'Young et le plan P où l'on peut observer les interférences. Pour eux, les photons sont "everywhere present" dans toute cette région. C'est seulement au moment où un photon manifeste sa présence en un point du [plan] P qu'il se localise en ce point. Naturellement je pense que cette opinion n'est pas acceptable, mais c'est celle qui est encore généralement acceptée. Les orthodoxes disent donc que les photons passent à la fois par les deux trous d'Young et que votre expérience ne prouve rien.

Si, au contraire, on adopte le point de vue de la théorie de la double solution, on doit dire que chacun photon passe par l'un des trous d'Young et l'on introduit ce que j'appelle les "trajectoires de guidage" qui sont définies en chaque point par le vecteur de Poynting. Si le photon suivait régulièrement une trajectoire de guidage, votre raisonnement prouverait bien qu'un photon qui a passé par l'un des trous d'Young doit nécessairement arriver sur la moitié du plan P qui fait face à ce trou et votre expérience serait probante" (Louis de Broglie to Selleri, 13 April 1971-FSPA).

²⁶¹ See (Bresson 2018), chapter five, page 160-163.

²⁶² ?? "Mais il est certain que le photon ne suit pas régulièrement une trajectoire de guidage, mais subit continuellement des perturbations aléatoires d'origine subquantique qui le font passer d'une trajectoire de guidage sur une autre" (Louis de Broglie to Selleri, 13 April 1971-FSPA).

Finally, de Broglie concluded by reiterating that the Selleri experiment would not be demonstrative:

We then see that a photon which has passed through one of the Young holes can be brought by subquantum perturbations on a guidance trajectory corresponding to the passage through the other Young hole. I therefore believe that, even from the point of view of the double solution, the experiment you propose would not be demonstrative²⁶³ (Louis de Broglie to Selleri, 13 April 1971-FSPA. My underline).

The letters sent by Selleri to de Broglie have not been found on FSPA²⁶⁴ and therefore I have no confirmation that the experiment with neutrinos proposed in his preprint by Selleri may have been fully introduced and/or developed also in one of his letters before April 1969. Namely, de Broglie did not speak about the experiment in his first reply to Selleri dated 24 February 1969, when Selleri had already sent his preprint with his proposal inside. It could be that Selleri wrote about his experiment in a more detailed way before 11 April 1969. Albeit de Broglie started his answer of 11 April 1969 giving thanks to Selleri for his “kind letter”, maybe confirming the probable presence of another Selleri’s letter prior to April 1969, sent jointly with the article or being independent from it. It also deserves to bear in mind that de Broglie continued in promoting Selleri’s first paper and research:

Dear Sir, I was very pleased to receive your letter and to see that you are continuing your reflections on the interpretation of Wave Mechanics by the theory of the double solution. The summary that you and your two collaborators have made of certain points in my book “Non-linear wave Mechanics” seems to me to be excellent and I totally agree. [...] I will be very happy to keep in touch with you about your work on the double solution theory and [I send you] all my encouragements [...] ²⁶⁵ (De Broglie to Selleri, 22 March 1970-FSPA).

²⁶³ “On voit alors qu’un photon qui a passé par l’un des trous d’Young peut être amené par les perturbations subquantiques sur une trajectoire de guidance correspondant au passage par l’autre trou d’Young. Je crois donc que. Même en adoptant le point de vue de la double solution, l’expérience que vous proposez ne serait pas démonstrative” (Louis de Broglie to Selleri, 13 April 1971-FSPA. My underline).

²⁶⁴ A visit is going to be planned at the de Broglie Archives at Foundation Louis de Broglie.

²⁶⁵ “Cher Monsieur j’ai été très heureux de recevoir votre lettre et de voir que vous poursuivez vos réflexions sur l’interprétation de la Mécanique ondulatoire par la théorie de la double solution. Le résumé que vous avez fait avec vos deux collaborateurs de certains points de mon livre Non-linear wave Mechanics me paraît excellent et je suis tout à fait d’accord. [...] Je serai très heureux de rester en relations avec vous au sujet de vos travaux

They also had a close correspondence went on up to 1980s. For instance, de Broglie wrote a deep recommendation letter for Selleri's contest for full professorship:

I have just learned that Mr. Franco Selleri, whose work I know well, is a candidate for a chair of Theoretical Physics in his country. Mr. Selleri's research deals with one of the most important problems in wave mechanics, that of the physical interpretation of the wave function and its relation to the prediction of measurement results. I can give a very favourable opinion of his research and I think that Mr Selleri fully deserves to be helped and encouraged, both in his past and in his future work²⁶⁶ (De Broglie to Selleri, 17 May 1980-FSPA).

In one of his many lists about his activity, Selleri emphasized the importance of his first paper in FQM. Already in late June 1969, only two weeks after the publication, he presented his (Selleri 1969a) in a Swedish international conference on elementary particles:

C7. **1969**, 25 June -1 July, Lund, Sweden
 INTERNATIONAL CONFERENCE ON ELEMENTARY PARTICLES
 Organized by: Institute of Physics, Lund
 Talk: On the wave function of quantum mechanics
 (List of international conferences-FSPA).

Another basic Selleri's paper, not published though, is: "Quantum theory and hidden variables, lectures on Frascati National Laboratories"²⁶⁷, written at Frascati between June and July 1969, held there in September 1969, and recollected in a paperback on November 1969²⁶⁸.

sur la théorie de la double solution et [je vous envoie] ainsi que à vos collaborateurs tous mes encouragements [...] (De Broglie to Selleri, 22 March 1970-FSPA).

²⁶⁶ "Je viens d'apprendre que M. Franco Selleri, don't je connais bien les travaux, est candidat à une chaire de Physique Théorique dans son pays. Les recherches que poursuit M. Selleri ont trait à l'un des plus importants problèmes de la Mécanique ondulatoire, celui de l'interprétations physique de la fonction d'onde et de sa rapport avec la prévision des résultats de mesures. Je puis donner un avis très favorable sur ses recherches et je pense que M. Selleri merite pleinement d'être aidè er encouragè, tant pour ses travaux passés que pour ceux qui poursuit" (De Broglie to Selleri, 17 May 1980-FSPA).

²⁶⁷ (Selleri 1969b) in the following.

²⁶⁸ It sounds indicative that Selleri held these lectures in the main Italian Particle Physics Laboratories and in the same year and place in which Adone, the storage ring, started his work for e^-e^+ collisions.

In this twenty-five pages long paper, Selleri introduced from the beginning in a very clearly way his attitude, his adherence to hidden variables theories as well as his methodological approach to the issue.

It represented a key work to meet the ideas Selleri carried on for his whole activity on FQM, more valuable because not published, even if already known:

These lecture notes are divided into five chapters. In the first two the foundations of Quantum Mechanics are reviewed critically. I made no effort to hide my dislike of the philosophical implications of Quantum Mechanics. I hope that the reader will forgive this personal attitude. In the third chapter the realistic postulate is stated and some problems connected with Quantum Mechanics are discussed by means of the new standpoint. [...] In the last two chapters we review some hidden variable theories and discuss the experiments which could distinguish them from Quantum Mechanics .
These lecture notes should provide an introduction to the field of hidden variables. They do not, however, constitute a comprehensive review to that field. The two main arguments which are missing are de Broglie's theory and Brownian Mechanics. The theory developed starting from the formal analogies between Quantum Mechanics and the Brownian motion (Selleri 1969b, pag. 3. My underline).

In the following, Selleri pointed out how impressive is the role of philosophy and metaphysics in physical sciences. Despite the belief of the vast majority of physicists, he showed with several examples how the same QM is intertwined with metaphysics.

So, the “struggle between two cultures”, how it could be appointed, between a factual and operational physics on one hand; and a unnecessary, almost disturbing metaphysics on the other hand, is therefore really far from reality:

The physicists and scientists in general are used to think that philosophy and, in particular, metaphysics are a waste of time and should not be considered seriously by anyone interested in the physical world.
[...] many of us still believe that metaphysics is totally irrelevant because our theories can adapt themselves to any metaphysical point of view.
[...] The conclusion seems to be that physics is more general than metaphysics and that the latter is therefore totally disconnected from the former and therefore irrelevant to the physicist. These foundations of our prejudices are in really rather shaky.

[...] That our theories can always accommodate themselves to any metaphysical point of view is simply not true (Selleri 1969b, pagg. 3-4. My underline).

[...] A very important fact which we wish to point out is that quantum mechanics is deeply interrelated to metaphysics. In fact it does not allow an arbitrary answer to the metaphysical question: does an object exist when we do not observe it? We will show that a positive answer is incompatible with the axioms of quantum mechanics (Selleri 1969b, pag. 4. My underline).

Hence, Selleri claimed on the philosophical negative and constrictive approach of the standard interpretation, which minimize and bypass a lot of objections simply arguing they are just metaphysical, but being unable to resolve some paradoxes, as EPR paradox, for instance:

Notice that as far as we limit ourselves to the prediction of experimental results Q.M. gives the right answer [to EPR paradox]. [...] The paradox disappears if we take the philosophical point of view (as the Copenhagen school did) that such statements do not make any sense and that sensible questions are only those concerning the results of measurements. The point of view that there are objectively existing entities [...] possessing physical attributes leads to the paradox and is therefore not compatible with Q.M.. Therefore [...] Q.M. is not consistent with all the possible metaphysical standpoints and limits in fact our metaphysical freedom. If this is so we should then pay more attention to the philosophers of science to make sure that our different metaphysical assumptions are consistent with each other. In fact the philosophers now claim that they are not (Selleri 1969b, pag. 5).

Then, Selleri introduced several statements in order to move a deep critical to QM's approach as well as to analyze the various paradoxes arising from it. Such criticism, always related to a big research for physical solutions and realistic opportunities, constituted a key topic in his activity:

A further point has to be stressed: the fact that one cannot assume a realistic point of view about the particles participating in our experiments is extremely unpleasant to many of us. You become a physicist to discover the 'secrets of Nature' and after many years you find that there was no Nature and, thus, nothing to discover. What then is physics? Only a description of our activities, of those manipulations which we call experiments. Of course, I am exaggerating.

In fact one can show that Q.M. is only inconsistent with the idea that two systems which have interacted in the past can be assumed to exist separately. But the exaggeration is not very great as an enormous number of interaction has taken place since the birth of the Universe (Selleri 1969b, pag. 6. My underline).

[...] We define [...] as dogmatic a theory whose basic axioms cannot be checked directly with experiments.

[...] Quantum mechanics is [...] a dogmatic theory. In fact some of the most important axioms - the superposition principle and Schrödinger equation - must be formulated for a quantity which is not directly observable, the wave function Ψ . Of course Ψ is used to calculate observable quantities, e.g. the probability density $\rho = |\Psi|^2$, and the predictions so obtained have been checked experimentally in an enormous number of cases, and always found to be correct. Obviously any theory worth of any attention must permit at least an indirect verification of the validity of its axioms. Quantum mechanics is just not directly verifiable, but then, according to our definition, it is a dogmatic theory. **This is a further metaphysical feature of Quantum Mechanics which appears particularly unappealing in a theory insisting so much in the fact that the only events which it makes sense to consider are the measurements** (Selleri 1969b, pag. 7. Selleri's underline. My **bold**).

With regard to the key topic of the reduction (or collapse) of the wave-function, Selleri underlined how the Copenhagen interpretation “resolve” the problem:

[...] When we read the result of a measurement on an instrument our knowledge changes abruptly and so the state vector must change discontinuously too. In this way the physicist becomes an active part in the process. Whether he becomes or not aware of the experimental result will change the wave function (and thus the properties) of the microscopic system.

[...] The conclusions is that psychological phenomena play a role during the measurement processes. This would be like saying that thought influences matter. Unfortunately we have seen that in general we cannot talk of matter as something objectively existing. Therefore the world, as described by Q.M., is more like the one of Fichtian idealism that the one studies by parapsychology (Selleri 1969b, pag. 8. My underline).

In addition, he pointed out how the generality of von Neumann's theorem, a “theorem of impossibility”, was not true, remarking the impressive, seminal

role played by John Stewart Bell with his 1964 and 1966 papers. The solution lies in a unusual class of hidden variables theories, the non-linear ones:

“It is not possible to construct a theory with hidden variables, reducing to Quantum Mechanics when the hidden variables are averaged over”. This is a free version of the famous von Neumann’s theorem, published in 1932, which denies the very possibility of a theory more general than Q.M.. This theorem lead naturally to the attitude that even the few attempts to build theories different from Q.M. were not worth of any consideration.

[...] In 1966, thirty four years after the theorem was published, Bell showed to everyone’s satisfaction that in the proof of the theorem it was implicitly assumed that the hidden variables satisfied certain properties of linearity. It is enough to postulate “non-linear” hidden variables in order to construct a theory against which von Neumann’s theorem cannot apply. A model-theory of this kind was actually proposed by Bohm and Bub. In general we can now say that the door for hidden variable theories is finally open.

We have introduced [...] the concept of “hidden variables” without any explanation of it. Up to a point the words “hidden variables” are self-explanatory: they denote physical quantities which have never been observed in the experiments performed up to now (Selleri 1969b, pag. 9. My underline).

It is worth stressing how Selleri denoted his philosophical realistic approach by defining hidden variables in the above way. It sounded completely different with respect to the Copenhagen interpretation, which denoted them as unseen and unknown, something metaphysical and thus useless.

In the third chapter of (Selleri 1969b), always underlining the unquestionable logical strength and predictive power of QM, Selleri put forward several reasons that the mainstream world of QM opposed against the opportunity to develop an alternative theory based on a realistic approach, underlining the compact front, which is formed since university studies, really convinced that QM is basically a perfect theory. And, by the way, there is a confirmation of his knowledge of the seminal book by d’Espagnat already in 1969:

[...] nobody can say to have proven that Q.M. is internally inconsistent or that it leads to disagreement with experiments. On the contrary it is perfectly logical and has produced an enormous amount of accurate predictions.

Trying to built an alternative theory will then seen to many a total waste of time. It is in fact generally agreed that one should not look for new theories until the old one has been shown unable to explain some empirical facts.

Nevertheless we believe that alternative possibilities should be studied energetically by theoreticians and that experiments to check the foundations of Quantum Mechanics should be performed in large number and accurately by experimentalists.

This for three fundamental reasons:

- a) We finally understood that the theorem of von Neumann is not of general validity. Therefore alternative theories are not excluded anymore by what we know.
- b) The philosophical prejudice in favour of a realistic philosophy is strong in the large majority of physicists. This prejudice did not turn against Q.M. simply because very few people knew its real implications. *The book of D’Espagnat should hopefully contribute to give a better comprehension of them.*
- c) Very few and rough experiments have been made to check the most strange consequences of Q.M..

The third point above is a natural consequence of the acritical acceptance of Q.M. by most physicists. Something you learn at the University as a student cannot be basically wrong. Besides a theory you cannot fully understand must be very respectable. Everything happened as if some kind of uncouscious “credo quia absurdum” had conditioned the scientific community. As a consequence investments of money and efforts on experiments checking the most strange aspects of QM were considered as foolish by the prevailing scientific culture. Nobody should feel surprised therefore, if such experiments have been done very rarely (Selleri 1969b, pag. 9-10. Selleri underline, *My italics.*).

After this long introduction, in order to build a theory with a realistic approach Selleri passed to introduce a line of research “which is not new”, but always referred to as de Broglie and Bohm:

In the following we will propose a line of thought, which is not new, but which seems to us the only way to build a theory of elementary phenomena in accordance with the realistic philosophy. We start from the following Realistic postulate: An elementary particle is always associated to a wave objectively existing.

This postulate is admittedly rather vague. **The only new fact is that the wave is postulated as objectively.** This is certainly in contradiction with Q.M., where even the particle, let alone the wave, cannot be assumed to be objectively existing.

What we claim is that a theory developed starting from the realistic postulate leads to predictions different from those of Q.M. only for experiments which have never been done.

The wave has to be thought of as a real entity in some kind of postulated medium.
 [...] **As we say before, the realistic postulate is not new. In fact De Broglie and Bohm have been working, among others, on this line of thought** (Selleri 1969b, pagg. 9-10. Selleri underline. My **bold**).

The realistic postulate provided for Selleri some advantages. The previous criticisms he made to QM are based mainly on a metaphysical level, because, “QM remains logically rigorous and empirically highly successful” (Selleri 1969b, pag. 11). But a new theory based on the realistic postulate can give some explanations, as, for instance, a plane explanation of the two slits experiment, described by Selleri with clarity:

The realistic postulate should be the starting point for building an equally rigorous and not less successful new theory, that is, we hope that this task may in the future be accomplished. What we can show right-away, however, is that at least our prejudices of metaphysical nature can be satisfied by the realistic postulate.

[...] A new theory incorporating the realistic postulate would not be dogmatic however because the superposition principle and Schrödinger’s equation would be postulated for an objectively existing Ψ and thus necessarily for a directly observable Ψ . Therefore these statements could at least in principle be verified with experiments. How in practice Ψ can be measured “directly” (that is, not merely by observing a probability and equating it to $|\Psi|^2$) is an extremely important problem.

[...] The celebrated two slit experiment [...] could now be explained very simply.

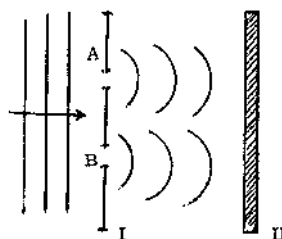


FIG. 3 - Schematic illustration of the two-slit experiment.

A plane wave accompanies a particle with given momentum. When only slit A is open the wave is diffracted and in those cases where also the particle passes through A we find on the second screen II a distribution of probability proportional to $|\Psi_A|^2$.

In fact the association of wave and particle has to be made in such a way that the particle is with greater probability in the points where the amplitude of the wave oscillations is larger, just because one wants to have a theory leading to all successful predictions of QM.

If both slits A and B are open the wave is diffracted through both and in the region beyond I interference takes place. If the particle passes through A or B it will be found on the screen II with a probability density $\rho = |\Psi_A + \Psi_B|^2$ where Ψ_A (Ψ_B) is the wave diffracted through slit A (B). Closing one slit means absorbing one of the waves (Ψ_A or Ψ_B) and the interference is obviously destroyed. One can maintain that the particle has passed either through A or through B in the interference experiment even though this statement cannot be checked experimentally. In other words that the particle passed either through A or through B is a metaphysical standpoint which we are free to take now if we wish so. This was not the case in QM where in a sense there is only the particle and Ψ is nothing physical, but represents only the knowledge which we possess of the elementary system (Selleri 1969b, pagg. 11-12. His underline).

Having found a possible resolution to several inconsistencies presented by QM, Selleri showed a new hidden variable, i.e., the wave foregoing introduced:

The previous considerations lead us naturally to the discussion of the concept of hidden variables. In practice we can say that the physical, objectively existing wave which we have postulated is a hidden variable.

Hidden because it has, until now, never been revealed directly in an experimental manner (how to reveal it directly is already discussed) (Selleri 1969b, pag. 12. My underline).

Then, Selleri dedicated some statement to Bell's theorem, a pivotal paper just published three years before and maybe one of the main argument in Selleri's research on FQM, with a busy and plentiful activity in analyzing Bell's theorem and inequality as well as founding other inequalities (as I am going to show in sections 2.7.3-2.7.7). Selleri immediately underlined the inherent local appearance of the hidden variable theories. Locality sounded as the main, pivotal cornerstone of Selleri's research in FQM.

No local hidden-variable theory can reproduce all of the statistical predictions of Quantum Mechanics. The validity of the previous statement has been proved by Bell a few years ago. The main point is that a hidden variable theory is supposed to be local, meaning in practice that in two regions of space, remote enough from each other, two measuring devices are placed, the results read on the first one cannot depend on the setup of the second one, and viceversa.

[...] $|P(a,b) - P(a,c)| \leq 2 - P(b',b) - P(b',c)$. This inequality deduced from very general considerations following from the postulated existence of local hidden variables, is extremely important because it is in general not satisfied by Quantum Mechanics (Selleri 1969b, pagg. 19-20. My underline).

In (Selleri 1969b) his proposal of experiment with neutrinos made in (Selleri 1969a) is also mentioned, and the Bohm-Bub model, as well as the Papaliolios experiment, are explained.

To introduce all of them, Selleri stated that this new class of experiments:

[...] have the common characteristic of: (a) being rather easy to perform; (b) disproving Quantum Mechanics if they were ever to give a positive result; (c) looking very strange and unusual at first sight. One should not take the third point as an important issue against them. The fact that they look strange and unusual merely reflects the polarization of thought along the canonical patterns which has existed during the last 40-50 years (Selleri 1969b, pag. 13).

As it turned out, (Selleri 1969b), although unpublished, constituted a very significant work, already full of many Selleri's "features" in building a paper: a description of the history and the state-of-the-art of the topic, always citing several scholars who contributed to it; a continue recall to possible experiments as well as an ability in proposing them; a critical approach, however, destined not to remain an end in itself but to propose a search for truth; and a physical and epistemological analysis for the sole purpose of increasing knowledge, despite his philosophical convictions. His approach always received several proofs of esteem and correctness by different scholars, regardless of everyone's scientific beliefs.

Another pivotal paper is surely the one written jointly with Vincenzo Capasso e Dino Fortunato, "von Neumann's Theorem and Hidden-Variable Models"²⁶⁹, and released in *Rivista del Nuovo Cimento* in the second quarter of 1970. It represented a deep review work, fifty-one pages long, with an "abstract" made by an index of ten paragraphs and two appendices.

²⁶⁹ Capasso V., Fortunato D., Selleri F. (1970), "von Neumann's Theorem and Hidden-Variable Models", *Rivista del Nuovo Cimento*, II (2), pp. 149-199. (Capasso, Fortunato, Selleri 1970) in the following.

They performed a precise and deep analysis of the different models as well as a remarkable and detailed state-of-the-art at that time, also introducing with wealth of detail several scholars and their studies on the matter. Then, the authors explain their contribution.

At the beginning, they introduced historically the criticism to the co-founding fathers of quantum theory, i.e., Einstein, de Broglie and Schrödinger, based on the hidden variables, mentioning the case of Boltzmann and the thermodynamics, which enriched the classic thermology as an example of hidden variables in the HoP. Atoms and molecules were hidden variables that no one accepted for many centuries. This last subject was always present in Selleri's papers.

Alternative formulations to quantum mechanics (Q.M.) have been sought ever since the beginning of this theory. In fact even some of the people who contributed most to the development of this theory, such as Einstein, de Broglie and Schrödinger, eventually turned against it. Essentially, the criticism of these authors arose because of the metaphysical implications and because of the rather formal structure of the axioms of Q.M., and certainly not because of limitations in predicting experimental facts or lack of rigour in its logical structure. Many attempts were based on the idea of hidden variables in which a theory more general than Q.M. was sought which could reduce to Q.M. itself when some unobserved (or hidden) variables were averaged over. These attempts were analogous in spirit to the successful generalization of thermodynamics given by Boltzmann with the help of the atomic hypothesis. Hidden-variable theories were proposed by Fürth, Fényes, Weitzel, de Broglie, Bohm and Vigier and many other authors. In the last few years there has been a revival of interest in the field of hidden variables. The most important results obtained is, in our opinion, the theorem by Bell, stating that no local hidden-variable theory can agree completely with Q.M.. What is even more remarkable is that experiment which can distinguish between Q.M. and any local hidden-variable theory have been proposed. These experiments had never been performed before in a form suitable for application of Bell's theorem. Three such experiments are now being performed simultaneously in the U.S. [Holt at Harvard, Clauser at Berkeley, Nussbaum at Bell Labs, NY]. Obviously we will soon know whether Q.M. or the hidden-variable idea is right.

In the present paper we review and discuss critically the theorems which have been presented on hidden variables, starting with von Neumann's theorem and including those of Jauch and Piron, Gudder, and Bell.

We also discuss some examples of hidden-variable models which are most suitable for our experimental verification (Capasso, Fortunato, Selleri 1970, pagg. 149-150).

[...] From a historical point of view, classical thermodynamics precedes the statistical one. Before Boltzmann, physicists did not know that matter has a molecular structure; all that they could experience were the macroscopic aspects (pressure, temperature, ...) of “hidden” (for those times) observables such as the velocity of molecules. We must say moreover that statistical thermodynamics is more successful than classical thermodynamics; [explaining] phenomena (such as Brownian motion) which the second does not foresee (Capasso, Fortunato, Selleri 1970, pag. 151).

Then they retraced the von Neumann demonstration of inability to rederive the results of QM with the hidden variables. Von Neumann discarded all the different opportunities to have a description of quantum processes other than that of statistical one. We know, and here in the paper is well bore in mind, how this absolute generality of the von Neumann’s hypotheses stated by himself was the reason of the failure of his hypothesis of linearity and his theorem as well, opening a gateway to a world in which other theories could explain well the quantum phenomena. By the way, this front door was enabled by John Bell. The three authors pointed out how von Neumann proved only that it is not possible to build a hidden-variable theory which satisfies his complex of hypotheses, and has Q.M. as an approximation.

Then, they reminded the Bohm-Bub model, showing how also this theory did not satisfy the hypothesis of linearity. Doing a remarkable review of criticisms of different type and sources to von Neumann’s theorem, Capasso, Fortunato and Selleri introduced other works made at that time, like Jauch-Piron’s one. Jauch and Piron arrived to a conclusion criticized by Bell, Bohm and Bub and also not accepted by themselves, given that they arrived to prove a theorem whose conclusion

seems to be very strong, because to rule out hidden variables it is sufficient to exhibit two propositions of the physical system which are not compatible.

[...] That conclusion given by Jauch and Piron in 1963 was smoothed by Jauch himself who, accepting the criticisms moved by Bell in 1966, evidenced that his own theorem could not reject all hidden-variable theories because some hypotheses about the structure

of a possible theory were made. Nevertheless he affirmed that these hypotheses were rather reasonable.

[...] we should like to evidence the fact that the same hypotheses of Jauch and Piron does not seem at all reasonable to us and then their result quite disputable (Capasso, Fortunato, Selleri 1970, pag. 165).

Still continuing, Capasso, Fortunato, and Selleri introduced the work by Stanley P. Gudder, looking at it as “interesting [...] to report”, even if Gudder reached the same conclusions of the foregoing scholars:

[...] Gudder, although [...] reformulating hypotheses which are more general than those of Jauch and Piron, does not disagree with them in the results. He proved that hidden variables are possible only in a classical scheme in which all physical quantities are compatible. In order to express more precisely Gudder’s theorem, it will be useful to recall his hypotheses too, which on the other hand are not very different from those of Jauch and Piron, even though they are based on a more realistic point of view.

[...] Gudder’s theorem seems to imply that no physically interesting quantum-mechanical question system admits hidden variables (Capasso, Fortunato, Selleri 1970, pag. 168).

All of them proved the impossibility of hidden variable theories:

[...] von Neuman, Jauch and Piron and at last Gudder, although starting from different logical schemes, demonstrated the impossibility to obtain Q.M. by averaging a more general theory on hypothetical hidden variables. By now we cannot but accept the results of the above authors.

Only when someone will be able to criticize in a satisfactory way Gudder’s hypotheses (just as has been done for those of von Neumann and Jauch and Piron), will we again be able to take into account the possibility of obtaining a theory more general than Q.M. in the sense of the H.V.

Therefore a physicist can now assume one of the following points of view. Although accepting the limits of Gudder’s hypotheses, he must, for the present, abandon the hope that the HV approach may lead to all the results of quantum mechanics when averaged over. Yet, from an epistemological point of view, we can think either of logical structures different from that indicated by Gudder, or of physical theories which, arranged in this structure,

- 1) are not deterministic (then do not follow the ideas of von Neumann’s hidden variables) but have Q.M. as a “limit” (in a wider sense).
- 2) are deterministic but not completely reproducing Q.M., of course.

Bell's theorem can be considered to agree with the latter point of view. This theorem, by using some sort of unknown parameters in order to construct a physics which is again deterministic, not only proves that we cannot reobtain Q.M. in a strict sense, when we average over them, but actually implies results which are in disagreement with it in some cases susceptible of an experimental verification.

In conclusion we agree with Bohm and Bub when they state that: "If an axiomatic structure is confirmed by the facts available at a given time, this can never imply that no other axiomatic structure is possible that could agree with the same set of facts" (Capasso, Fortunato, Selleri 1970, pag. 168).

Specifically, Capasso, Fortunato, and Selleri contribution dealt with the hidden variables and the critic to von Neumann's theorem, and proposing experiments to prove Bell's theorem, still remarking the already definition of an objectively existing wave. This part basically consists in a quite complete reprise, albeit broadened, of the proposal made in (Selleri 1969b).

They wound up the paper with a summary and also underlining the pivotal contribution by Bell and Clauser, Horne, Shimony and Holt (which I introduced in the paragraph related to Bell's inequalities) which opened the opportunity to perform some experiments to differentiate between QM and hidden variable theories.

The aim of this review paper has been to stress that quantum mechanics, like all physical theories, must not be considered as final. We discussed the possibility of obtaining theories more general than quantum mechanics in the light of the famous von Neumann's theorem in the versions given by von Neumann himself, by Jauch and Piron and by Gudder.

The conclusions we can draw from this analysis are the following:

- 1) in principle, we could construct a theory which, although more general than Q.M., contains the indeterminism introduced in sciences by Q.M. itself;
- 2) it is not possible to construct a strictly deterministic theory reducing exactly to quantum mechanics when some hypothetical hidden variables are averaged over.

This last point was stressed also by Bell, who used the locality assumption for the hidden variables. This is, in our opinion, a very natural restriction as nonlocal hidden variables are a very unpalatable dish. The most important conclusions reached by Bell and by Clauser, Horne, Shimony and Holt were that experiments which can distinguish between local hidden variables theories and quantum mechanics have never been performed.

They are being performed, now, however (Capasso, Fortunato, Selleri 1970, pagg. 192-193. My underline).

The fourth and last of the picked early papers, “Realism and the Wave-Function of Quantum Mechanics”²⁷⁰, matches to his talk at Varenna 1970 Summer School on FQM. The proceedings were published in 1971. In less than ten pages, Selleri pointed out very plainly his views based on two standpoint: a realistic philosophy, with locality at its center, against the Copenhagen interpretation; and the description of individual systems by QM. As for his first standpoint, in the introduction Selleri claimed in this way, emphasizing the key role of the social responsibility of the scientist, another seminal topic in his approach:

Let me state from the beginning that I accept the realistic philosophy. It seems to me very difficult to take seriously a different philosophical standpoint, although it is certainly logically possible. The point is that practically nobody in the society of which the physicist is a part has any doubt about the actual existence of an objective reality outside the observer. By objective I mean existing independently on its being observed. [...] Some ideas must be taken as self-evident without the need for any explanation. These are *a priori* ideas, as is well known from philosophy, and the outside reality is one of them.

[...] In this time where the social responsibility of the scientist is so strong, where the destruction or the survival of the world depends also on him, it is important to develop a science not in basic contradiction with the social reality. The foundations of physics (and in particular of quantum mechanics) should then better be based on a realistic philosophy (Selleri Varenna 1971, pagg. 398-399. *Italics* in the original text. My underline).

In Selleri’s thought, science and society have to be related one another. Regarding the individual systems described by QM, Selleri stated:

The second predecision I am going to make is related to quantum-mechanical systems and to their description. Does quantum mechanics describe individual systems or only statistical ensembles?

²⁷⁰ Selleri F. (1971), “Realism and the Wave-Function of Quantum Mechanics”, in D’Espagnat B. (ed), Società Italiana di Fisica, *Foundations of Quantum Mechanics. Proceedings of the International School of Physics “Enrico Fermi”*, Course IL, Varenna sul Lago di Como, 29 Giugno-11 Luglio 1970, New York-London: Academic Press, pp. 398-406. Hereafter referred to as (Selleri Varenna 1971).

[...] I prefer the first possibility and will therefore not hesitate to attribute a wave function to an *individual elementary system*.

[...] As is well known, when quantum mechanics is associated with the two previous predecisions (realistic philosophy and wave function associated to the individual systems) considerable difficulties arise. I certainly have not a general solution to offer, valid for any problem. What I will present are some general considerations which point to possible experiments which have never been performed and which would help in clarifying the situation. I will discuss separately the case of single particles and of two correlated particles (Selleri Varenna 1971, pagg. 398-399. *Italics in the original text. My underline*).

After introducing the two slits experiment, practically recalling the one in (Selleri 1969a), he dealt with the wave function for correlated systems, i.e., the wave function of the first and second type. Using Bell's inequalities applied to particle physics, Selleri showed that the empirical evidence for wave functions of the second type (or even, mixtures state, improper mixtures, or the entangled states as known today) is not overwhelming.

Suppose we have two particles A and B. We say that the system A+B is described by a wave function $\Psi^{(I)}$ of the first type if one can write [...] $\Psi^{(I)}(A+B) = \varphi(A) \cdot \chi(B)$.

[...] In quantum mechanics, wave function of the second type [...] have generally the form $\psi^{(II)}(A+B) = \sum_k C_k \varphi_k(A) \chi_k(B)$ (Selleri Varenna 1971, pagg. 400-401).

Selleri referred again to d'Espagnat's book²⁷¹, introducing mixtures of the first and second type and bringing forward some concepts he will research on in his next papers:

The above definition of $\Psi^{(I)}$ and $\psi^{(II)}$ follows closely the definition of mixtures of the first and second type given in d'Espagnat's book (*Conceptions de la physique contemporaine* (Paris)). The most paradoxical aspects of quantum mechanics can be attributed to the existence of wave functions of the second type. It is not this the place to review these paradoxes which can be found in the quoted book by d'Espagnat, or in Jauch's book, or in many other places. We will simply recall that these paradoxes arise in connection with Einstein, Podolsky, Rosen experiment and with the description of the measurement process.

²⁷¹ Still confirming his deep comprehension and knowledge of the French physicist's book in 1970, even though the paper was published in 1971.

The solution of these paradoxes, at least in the Copenhagen interpretation, consists in denying the possibility of considering as objectively and separately existing two systems which interacted in the past, no matter how far from each other they may be in the present. Such a philosophical standpoint is not consistent with the one which I assumed in the introduction (Selleri Varenna 1971, pag. 401. My underline).

[...] the experimental verifications of Bell's theorem are simultaneously a verification of the existence of second type wave functions in Q.M.

Considering that no such verifications have ever been done and that second-type wave functions are responsible for the paradoxical aspects of QM, it would in my opinion be strange if the experiments being made by Clauser, Holt, Horne, and Shimony were to confirm quantum mechanics (Selleri Varenna 1971, pagg. 402-403).

In conclusion it seems to me that it would not be so terribly surprising if the experiments being performed by Clauser, Holt, Horne, and Shimony were to give a result in contradiction with QM, since it is really the first time that the existence of wave functions of the second type is put to a conclusive test (Selleri Varenna 1971, pag. 406).

To sum up, the early Selleri's papers on FQM already revealed the cornerstones whereon Selleri built his research. He was devoted to wave-particle duality, thanks to the influence of Albert Einstein and the appreciated work of de Broglie. Strictly connected to this, it sounded natural studying the hidden variables theories and the empty waves. He was deeply engaged in the realistic philosophical approach, based on locality, causality and the arrow of time, that is, Local Realism (further referred to also as LR). His criticism to QM and its philosophical and metaphysical shaded areas was aiming at a better comprehension as well as a more linear explication of the quantum phenomena. Selleri has always sought a physical and philosophical clarity in the theory. As underscored many times in these first two chapters, despite being a theoretical physicist, Selleri bore in mind all the time the crucial role inherent in experiments to perform in order to discriminate between QM and hidden variable theories.

In FQM the wave-particle duality was one of the main argument between the different approach to Quantum Theory, and the hidden variables theories were such an early form of rejection of the standard interpretation of QM.

Another objection to QM was the EPR and Bell's Theorem, even though the latter, conceived at the beginning as an attempt in this direction, it turned out to be perhaps the most difficult obstacle to overcome for those who wanted to create an alternative interpretation to QM. A good reason for Selleri to try facing this issue. Indeed, as I will show in the section 2.7, Selleri's activity on Bell's inequalities was a deep, seminal part of his research.

Anyway, coming back to Selleri's early works, which were the reactions to them? Several evidences emerged!

Notably, (Selleri 1969a) received several clarification and encouragement, as we have seen, from de Broglie.

(Selleri 1971) was brought to the centre of Freire's 2003 interview (AIP 2003). Freire underlined the role of realism in physics presented by Selleri at Varenna and asked him about its origin and its role in Selleri's life. Selleri replied with a deep answer, also pointing out once again the remarkable role played in his FQM activity by the time spent in Nebraska in the year 1970-1971 as well as how his realistic attitude helped him with the OPE model in 1960. He introduced for the first (and last) time the name of David Golden, who invited him in Nebraska. I found neither letters between them nor any documents linking them, apart from this important quote in Freire's interview:

I have the feeling that I always was a realist. I remember in high school the philosophy teacher - who was not very good anyway - arguing against realism and I remember I was scandalized. "How can you say that nonsense?" I asked myself. And then of course my choice to be a physicist was basically not because of prestige, but basically out of curiosity to understand the real world [...]. And now when you arrive fully in the scientific activity you find that many people teach and say that the reality is not there. They make you feel that you are misplaced, yes, that you are not in the right place. So I had a very strong rejection of the antirealistic positions - very strong. And for example in one pion exchange model I believed that really there is a pion exchanged. It is visible, so to say, in the right variables. There is a typical maximum which is always there. You can see it. In 1970 I was already in Bari, but I went to the United States for one academic year. I was invited by an American physicist who had been here in Bari, David Golden, who came from the University of Nebraska. So I went to Nebraska for one year. They had a wonderful library and I can say I spent most of my time studying the books and articles in the foundation of quantum mechanics.

And when I came back from that year in Nebraska I knew a lot about the history of quantum mechanics. Actually I decided I wanted to understand how it had been possible that the quantum mechanical antirealistic view was built.

So I started from the biographies of the physicists, twelve physicists, all the top big names - including Jordan, Pauli, Dirac. And I read books written by those people, especially autobiographical books or books exposing the main ideas like for example *Physics and Philosophy*, by Heisenberg.

And then it was in that way I discovered how strong was the opposition to the Copenhagen approach, because soon I found out that Einstein was against, Planck was against, Schrodinger was against, de Broglie was against, Ehrenfest was against. It was not just one man, it was a lot of very important people. And then the problem, which is still open today (perhaps you know the answer, but I don't) is: how is it possible that the existing scientific community was born from such a splitting in the group of the creators and founders? You can say basically that twelve of the most important people were split 50:50 against and in favor of the Copenhagen quantum mechanics. Now from a situation like this normally in any human activity you would find the later cultural community also split, but not in physics. In physics you have 99 to 1. Fifty-fifty for the founding fathers, but 99 to 1 for the people who are active in research. How was it possible, through repression and control of positions and publications? And then also a lot of dogma exists. People do not dare opposing important ideas. They feel that it is too dangerous, but they do not dare anyway (AIP 2003. My underline).

In the above excerpt, some crucial basis of Selleri's approach to FQM are well introduced and explained.

In the same interview, Freire also asked Selleri concerning the close bond between a realistic philosophy and the social responsibility. Selleri claimed that his connection has never changed, neither then nor ever. Above all, in his statement there is another explication of the remarkable social and political significance for Selleri's scientific activity and his choices of a real, local and causal physics in three-dimensional space and time, namely, his willingness to communicate the outcomes to everyone, strictly related to his political belief. Everybody has to find the opportunity to be informed:

Basically I always thought that we get the money to do research [...] and that it is our duty to build a science that can be communicated to everybody. And at those times I was thinking in terms of the working class, the working people.

That is to say, if the only way to understand what I'm doing is to study differential equations or Hilbert space, it is obvious that there is no possibility to communicate because it takes too long to learn, there is too high a threshold. If instead I build physics in three-dimensional space and in time according to the rules of causality then I can communicate my results. Not the technical aspects, but I can tell what I found of how the world really works. I can explain the result I achieved. And I always thought that it is our duty to do just that as much as possible. Seeing that important ideas of the realist people like de Broglie and Einstein and so on were as much as possible forgotten, I understood that the scientific community does not work properly. Because you see, you can of course say Einstein is wrong, anybody can be wrong, but you have to show that he is wrong. You cannot say he is wrong "because I say so." And that proof was missing. Instead it was very clear that in physics fashion plays a great role.

[...] you could see a very large number of theoreticians all over the world working very hard for several years on a problem and then abandoning it without any result. For example I remember a lot of work went on pion nucleon elastic scattering, low-energy elastic scattering. Well, I can tell you that not even the sign of the small phase shifts was predicted when the problem was abandoned, not even the sign. It was a shocking activity to watch. You have a strong collaboration, everybody is doing research on the same problem, but after some years they abandon quickly the problem they were studying and pass to something else [...]. That is the pattern of theoretical particle physics for the strong interactions.

[...] in my opinion the social responsibility is to do something different, to do something for which you can see the progress going on and which you can communicate especially to other people - to politicians, to interested people, to workers and to the taxpayer. So I am very much in favor of a social control. I know it sounds utopian because the reality is very, very different, the reality is a big science in which you can justify the need for big money in a very superficial way, as it is done in the United States and also in Europe. This produces distortions of the scientific culture and it is not good (AIP 2003. My underline).

Perhaps a remarkable proof of the value of (Selleri Varenna 1971) resides in a comment made by Freire in the above interview:

[...] your talk at Varenna School, I think that it is very representative of your own line of approach to foundations of quantum mechanics. Because you maintained indeed your philosophical realistic approach in a very open sentence since the beginning. Second one, you explicitly denied the possibility of the standard quantum mechanics description for two systems. You denied that this kind of description could be a real phenomenon, so you did not accept non-locality. And a third one, you proposed experiments to decide, to see the questions (AIP 2003).

Also (Capasso, Fortunato, Selleri 1970) was a remarkable review paper, often cited as a work on hidden variables and only by referring to surnames, a practice used in physics to emphasize an outstanding work, known to all. During the Garuccio degree thesis, made with Selleri, the former remember how:

We started working on these issues, on the Bell's inequality. Selleri had already worked with Capasso and Fortunato, writing these important articles, this important summary, the work of Capasso, Fortunato, Selleri (when you call a work in this way, like Ghirardi, Rimini, Weber, for instance, it means that it is a classic, super quoted and considered) so the work in a certain sense for me was very simplified (Interview with Augusto Garuccio, May 27th, 2019)

Even Max Jammer, in his pivotal (Jammer 1974) cited (Capasso, Fortunato, Selleri 1970) using only surnames and claiming that:

[...] Gudder's proof of the inadmissibility of hidden variables in quantum mechanics is certainly an improvement over the demonstration proposed by Jauch and Piron. In fact, Gudder's proof seems to have impressed Capasso, Fortunato, and Selleri to such an extent that in their review paper on the hidden variable problem they did not hesitate to make this statement: "Only when someone will be able to criticize in a satisfactory way Gudder's hypotheses (just as has been done for those of von Neumann and Jauch and Piron), will we again be able to take into account the possibility of obtaining a theory more general than quantum mechanics in the sense of the hidden variables"²⁷² (Jammer 1974, pag. 321. My underline).

2.3.2-A PREVIEW ON LOCAL REALISM

Since his first papers, Selleri lined up on precise ontological, methodological and philosophical cornerstones, carrying them on for his whole activity within the FQM, even at the price of clashes, divisions, and isolations: mainly Wave-Particle Duality and Local Realism²⁷³. Among the several definitions Selleri gave on LR, I submit the following one:

²⁷² The statement by the three authors here reported is in (Capasso, Fortunato, Selleri 1970, pag. 168).

²⁷³ I am going to introduce a general glance on LR, still bringing forward some topics I would explained later.

“realism” means that the systems of the atomic world really exist in space and time independently from physicists and their observations, and “local” means that all causes act by direct contact on the places where the effects are produced, i.e. that there are not the famous actions at a distance of newtonian memory²⁷⁴ (Nutricati 1998, pag. 9. My *Italics*).

In several books and papers Selleri revealed all the features of LR. It is based on three grounds: locality, reality and arrow of time. In two 1999 letters, he stated that his paper “Local realism as an antidote to action at a distance”²⁷⁵:

spelling out as clearly as possible the basic concept of local realism and some of its consequences (Selleri to Liu Quan-Hui and to Shen Hui-Chuan, 25 January 1999-FSPA).

In the abstract of the above paper, he pointed out the above importance and claimed how all experiments performed up to the end of 1998 have failed in brighting a resolution of the incompatibility between QM and LR, still proposing a possible solution of the EPR paradox with neutral kaons:

This is a short review paper containing the exact formulation of local realism (reality+locality+time’s arrow). [...] Experiments in particle physics could, however, change the whole picture in the next few years. The predictions of quantum theory, based as they are on action at a distance, are at risk of being falsified (Selleri LR antidote 1999, pag. 307).

In the following, Selleri underscored the definitions and assumptions on which Bell’s theorem is based²⁷⁶. Specifically, separability predicts that:

Any two objects can be separated enough to make their mutual influences arbitrarily small.

²⁷⁴ “‘*realismo*’ significa che i sistemi del mondo atomico esistono davvero nello spazio e nel tempo indipendentemente dai fisici e dalle loro osservazioni, e “*locale*” significa che tutte le cause agiscono per contatto diretto sui luoghi ove si producono gli effetti, cioè che non esistono le famose azioni a distanza di newtoniana memoria” (Nutricati 1998, pag. 9. My *Italics*).

²⁷⁵ Selleri F. (1999), “Local realism as an antidote to action at a distance”, in *Instantaneous Action-at-a-Distance in Modern Physics: “Pro” and “Contra”*, Chubykalo A., Pope V., Smirnov-Rueda R. (eds), Commack, New York: Nova Science Publ., pp. 307-321. (Selleri LR antidote 1999) in the following.

²⁷⁶ Selleri identifies six features, i.e., the dichotomic physical quantity, the EPR-type experiments, the correlation function, the Bell’s measurable linear combination of correlation functions, the EPR reality criterion, and finally the separability.

To put this otherwise, given any two objects, there exists a distance between them, above which the modification on any physical quantity, owing to their mutual interaction, is negligibly small. All propagations furthermore go forward in time and no modification of the past can arise from future events. Separability, so formulated, is based on the following two assumptions:

Locality: *Given two objects A and B, the modifications of A due to anything that may happen to B (collision, interaction with other objects, disintegration, measurement, ...) can be made arbitrarily small, for any measurable physical quantity A, by increasing the A-B separation.*

Time's arrow: *All the effects generated by an event in space make themselves felt only after the event has taken place* (Selleri LR antidote 1999, pag. 311. His underlining and *Italics*).

In the remainder of this important review article, Selleri described in detail the three postulates of the LR. Nevertheless, the same concepts and statement are practically repeated in (Afriat, Selleri 1999)²⁷⁷, where two chapters are devoted to LR and several statements are referred to the pillars of LR. This detail made me lean towards the use of this specific text, being quite sure that the part concerning these topics was developed by Selleri, given the correspondence with his previous and subsequent works. I decided to mention also excerpts by (Selleri LR antidote 1999), and (Selleri Paradossi, 2001), too:

[The EPR paradox has its conceptual background]. The underlying philosophy is that of local realism, which, in addition to realism and locality, includes time's arrow. From these natural and intuitive elements Bell's inequality [...] is rigorously deduced. Of course it is grossly violated by quantum mechanics, which is therefore inconsistent with the principles represented in the inequality (Afriat, Selleri 1999, pag. 31).

[...] Local realism is characterized by the following three postulates:

1. There is an objective reality, which exists independently of observation. [This is the Reality postulate].
2. The interaction between two objects can be made arbitrarily small, even negligible, by increasing their separation. This is the assumption of locality [or the Locality postulate].

²⁷⁷ Afriat A., Selleri F. (1999), *The Einstein, Podolsky, and Rosen Paradox in Atomic, Nuclear, and Particle Physics*, New York/London: Plenum Press. (Afriat, Selleri 1999) in the following.

3. Time has a suitably defined “arrow”, which means that all physical propagations go from the past to the future and that, hence, the past cannot be modified [This is the postulate of the Time’s Arrow] (Afriat, Selleri 1999, pagg. 31-32).

Selleri and Afriat pointed out how the Reality postulate turns out to be anything but a metaphysical concept, as is often wrongly considered:

[...] The idea that objects exist independently of human beings and their observations is so obvious that it is more or less accepted by everyone, including most theoretical physicists. It is, however, sometimes dismissed as being merely metaphysical and as forming no part of a sensible scientific attitude. It is not metaphysical, however, for when it is combined with locality and the arrow of time it has many empirical consequences, which could, in principle, be falsified by suitable experiments, in particular, those that test Bell-type inequalities [of the weak type] with very accurate instruments (Afriat, Selleri 1999, pag. 32).

Selleri also emphasized the complex relationship between observation and nature:

[...] Admittedly the senses give an incomplete and imperfect image of reality. Any correspondence between sense-perceptions and elements of reality cannot be strictly unambiguous. Different situations can be indistinguishable, and similar situations can give rise to different perceptions. The instrument that assist observation, similarly, can introduce distortions and perturbations. The realist will claim that the complexity of this relationship between observation and nature is no reason to abandon a belief in the external reality. The realist can then wonder how to give this view rigor and even empirical substance, rather than leaving it vague and metaphysical. (Selleri LR antidote 1999, pag. 315).

[...] Once the choice of realism has been made, however, it is a matter of not leaving it at the purely ideal level but of making it intervene concretely in scientific reasoning. That is, answering this question: [...] which “elements of reality” can we enucleate from the sea of objectivity? Which properties of which systems should we consider real?²⁷⁸ (Selleri Paradossi, 2001, pag. 135).

²⁷⁸ “Una volta fatta la scelta del realismo, si tratta però di non lasciarla al livello puramente ideale ma di farla intervenire concretamente nel ragionamento scientifico. Si tratta, cioè, di rispondere a questa domanda: [...] quali “elementi di realtà” possiamo enucleare dal mare dell’oggettività? Quali proprietà di quali sistemi dobbiamo considerare reali?” (Selleri Paradossi, 2001, pag. 135).

Then Selleri and Afriat introduced the EPR reality criterion, summarizing the main features of the ‘element of reality’:

1. Its existence does not necessarily depend on an act of measurement. This is consistent with the general hypothesis of realism, which logically precedes the formulation of a reality criterion by means of which single pieces of reality can be recognized in specific situations.
2. It is considered the cause of the result exactly predicted by the measurement, *if* measurement is indeed undertaken. Hence realism and causality are closely connected in the EPR reality criterion.
3. It is also viewed as being determined at least partly by the measured object and not entirely by the measuring apparatus, which has been tested and could, in principle, give results that differ from those predicted. If it can be predicted with certainty that such an apparatus will give a predetermined result interacting with an object, the decisive factor that produces the result of measurement can only be in the object. (Afriat, Selleri 1999, pag. 34. *Italics in the original text*).

Selleri recalled two criticisms to EPR deterministic reality criterion, which will lead to the probabilistic one:

For a realist the EPR criterion has a solid conceptual basis. It is, however, open to two secondary criticisms. To begin with, its applicability is limited as it only recognizes as real, in the atomic domain, what quantum mechanics describes with eigenstates of the measured observable. Why can one not introduce elements of reality for situations described, more generally, by superpositions? [...] The requirement that a physical quantity be predictable with certainty represents the other weakness of the EPR reality criterion: when can absolute certainty ever be reached in an actual physical situation? In practice all kinds of perturbations [...] combine to eliminate true certainty. These difficulties can be overcome by means of a probabilistic generalization of the EPR criterion, in which the *prediction of a probability* is used to deduce the existence of local physical properties of statistical ensembles. Such properties generalize the ‘elements of reality’, and have been studied extensively with the conclusion that Bell’s inequality holds also in this more general framework (Selleri LR antidote 1999, pag. 317. *Italics in the original text*).

Concerning the Locality postulate, Selleri and Afriat also claimed that:

The idea of “locality” is based on the reasonable belief that the intensity of interaction between objects depends inversely on their separation.

[...] Objects may well interact, to some extent, no matter how far apart they are. This is not being denied. [...] the force of gravity, for instance, [...] acts between any two objects and diminishes with the inverse square of the separation. It tends to zero as that separation tends to infinity, but never vanishes for any finite distance. Only something much simpler and easily acceptable is being claimed, namely that at large distances interactions have very small effects, so small that in practice they can be neglected in normal experiments. All known interactions tend to zero as the distance between the interacting objects tends to infinity.

[...] Thus, no known interaction really violates the idea of locality as formulated. [...] Given two separated objects A and B, the modifications of A due to anything that may happen to B [...] can be made arbitrarily small, for any measurable physical quantity, by increasing their separation (Afriat, Selleri 1999, pagg. 35-36).

In (Selleri LR antidote 1999), Selleri stressed also how the formalism of QM lead to put under examination the general validity of locality and emphasized how the above formalism and its abstract configuration space is believed to correspond to the true atomic reality:

[...] locality postulate is clearly of very general validity and there appears to be nothing to which it cannot be applied. The formalism of quantum mechanics is about the only thing that can lead one to question it. In spite of this, however, one is tempted to believe in its universal validity.

It is often objected that the formalism of quantum mechanics does not represent a reality in ordinary space, since the Schrödinger equation for two particles yields a wave that propagates in a six-dimensional configuration space. [...] It is sometimes believed, therefore, that the abstract spaces of quantum mechanics have an irreducible meaning and represent the true atomic reality. The notion of spatial distance may then lose all meaning; and those who embrace locality would appear to be naïve realists who do not understand the subtleties of the quantum formalism (Selleri LR antidote 1999, pag. 318).

Soon after he underlined his thoughts in defense of an objective reality, using philosophy, logic and *buonsenso*, something quite different compared to common sense (according to Selleri's words to Freire):

But the mathematical apparatus of quantum mechanics, although useful and accurate in its empirical predictions, has no objective existence outside our theoretical schemes.

Those who support the contrary are in our opinion naïve Platonists, who do not understand the complexity of the relationship between mathematical formalism and objective reality. Mathematics is a human construction and so are all the abstract spaces used in quantum mechanics. Ordinary space-time has instead a more fundamental role, if only because it has imposed itself on our species for reasons of survival, long before physics was developed. Whoever does not regard this as decisive can be asked to provide a solid scientific reason for the abandonment of space-time. This is a request that so far nobody can satisfy, because all the proposals of an extra-spatial physics have their origin in cultural tendencies and philosophical fashions imported from outside the discipline, without adequate empirical foundation. [...] today the formulation of Bell's theorem has, if anything, aggravated the situation.

Locality has been abandoned by many physicists [...], who have invented worlds in which instantaneous actions at a distance between microscopic objects are somehow possible. Common to all these attitudes is the desire to save our current quantum theory, cost what it may. This is understandable, given the successes of the theory. The real problem, however, is to understand whether the right way out of the paradoxical situation is a denial of very simple and general assertions that have yet to be refuted empirically in any domain, or rather a modification of the existing theory, replacing it with a better one which preserves its correct empirical predictions while becoming compatible with local realism (Selleri LR antidote 1999, pagg. 318-319. My underline).

Selleri stressed many times in his life how QM is undoubtedly a powerful theory. But he disagreed with the way in which the paradoxes which emerge from QM have been addressed and “resolved”. Local realism is the center from which an alternative theory to QM could unfold.

Concerning now the third LR postulate, that of time's arrow, it showed how the effects of an event are always felt after the beginning of their propagation:

The third principle that features in an unmetaphysical characterization of realism is the impossibility of modifying the past. [...] A flash of light directed at the sun will reach it about 8 min after it was produced on Earth [and vice versa].
All the effects generated by a propagation in space and time make themselves felt [only] after the beginning of the propagation (Afriat, Selleri 1999, pag. 37. My underline).

The underlined statement in the previous citation could be seen as the “time's arrow” assumption.

Then, they strongly emphasized the distortions of the contemporary physics, a Selleri's strong point which here, however, revealed itself in all its heavy weight :

Even these assertions can appear to be of obvious validity to the uninitiated; only those accustomed to extravagant fantasies of contemporary physics will not be surprised if they are contested. Naturally, time's arrow has yet to be refuted experimentally.

An abandonment of time's arrow has been proposed more than once recently. Clearly this leads to fantastic scenarios. If propagations from the present toward the past are indeed possible, the past can be modified through choices and decisions made in the present.

This is clearly commonplace in science fiction, which may have its appeal, but which should not be embraced for reasons of cultural or philosophical fashion, and only, if ever, once solid scientific grounds have made it necessary - that is, if physics is to continue being, as one would hope, a serious activity that advances by conjecture and refutation.

Today, instead, the dogmatism that once prevailed has been supplanted by total anarchy, in which it seems that any idea can form the acceptable basis of a scientific development, irrespective of how well founded it may be. This would be less harmful if it were just a matter of provisional acceptance, pending experimental and conceptual clarification. Unfortunately, however, the boundary between the provisional and the definitive has been blurred, and there are fervent supporters of just about anything, including the idea of a propagation toward the past (Afriat, Selleri 1999, pagg. 37-38. My underline).

Sure a substantive, harsh criticism!

According to the authors, this approach related to, and in favour of, propagations toward the past is that antiparticles are to be considered ordinary particles travelling backward in time. Hence, a positron, for instance, would be no more than a normal electron going from the future towards the past. They evoked the recount made by Luis W. Alvarez²⁷⁹ on the discovery of the positron made by Carl D. Anderson (1905-1991)²⁸⁰:

[...] Most physicists would say that the discovery of the positron involved the observation that an electron-like track in a magnetic cloud chamber bent the wrong way.

²⁷⁹ Luis Walter Alvarez (1911-1988): See <https://www.nobelprize.org/prizes/physics/1968/alvarez/biographical/> and https://en.wikipedia.org/wiki/Luis_Walter_Alvarez. Both accessed 24 September 2020. The excerpt submitted here belongs to Alvarez L.W., in *Proc. 1975 Int. Symp. Lepton and Photon Interactions at High Energies*, Stanford (1975), p. 967.

²⁸⁰ Carl David Anderson: See <https://www.nobelprize.org/prizes/physics/1936/anderson/biographical/> and https://en.wikipedia.org/wiki/Carl_David_Anderson. Both accessed 24 September 2020.

[...] Anderson's great discovery of the positron rested entirely on the fact that he knew which direction his positron was going; he placed a lead plate in his cloud chamber and saw the particle lose energy and "curl up" after it went through the plate. Many observers had seen particles that were *consistent* with the positron hypothesis, but Anderson was the first one to be able to *reject* all other alternatives. That is why we recognize him as the discoverer of the positron (Luis W. Alvarez, in Afriat, Selleri 1999, pag. 38. *Italics in the text. My underline.*)

Below I submitted the imagine picked by the authors to explain better how a positive particle is moving upward, having a loss of energy emphasized by an increased curvature. Conversely, travelling toward the past would require a gain in energy and a decrease of curvature. Experiment spoke straight and evident!

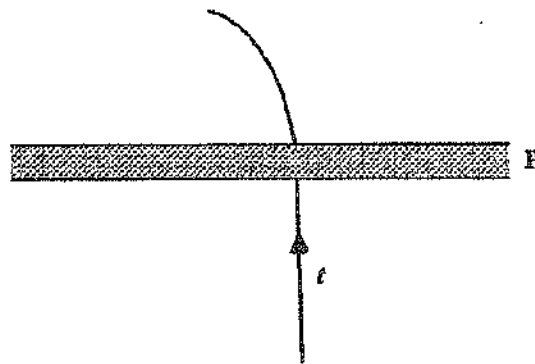


FIGURE 2.1. Anderson's discovery of the positron. The track *t* is produced in a cloud chamber by a positively charged particle moving upward (not by a negatively charged particle moving downward), because the particle loses energy in the lead plate *P*, as is clear from the increase of curvature in the magnetic field.

(Afriat, Selleri 1999, pag. 38)

The conclusion of Afriat and Selleri is clear:

Hence, a positron is *not* a normal electron travelling toward the past. If it were it would have to gain energy crossing the lead slab. Given the complexity of the multiple interactions with atoms that take place in the slab, the probability of an energy gain is extremely low, and its regular observation would be entirely inconsistent with all known laws of probability and thermodynamics. Having dealt with this element of confusion, it can be said that every known phenomenon involves the propagation of matter and/or energy from the past to the future, and never the other way. So time's arrow does exist in nature (Afriat, Selleri 1999, pagg. 38-39. *Italics in the original text. My underline.*)

So, all in all, the three postulates give birth to a realism which is not metaphysical, even if a lot of scholars decided to question them:

These postulates constitute a sort of falsifiable and therefore not metaphysical realism, because it is possible to use them to deduce in a logically impeccable way a whole series of consequences that are numerically incompatible with the predictions of quantum mechanics.

Truly decisive experiments have never been done, but nothing forbids that further technological developments and/or the study of new type of correlated pairs could allow to solve the EPR paradox experimentally²⁸¹ (Selleri Paradossi, 2001, pag. 142).

We have already said that these postulates constitute a form of falsifiable, and hence nonmetaphysical, realism. This is why it is possible to deduce from local realism, with the utmost rigor [...], the so-called [Bell's] weak inequalities, derived without additional assumptions of any kind.

[...] Bohr and Heisenberg have suggested that only a macroscopic reality can be said to exist independently of observation, and that it makes no sense to speak of any such existence of microscopic objects. Einstein disagreed²⁸² (Afriat, Selleri 1999, pag. 39).

[...] Locality has been abandoned by many physicists, such as Bohm, Stapp, and Ne'eman, who have invented worlds in which instantaneous actions at a distance between microscopic objects are somehow possible. Time's arrow has been questioned by Wheeler, Costa de Beauregard, and other physicists who have entertained the possibility of an active modification of the past.

[...] Common to all these attitudes is the desire to save our current quantum theory, cost what it may. This is understandable, given the great successes of the theory. The real problem, however, is to understand whether the right way out of the paradoxical situation is a denial of very simple and very general assertions that have yet to be refuted empirically or a modification of the existing theory, replacing it with one that preserves its correct empirical predictions while being compatible with Einstein's realism. The real choice between these possibilities can only be provided by experiment. There are two possibilities:

²⁸¹ "Questi postulati costituiscono una sorta di realismo falsificabile e quindi non metafisico, perché è possibile usarli per dedurre in modo logicamente ineccepibile tutta una serie di conseguenze che sono numericamente incompatibili con le predizioni della meccanica quantistica. Esperimenti veramente decisivi non sono mai stati fatti, ma nulla vieta che ulteriori sviluppi tecnologici e/o lo studio di coppie correlate di tipo nuovo possano permettere di risolvere il paradosso EPR per via sperimentale" (Selleri Paradossi, 2001, pag. 142).

²⁸² In (Afriat, Selleri 1999) the authors reported the statement made by Einstein at page 7 in (1953), *Louis de Broglie, Physicien et Penseur*, George A (ed.), Paris: Albin Michel. The statement argued that: "There is such a thing as the 'real state' of a physical system, which exists objectively, independently of any observation or measurement, and which can be describe, in principle, with the means of description afforded by physics...I am not ashamed to make the 'real state of a system' the central concept of my approach".

1. The weak inequalities will be found to be in disagreement with experimental results, while quantum mechanics will be confirmed once more.

2. The predictions of quantum mechanics will be violated by these new experiments that do agree with the weak inequalities.

(We shall ignore the pessimistic possibility that experiments violate both the theory and the inequalities!) In the first case the quantum paradigm of modern physics will necessarily have to be accepted; in the second Einstein realism will have survived a very important test (Afriat, Selleri 1999, pagg. 39-40).

Selleri's research on LR allowed to deduce an infinite number of other inequalities, which gave restrictions that cannot be obtained from Bell's. confirming this way how LR is more general.

Selleri put locality at the center of his program, try maintaining it at any cost. The key topics and themes he dealt with were Energy-Momentum Conservation, Hidden Variables Theories, EPR Paradox, Bell's Inequalities, Empty Waves and so forth.

In the Selleri's archive I found a note he wrote for a conference held in Bari (without his attendance because of health problems) in October 2013 (one month before Selleri's death) for the 50th birthday of Department of Physics²⁸³. Concerning the sense of the research he carried out during the forty years (at that time) spent in Bari, Selleri claimed that:

The subjects dealt with were mainly three: wave-particle duality [including hidden variables, Bell's inequality and empty waves], 1970-1990; EPR Paradox, 1980-2001; Relativistic theories, 1994-2012²⁸⁴.

I would like to underline how Selleri paid tribute to whom inspired him towards local realism: Albert Einstein. In a note found in his archive and titled *What I think about Einstein*, Selleri claimed that:

²⁸³ With the presence at Bari of the Italian physicist Michelangelo Merlin, the Institute of Physics was born in 1959 and opened in 1963, being at the same time independent from Mathematics. Further information about the history of Science Faculty in Bari are available in the interesting Campanile B., De Frenza L. (2017), *...E pur si vuole! La facoltà di Scienze a Bari. Origini, istituzione, sviluppi fino agli anni Settanta*, Bari: Edizioni Giuseppe Laterza.

²⁸⁴ "Gli argomenti trattati sono stati principalmente tre: La dualità onda-corpuscolo [comprendente hidden variables, Bell's inequalities and empty waves, 1970-1990], Il paradosso di Einstein-Podolsky-Rosen, 1980-2001, Le teorie relativistiche, 1994-2012" (FSPA).

My first contacts with Einstein's philosophy were when I was interested in the Foundations of Quantum Mechanics. I greatly admired Einstein who had the force to refuse the Copenhagen formulation of quantum theory and especially to defend local realism, the idea that in the objective reality at the atomic level there is no action at a distance. Now, as in 1935, it is extremely difficult to find a physicist declaring a sympathy for local realism, in spite of the fact that it is the most natural standpoint for all types of phenomena, those of the atomic level excepted, eventually (FSPA).

Selleri also recognized a philosophical consonance with Giacomo Leopardi²⁸⁵, the Italian philosopher, poet, essayist, and philologist. In a letter sent to the Leopardi family, in order to thank for the invitation to visit an exhibition, Selleri stated:

Since high school I had a predilection and a peculiar sensibility for Giacomo Leopardi's poems. Later in the years I realized that this resonance was certainly due to the enthralling force of the poet's personality; but at least in part also to a deep philosophical concordance²⁸⁶ (Selleri to Leopardi family, 29 July 1996-FSPA)

2.4-SELLERI'S INTERTWINING WITH THE SOCIAL-POLITICAL EVENTS IN THE 1960s AND 1970s

2.4.1-END OF 1960s AND THE WHOLE 1970s: A WORLD OF UNREST, CLASHES, UPHEAVALS AND SOCIAL-POLITICAL TURMOIL. THE ITALIAN PECULIARITY, ALMOST A CULTURAL HERITAGE

Before introducing Selleri's contributions to Bell's inequality and EPR, following a chronological line, I am going to reconstruct here and in the next two paragraphs the social and political context in which Selleri moved, so often evoked in the previous pages of this work.

²⁸⁵ Giacomo Leopardi (1798-1837): The Count Giacomo Taldegardo Francesco di Sales Saverio Pietro Leopardi. It sounds so hard trying to describe the greatness of this Italian excellence in the fields of poetry, philosophy, philology and so on. One of the main character of world literature. See the website of Casa Leopardi: <https://www.giacomoleopardi.it/>. See also <http://www.treccani.it/enciclopedia/giacomo-leopardi/> and <https://www.poetryfoundation.org/poets/giacomo-leopardi>. All sites accessed 24 September 2020.

²⁸⁶ "Fin dai tempi lontani del liceo ho avuto una predilezione ed una sensibilità particolare per le poesie di Giacomo Leopardi. Più avanti negli anni ho capito che questa risonanza era dovuta sicuramente alla forza trascendente della personalità del poeta; ma almeno in parte anche ad una profonda concordanza filosofica" (Selleri to Leopardi family, 29 July 1996.FSPA)

And that was also reflected in the deep and widely debated meetings of the SIF Executive Board during the period in which Selleri was a member. I will therefore refer, in an obviously far from exhaustive manner, to the situation in those years, specifically in Italy; I will investigate, through interviews and unpublished work, Selleri's link with the social and political context, so pivotal in his approach to scientific research; and I will also develop his role within SIF during his time as a member of the Executive Board. A role that could not disregard his ethical and political beliefs. Obviously the socio-political context will always be present in the thesis. We will see how Selleri produced several works, even unpublished as well as articles in newspapers and cultural and political magazines for several years specifically related to social and political issues.

There are scholars who have addressed the subject of the reconstruction of Selleri's activity in the international and Italian context of those years in a broad and extensive way, certainly more detailed than me, such as (Freire 2014) and (Baracca, Bergia, del Santo 2017). But also here I would like to emphasize how my research can contribute to the debate, mainly through unseen archival documents, letters and interviews.

Firstly, I decided to take into account the social situation in Italy at the end of WWII and until the 1950s and the early 1960s, the period related to the end of Selleri's studies and his search for work. This choice aimed at revealing the context in which Selleri started to find work. Then, the deep and compelling interplay between Selleri's activity and the social and political choices made during the 1970s as far as the 1980s is also explained.

It goes without saying that, as in the previous parts of the thesis, the sources I have chosen are the result of a personal selection and certainly do not want to expect to be exhaustive. Especially in this case, given the delicacy and the extent of the topic in hand.

During the abovementioned period, Italy experienced a period of accelerated economic growth called "miracolo economico" or "boom". The country, which the WWII had left defeated, destroyed and impoverished, suddenly

turned into one of the main industrial powers in the West. Together with this growth and development, a lot of severe differences that will underpin the political upsets of the following years deeply intruded in the social fabric of the nation. In the following, it is reported an impressive picture of the social and political situation underlying the unique development of Italy during the foregoing period.

I found this far-reaching report in the site of Treccani encyclopedia²⁸⁷. It starts with an analysis of the occurrence as seen in several studies:

[...] In Great Britain, the *International history of the twentieth century and beyond* (2004)²⁸⁸ [edited by four] lecturers of the London School of Economics and King's College of London, speaks of an "extraordinary success", which had profound repercussions not only on the Italian economy and society, but also on the politics of the old continent, since it contributed to making Italy one of the nations that played a key role to the process of creation of the European Economic Community and integration among States.

In France, the *Histoire économique et sociale du XX^e siècle* (2002), a collection of essays by various authors, edited by Jean-François Muracciole, of the University of Montpellier, referring to the Italian situation after World War II, underlines the pragmatism that united the protagonists of the reconstruction process, and dwells in particular on the figures of Luigi Einaudi, promoter in 1947 of an important monetary reform, and Enrico Mattei [a skilled entrepreneur, but also a passionate patron], who between 1945 and 1962 carried out innovative industrial and political strategies [...].

In the United States, Iván Tibor Berend, director of the Center for European and Eurasian studies in the Faculty of History at UCLA, [...] in his book *An economic history of twentieth-century Europe: economic regimes from laissez-faire to globalization* (2006) identifies the leading sectors of Italian development in the industries that manufactured automobiles, household appliances, furniture and office machinery, all products that quickly became a worldwide reference point both for innovative technological solutions and for the quality of design and design work (Treccani 2013).

A critical role was also played by the choices of Italian politicians:

²⁸⁷ (Treccani 2013) in the following: http://www.treccani.it/enciclopedia/il-miracolo-economico-italiano_%28Il-Contributo-italiano-alla-storia-del-Pensiero:-Tecnica%29/. Accessed 24 September 2020. In order not to further overloading the thesis with other notes, all the Italian original text belonging to Treccani are introduced in Addendum 3, pp. 970-972.

²⁸⁸ Anthony Best, Jussi M. Hanhimäki, Joseph A. Maiolo and Kirsten E. Schulze Eds.

[...] The victory of the DC under the leadership of Alcide De Gasperi (1881-1954) in the elections of 18 April 1948 had the effect of definitively placing Italy [defeated in WWII] in the so-called Western bloc and its inclusion in the European Recovery Program, better known as the Marshall Plan, launched by the United States government to support the European allies and attempt to reorganize the entire world economic system as opposed to the 'communist bloc'.

[...] Thanks to the Marshall Plan, active from 1948 to 1952, Italy received aid of about 1400 million dollars, which it used to complete [post-war] reconstruction and to relaunch transport, agriculture, public works and industry sectors (Treccani 2013).

But, simultaneously with this evident development, emerged some severe problems, still not completely resolved today, having a long-lasting impact on Italian history, like the cost of labour, strictly connected to the strong repression against strikes, the attempts to undermine the unions, and so forth:

[...] Apart from the timid legislative interventions of the De Gasperi government, growth took place without strong and clear political planning, and rather followed the logic of the market, bowing to the initiative of the large industrial groups.

A first significant factor of imbalance concerned the cost of labour: companies managed to keep the prices of goods competitive for a long time by exploiting labour and hindering trade union activities.

Even more impressive is the number of workers who were involved in the harsh repression with which the police forces, led by Interior Minister Mario Scelba, fought the strikes. Between 1948 and 1954, 75 workers were killed for taking part in trade union demonstrations, 5104 were injured and about 150,000 arrested (61,200 of the latter were convicted).

It was at the height of the Cold War, and pressure from the United States on the Christian Democratic government to limit the trade unions' initiative was strong and insistent. In 1954 the U.S. ambassador to Italy, Clare Boothe Luce, meeting the president of FIAT²⁸⁹ Vittorio Valletta, told him that his government would have severed relations with the Turin company if the latter had not succeeded in reducing the weight that the CGIL²⁹⁰ had among the workers. Fear of disrupting relations with the United States led FIAT's management to start waves of layoffs and internal transfers of the most combative trade unionists, who were sent to the departments where the work was harder.

²⁸⁹ FIAT is the acronym for Federazione Italiana Automobili Torino, surely the biggest Italian automotive factory.

²⁹⁰ CGIL is the acronym for Confederazione Generale Italiana del Lavoro, one of the three great confederate trade unions present in Italy, together with CISL and UIL.

Indeed, the company even allowed the police to use the lists of employees to start filing those suspected of being communist sympathizers: something very similar to the McCarthyism then widespread in US institutions and society.

[...] After five, six years of relative ‘social peace’, new protests broke out in 1962, when the FIOM²⁹¹ (a specific union of the CGIL), during the negotiations for the metalworkers’ contract, asked for a reduction in working hours from 44 to 40 hours per week, spread over five days instead of six.

In July, Fiat, Lancia and Michelin workers crossed their arms in Turin, claiming wage increases and recognition of the third week of paid leave. Their mobilization lasted a month, during which the workers received the support of the inhabitants of the Borgo San Paolo district. The Michelin workers occupied the Porta Nuova railway station, while for the first time in several years the FIAT Mirafiori workers also abstained from work: it was the dawn of a new and tumultuous era in the history of industrial relations in Italy.

For four days, the police and workers, armed with sticks and bolts, faced each other in Piazza Statuto, while the police vehicles loaded the crowd. The leaders of the left-wing parties and some journalists denounced the presence in the crowd of ‘provocative agents’, but the trials against the arrested protesters showed that they were workers, mostly young and originally from the South: a sign that the hope for harmonious development from an economic and social point of view had not come true.

The rupture between the main trade unions prevented the workers’ movement from having sufficient weight to negotiate contracts with companies for a long time, and living conditions inside the factories improved only after 1970, when the Workers’ Statute was approved [in the wake of the protests].

Another factor of imbalance in Italian society at that time can be identified in the migratory waves, which in less than twenty years profoundly changed the structure of the population, generating an unprecedented reshuffling.

The phenomenon took on even more radical aspects in the South, where young people left en masse, with Milan and Turin as their main destinations. [...] Here they found a widespread attitude of hostility and contempt, if not real racism, which manifested itself first of all in the real estate advertisements, in which it was specified: “here we do not rent to southerners”.

[...] The years of the “miracolo economico” represented the key period of an extraordinary process of transformation that affected every aspect of everyday life: culture, family, entertainment, consumption, even language and sexual habits.

Young Italians, influenced by their English and American peers, accentuated the transgressive behaviour in clothing, musical tastes and lifestyle choices.

²⁹¹ FIOM is the acronym for Federazione Impiegati Operai Metallurgici.

At the same time their civil and political involvement grew, which in the long run, during the crisis phase that followed the “miracolo economico”, became the outlet for a long-hatched feeling of disappointment.

Between the spread of the first forms of youth protest and the growth of a renewed civic sense, in the final phase of the economic miracle there were several signs of a new change in the collective ethos, which would come to light in 1968. The clash between generations over clothes and music, lifestyles and skirt length for girls concealed a much more radical clash over ways of conceiving life and politics (Treccani 2013).

The birth of the themes underlying the political unrest, which upset the country over the mid 1960s, the whole 1970s and the very early 1980s, leaving unhealed wounds between people, are clearly well stated between the lines above reported.

Concerning the years between the mid of 1960s and the early 1980s all over the world, it deserves to be underlined the crucial turmoil of the student members at Berkeley University in September 1964 with the *Free Speech Movement*, which asked for the lift of the campus ban on political activity and the approval of several students’ right as, for instance, the freedom of expression and the academic freedom. These events were dramatized in the movie *The Strawberry Statement* by Stuart Hagmann.

In 1968 several student movements in Europe gave birth, even with different ways, to severe unrest. For instance, I recall here the upheavals in Berlin, in April 1968, following the attack against Rudy Dutschke, leader of the SDS, the German Socialist Student League, an event of which there is also a 5-minute film document; the famous French May, on which there is a huge bibliography; and the clashes in Rome at Valle Giulia, the venue of the Faculty of Architecture. In the latter case emerged immediately the Italian peculiarity linked to opposing ideological visions between extreme right and extreme left and their common opposition to police.

In addition, in Italy (as well as in France) the student movements were linked to the worker ones, creating a wide unrest on converging topics and the basis to continue a long and varied turmoil. All these movements created also a counterculture full of creativeness and cultural products.

According to (Freire 2014), who linked his view to an international situation:

The 1960s and the early 1970s are landmarks in twentieth century history concerning cultural turning points and political unrest. The emergence of counter-cultures, rock music, drugs, sexual liberation, environmental concerns, feminist movements, and protests against the Vietnam War and the political establishment featured in the news at that time. The expressions “soixante-huitards” and “’68 generation” are used nowadays as part of the common lexicon when identifying particular social sensitiveness. The implications of the 1968 events were far reaching (Freire 2014, pagg. 197-198).

Going now to the specific situation in Italy, it is almost impossible to illustrate the complex and multifaceted situation that Italy experienced in the years between 1960 and 1984. And that makes my country possess a unique peculiarity in the world, almost a cultural heritage, which has shaped the relationship between citizens and institutions and is far from being overcome, given also the enormous current polarizations. Nor is this thesis the appropriate place for such an analysis. However, within the landscape of which Selleri was a part, it seemed prominent to me to at least try to introduce the context.

One of the peculiarity of Italian case lied in the workers struggle in the so-called “Autunno Caldo” (Hot Autumn) in 1969 as well as the response from the institutions. The connection between students and workers in Italy was two-ways, with a continuous mutual strengthening, despite the cultural differences.

The peculiar strong opposition between the extreme left and the extreme right led many young people to clash in the streets in a *crescendo* of violence. Among the many and varied texts on the years in Italy between 1969 and 1984, I would like to quote Cucchiarelli P., Giannuli A (1997), *Lo Stato parallelo. L'Italia “oscura” nei documenti e nelle relazioni della Commissione Stragi*, Gamberetti Editrice²⁹².

²⁹² (Cucchiarelli, Giannuli 1997) in the following.

In Italy various expressions have been coined to give uniformity and a possible interpretation to the fundamental and often mournful events, seasoned by “misrepresentations, lack of culprits, fugitives, disappearance of evidence, procedural failures, killings, silences and a thousand of ‘incomprehensible’ episodes”²⁹³, that have occurred since the massacre of Portella della Ginestra on 1 May 1947 during the Labour Day²⁹⁴. In 1969 started a long and sad sequence of massacres. On 25 April, the Liberation Day, a bomb went off at FIAT Fair Pavillon in Milan, with several serious injures, and simultaneously another bomb was found at the Central Train Station. Some months later, on 9 August, eight bombs exploded on diverse trains, with 12 injured.

The day of 12 December 1969 remained carved in the history of Italy. The infamous massacre in Piazza Fontana to the Banca Nazionale dell'Agricoltura in Milan, with 16 dead and 84 injured; and the simultaneous explosions in Piazza della Scala, also in Milan; and in Banca Nazionale del Lavoro, Altare della Patria and Museo del Risorgimento in Rome, leave the country stunned. But from there, at least 15 years of massacres that have gone unpunished bloodied Italy²⁹⁵. Even if a complete truth has almost never been reached, for many of these massacres it has been at least possible to ascertain the black matrix, that is, the Italian extreme right, after an endless series of deceptions that wanted to trace the responsibility back to the anarchists or to the extreme

²⁹³ “depistaggi, mancanza di colpevoli, fughe dei responsabili sparizione di prove, fallimenti processuali, uccisioni, silenzi e mille e mille ‘incomprensibili’ episodi” (Cucchiarelli, Giannuli 1997, pag. 43).

²⁹⁴ La strage di Portella della Ginestra fu un eccidio da parte della banda criminale di Salvatore Giuliano che sparò contro la folla di contadini riuniti per celebrare la festa del lavoro, provocando undici morti e ventisette feriti. I motivi per cui venne compiuto e, nei giorni successivi, vennero assaltate sedi dei partiti di sinistra e delle camere del lavoro della zona risiedono, oltre che nella dichiarata avversione del bandito nei confronti dei comunisti, anche nella volontà dei poteri mafiosi, dell'autonomismo siciliano e delle forze reazionarie di mantenere i vecchi equilibri nel nuovo quadro politico e istituzionale nato dopo la seconda guerra mondiale e, nonostante non siano mai stati individuati i mandanti, sono certe le responsabilità degli ambienti politici siciliani e americani interessati a intimidire la popolazione contadina che reclamava la terra e aveva votato per il Blocco del Popolo nelle elezioni del 1947: https://it.wikipedia.org/wiki/Strage_di_Portella_della_Ginestra; and https://www.wikimafia.it/wiki/index.php?title=Strage_di_Portella_della_Ginestra. Both accessed 24 September 2020.

²⁹⁵ July 22nd, 1970: Gioia Tauro massacre (6 dead and 66 wounded). May, 31st, 1972: Peteano killing (3 dead and 2 wounded). May 17th, 1973: Milan, police headquarters massacre (4 dead and 52 wounded). May 28th, 1974: Brescia, della Loggia Square massacre (8 dead and 102 wounded). August 4th, 1974: Italicus train killing (12 dead and 105 feriti). August 2nd, 1980: Bologna train station slaughter (85 dead and 200 wounded). December 23rd, 1984: 904 express train massacre (16 dead e 267 wounded).

left. A period which, unlike the previous ones that had seen traumatic episodes of chronicle, reaches a systematic mixture of different subversive components. Thus we speak of a “Strategia della Tensione²⁹⁶”, which Cucchiarelli and Giannuli date back to the period 1960-1974, with massacres attributable to a interweaving inherent in conceiving a coup d’état. While the massacres of the following years are seen in this perspective as the backlash of extremist fringes (Cucchiarelli, Giannuli 1997, pagg. 369-370). If we consider this additional specific period of 15 years (1960-1974) it must be said that there were at least 140 bombings, an unprecedented situation throughout the Western world at that time. Rather than of diverted sectors of the State, one should speak of a “Parallel State” with its institutions that have shown schizophrenia and bipolarism capable of stiffening the freedoms proper to a democracy in order to “return” to being an authoritarian State, and in any case to catalyze the most varied subversive forces in a perspective aimed at promoting by all means fidelity to the Atlantic Pact and to prevent a PCI in government, even beyond the constitutional dictates. According to (Cucchiarelli, Giannuli 1997):

[...] the strategy of tension has represented something more than the mere succession of massacres and related attempts at coup d’état. It has been, together, the most evident manifestation of the conditioning imposed on the sovereignty of our State and the reagent which has fused in a single deadly mixture the main expressions of the deviance of power [...] ²⁹⁷ (Cucchiarelli, Giannuli 1997, pag. 13).

A very deep and articulated analysis is specifically contained on pages 369-395 in (Cucchiarelli, Giannuli 1997), to which I refer.

In the Italian peculiarity of those years we also talk about the so-called “anni di piombo²⁹⁸”, an expression linked to the period 1969-1981²⁹⁹ and that

²⁹⁶ Strategy of Tension.

²⁹⁷ “[...] la strategia della tensione ha rappresentato qualcosa di più del semplice succedersi delle stragi e dei connessi tentativi di colpo di stato. Essa è stata, insieme, la più evidente manifestazione dei condizionamenti imposti alla sovranità del nostro Stato e il reagent che ha fuso in un’unica micidiale miscela le principali espressioni della devianza del potere [...]” (Cucchiarelli, Giannuli 1997, pag. 13).

²⁹⁸ Years of Lead.

²⁹⁹ Actually, the effective range changes depending on the different historians who have studied it.

derives from the eponymous 1981 film (*Die bleierne Zeit*, which won the Golden Lion to the 38th Mostra internazionale d'arte cinematografica di Venezia) shot by Margarethe von Trotta on the political situation in West Germany in those years. There was an extreme political dialectic that led to violence and clashes in the streets, as well as armed struggle and episodes of terrorism by the most extremist fringes³⁰⁰.

The year 1977 was probably the one of the violent turnaround. Primo Moroni and Nanni Balestrini summarized in this way:

In '77, the daily generalisation of a political and cultural conflict that branched out in all the places of the social sphere broke out, exemplifying the clash that ran through all the seventies, a harsh clash, perhaps the toughest, between the classes and within the class, that has ever occurred since the unification of Italy. Forty thousand denounced, fifteen thousand arrested, four thousand sentenced to thousands of years in prison, and then dead and wounded, hundreds on both sides³⁰¹.

The country truly experienced a situation of extreme danger in the streets, with continuous firefighting and attacks, with a peculiar exacerbation in the period 1977-1981. The response of the national Institutions was in the creation of special laws and pools of anti-terrorism magistrates (who paid also with their lives) who also became a model in other European countries which then clashed with their national realities. But they also brought a hard blow to the rights of the population, which, however, generally accepted this situation of a limited freedom in order to no longer continue to live with the aforementioned risks.

As a result of this peculiar situation, any field of Italian social and cultural life has certainly not remained unaffected by the external environment. On the contrary, it has been permeated by this atmosphere of showdown.

³⁰⁰ A good synthesis I took some cue from is disposable at: https://it.wikipedia.org/wiki/Anni_di_piombo. Accessed 24 September 2020.

³⁰¹ “Nel '77, divampò la generalizzazione quotidiana di un conflitto politico e culturale che si ramificò in tutti i luoghi del sociale, esemplificando lo scontro che percorse tutti gli anni settanta, uno scontro duro, forse il più duro, tra le classi e dentro la classe, che si sia mai verificato dall'unità d'Italia. Quarantamila denunciati, quindicimila arrestati, quattromila condannati a migliaia di anni di galera, e poi morti e feriti, a centinaia, da entrambe le parti” (Moroni P., Balestrini N. (1988), *L'orda d'oro*, Milano. SugarCo).

However, this has also led to new cultural perspectives and epistemological and scientific breaks. And physics, with its places and institutions, has certainly not been immune to all this, developing a reaction to the mainstream physics, that is, PP and QM.

Another character of the Italian heritage lied surely in the presence of many scholars politically deployed, also at institutions level.

As stressed by (Baracca, Bergia, Del Santo 2017):

In Italy the dissatisfaction of the young generations was rooted in the student protests of 1968 and the subsequent labour and social fights, which challenged the role of scientists.

The young generations of physicists searched for new scientific approaches and challenged their own scientific knowledge and role. The criticism to the foundations of quantum mechanics and the perspectives of submitting them to experimental tests were perceived as an innovative research field and this attitude was directly linked to the search for an innovative and radical approach in the history of science. All these initiatives gave rise to booming activity throughout the 1970s, contributing to influence the scientific attitude and the teaching approach.

[The young generations of physicists showed] active involvement in the social-political events of that crucial period, as well as the aspect of their cultural and ideological engagement, which objectively renovated the public attitude towards science and its historical and social interpretation (Baracca, Bergia, Del Santo 2017, pag. 67).

On the other hand, (Freire 2014) clearly expressed the connection between the social and political events and the world of research, starting by the title of one of his chapter: “ ‘From the Streets into Academia’: Political Activism and the Reconfiguration of Physics Around 1970”. And still continuing claiming that:

The political and cultural unrest of the late 1960s influenced the debate on quantum physics by helping those who wanted to push research on foundations from the margins to mainstream physics (Freire 2014, pag. 197).

The Italian case is then an evidence of how physicists who were politically active were able to open the borders of the physics discipline to include and value themes which were not usual in the discipline's agenda, namely foundations of quantum mechanics and history of physics.

The activists were joined by physicists who, independent of their political stances, were interested in promoting the research on such themes or accepted them as a strategy to accommodate social and professional tensions. In the case of foundations of quantum mechanics, this confluence allowed the creation of a professional space to boost its research (Freire 2014, pagg. 221-222).

The Italian case, in particular, reminds us of the case built by the historian Paul Forman³⁰² when he argued on the influence of social factors in the direction of the research adopted in the building of the quantum theory in the early 1920s. As science is a locally situated cultural practice, it is influenced by all the other dimensions from culture to politics and economics (Freire 2014, pagg. 229-230).

During the foregoing years, all the universities were practically occupied. Some important testimonies can be found in the already mentioned books of (Bonolis, Melchionni 2003) e (Bonolis 2008).

For instance, in the former, Francesco Calogero³⁰³ argued about those years and his personal recollections, giving us a clear portrayal of the situation in the universities:

I lived it quite personally. First there was the period of the occupations, with the death of Paolo Rossi. One of the reasons my son's name is Paolo is that he was born in those very days. I was partly involved in the occupation of the university. The occupation of the University of Rome, in 1966, was directed against the fascist preponderance in the university, both by students squads and also by a certain academic hierarchy. Afterwards there was another occupation and gradually the student movement degenerated, in my opinion, and eventually there was even a certain contiguity with terrorism. That was a significant period for everyone and also quite hard and difficult, because the Physics Department here in Rome was for several years severely handicapped because of a small group of extremist students who had lurked inside, the so-called "collective of physics", which used methods of struggle even quite violent, so much so that some teachers were even personally attacked³⁰⁴ (Bonolis, Melchionni 2003, pag. 89).

Also Giorgio Parisi³⁰⁵ expressed his memories and feelings:

³⁰² By the way, Selleri quoted Forman as a huge example of the influence of society on scientific activity, inserting several references to him in his works.

³⁰³ See (Bonolis, Melchionni 2003, pagg. 67-68).

³⁰⁴ The Italian original text is in Addendum 3, pp. 972-973.

³⁰⁵ For wide information on Giorgio Parisi, see (Bonolis, Melchionni 2003, pagg. 291-292).

The '68 was an extremely important experience for me, because I came from a family of liberal tradition, but my father preferred to vote for Democrazia Cristiana as a more solid bulwark against the danger of the Communists. So for me 1968 was a period of change of perspective. I had attended [...] a school where many were monarchists or at least right-wingers. In that environment, being liberal, I had finished being a leftist. During 1968 I was against the occupation, as an illegal activity; but, once it was done, I took part in all the activities and slowly this caused me to move strongly to the left. In 1969 I was now completely in agreement, from the ideological point of view, with the Marxist position. [...] [a] strong socialization was an important consequence of that period, which gave me a remarkable openness³⁰⁶ (Bonolis, Melchionni 2003, pag. 296).

As already pointed out in Chapter 1 (page 50), Giorgio Salvini highlighted how the departure of electron-positron collider Adone in Frascati had been delayed due to social and trade union unrest that also pervaded the world of research. I would like to draw a peculiar focus to the statement by Ettore Salusti³⁰⁷, who in (Bonolis 2008) underlined the unusual changement in progress concerning FQM at the end of 1960s and the early 1970s, at the beginning of the path towards the future mainstream character of QM:

During the tensions of '68, one day I realized that in Physics [Institute] there were rooms full of people doing quantum mechanics, many, even too many, while the country probably suffered from serious cultural deficits. So I tried to ask information [...] to the people who were hanging out in the real moment and not in the quantum world³⁰⁸ (Bonolis 2008, pag. 480).

Another source for describing the Italian situation, with the impressive renewal took by young generation of scholars is (Baracca 2017)³⁰⁹:

The student protest of '68 and the Hot Autumn opened a decade of radical claims and struggles, in the context of which the generation of physicists and scientists, strongly committed on the political and social level, produced a more balanced scientific development, with the progress of many important sectors that in the postwar period were

³⁰⁶ The Italian original text is in Addendum 3, p. 973.

³⁰⁷ For further information, see (Bonolis 2008, pagg. 469-470).

³⁰⁸ “Durante le tensioni del '68, un giorno mi accorsi che a Fisica c'erano delle stanze piene di persone che facevano meccanica quantistica, tanti, persino troppi, mentre probabilmente il paese soffriva di gravi deficit culturali. Allora provai a chiedere informazioni [...] alle persone che bazzicavano nel mondo reale e non nel mondo quantistico” (Bonolis 2008, pag. 480).

³⁰⁹ Baracca A. (2017), *Storia della fisica italiana. Un'introduzione*, Milano: Jaca Book.

penalized by the predominance of high energy physics.

[...] There was also a process of democratization in universities, with the overcoming of traditional institutes with departments and the creation of democratic management bodies, such as Faculty Councils and Degree Councils.

[...] Young graduates entered massively into universities and research centers, but with very precarious working relationships [...]. In any case, they brought an air of renewal in structures already archaic³¹⁰ (Baracca 2017, pag. 178).

A very interesting source which, quite in real time, described from behind those strange years, showing the deep intertwining between the political radicalization and the critical reflection on science is (Baracca, Bergia 1975)³¹¹:

The great explosion of protest in the '68 and the years immediately following had given rise to a process of critical reflection on science that finds no precedent in the memory of the researchers of our generation. Many of them had undergone at that time [...] a process of political radicalization; others, probably the most, although without resolutely siding with the positions of dissent towards the scientific establishment [...] and the very aims of research, were projected into a state of widespread dissatisfaction with the contents and methods of the research in progress. The disruptive value of that movement is not diminished by the fact that that state of mind often found no way to overcome the level of pure rejection, due to the lack of specific analysis and indications of a way out. Even those who had matured a clear political choice found themselves in the grip of a crippling alternative: that is, between the pure and simple abandonment of a research activity in which they could no longer believe or, given that nothing alternative could be identified and that the abandonment of their scientific terrain of activity was judged politically wrong, the total split between political activity and work commitment.

Certainly, there was no lack, nor had there been in previous years, of analysis on science capable of going beyond the narrow limits of a sectorial vision and providing a solid ground for general reflection; we refer, specifically, to those analyses, Marxian moulded, which stressed the decisive importance of the mode of production in conditioning the choices of research and in shaping certain characteristics.

³¹⁰ “La contestazione studentesca del '68 e l'Autunno Caldo aprirono un decennio di rivendicazioni e lotte radicali, nel cui contesto la generazione di fisici e di scienziati, fortemente impegnata sul piano politico e sociale ha prodotto uno sviluppo scientifico più equilibrato, con il progresso di molti settori importanti che nel dopoguerra erano stati penalizzati dal predominio della fisica delle alte energie (Baracca 2017, pag. 17).

[...] Vi fu anche un processo di democratizzazione nelle università, con il superamento dei tradizionali istituti con i dipartimenti e con la creazione di organi di gestione democratica, come i Consigli di Facoltà e i Consigli di Corso di Laurea. [...] I giovani laureati fecero ingresso massicciamente nelle università e nei centri di ricerca, ma con rapporti di lavoro molto precari [...]. In ogni caso portarono un'aria di rinnovamento in strutture ormai arcaiche” (Baracca 2017, pag. 178).

³¹¹ Baracca A., Bergia S. (1975), *La spirale delle alte energie*, Milano: Bompiani.

However, in our opinion, they had the defect of being too little specific, that is, of not having been brought to the heart of a defined field of research, so that they were often reduced to petitions of principle, not verified on the current reality³¹² (Baracca, Bergia 1975, pagg. 5-6).

Among the occupied universities, now I want to turn my attention to Bari. According to (Campanile, De Frenza 2017)³¹³:

[...] the need for renewal of the university institution manifested itself in the squares and faculties. The Faculty of Science [of Bari] listened to the protesting students' requests and firmly rejected the Government's attempt to limit the universities' autonomy.

But the most significant clash took place at the scientific level because the inadequacy of the model that contrasted humanistic and scientific culture was emerging more and more strongly, while it was clear that the confrontation was between modernizing drives and conservative resistance.

[...] By 1969 the student population had taken on its own identity and in the Faculty of Science its strength was such that it was able to negotiate with the teachers the amendment of the Statute for a degree in Physics³¹⁴ (Campanile, De Frenza 2017, pag. 85. My underline).

Some recollections come from several interview I made with some scholars. For instance, Giancarlo Scalera³¹⁵, concerning the mood of the 1970s, claimed that:

I arrived in the years '70-'75 in Bari. Difficult years, years of lead. You could hear all sorts of things, there were assemblies all the time.

³¹² The Italian original text is in Addendum 3, p. 973-974.

³¹³ Campanile B., De Frenza L. (2017), *...E pur si vuole! La facoltà di Scienze a Bari. Origini, istituzione, sviluppi fino agli anni Settanta*, Bari: Edizioni Giuseppe Laterza.

³¹⁴ "Nel 1969, la necessità di rinnovamento dell'istituzione universitaria si manifestava nelle piazze e nelle Facoltà. La Facoltà di Scienze [di Bari] prestò ascolto alle richieste degli studenti in protesta e respinse con fermezza il tentativo del Governo di limitare l'autonomia degli Atenei. Ma lo scontro più significativo avveniva a livello scientifico perché emergeva sempre più forte l'inadeguatezza del modello che contrapponeva cultura umanistica e scientifica, mentre era chiaro che il confronto era tra spinte modernizzanti e resistenze conservatrici. Nel 1969 ormai la popolazione studentesca aveva assunto una propria identità e nella Facoltà di Scienze la sua forza era tale da poter negoziare insieme ai docenti la modifica dello Statuto per la laurea in Fisica" (Campanile, De Frenza 2017, pag. 85. My underline).

³¹⁵ Giancarlo Scalera (1949-), after his degree in physics at Bari University, discussing a Thesis on Foundations of Physics, and some papers written with Selleri, in which he proposed a local model that is able to violate the Bell's inequality, Scalera worked for 35 years in Rome at the Section of Seismology and Tectonophysics, National Institute of Geophysics and Volcanology (INGV).

There were dangerous and very strange people around. I was scared, they said that in the eskimo they had a gun (Interview with Giancarlo Scalera, Rome, 7 December 2017).

Nicola Cufaro Petroni³¹⁶, always on the situation at Bari university, confirmed the special relationship between the diverse members and gave us the precise period, February 1969, in which Physics Institute was occupied:

I joined the university in November 1967 and then there was the whole mess in 1968 when political activity had been going on in Physics Institute. I don't remember it in 1967. What was still present in 1967 was the *goliardia*, with the predominantly right-wing goliardic groups that lived more at the central venue of Bari university. Here on campus they were less and the fact that we were here kept us sheltered.

Then everything was overwhelmed, from '68 and '69 by the occupation of Physics Institute in February 1969. At that time there was Merlin, who was supposed to close us down and open the Institute in the evening when he left. But I have to say that there was never a real confrontation with teachers (Interview with Nicola Cufaro Petroni, Bari, 31 May 2019).

I took into account right now the deep relationship Selleri had with the social and political framework during the years of his research on FQM. I mentioned here below several evidences belonging to Selleri archive and AGAB (correspondence, unpublished papers and so forth), as well as some interviews I made, so as to show how he was very affected by the challenging time he lived.

Some quotations, which have well shown the centrality of social and political ideas in Selleri's activity, have already been included in Chapter one. Below are some others letters and interviews, as well as unpublished works by Selleri himself that I found during my researches.

Concerning Selleri's political commitment, and his contribution, predominantly scientific, inside the Communist Party, Angela Pizzi plainly claimed:

³¹⁶ Nicola Cufaro Petroni (1949-) Associate Professor in Probability and Statistics Mathematics at the University of Bari, graduated in violin at the Conservatory. Among his interests there are Stochastic Mechanics; Entanglement of Quantum States; Foundational Aspects of Quantum Mechanics. He is the author of scientific works on peace and disarmament, being also USPID secretary. For further information, see <https://www.ba.infn.it/~cufaro/homepage.html>. Accessed 24 September 2020.

[...] When I met him, in 1978 [for the second time], he had already move away. He did not show a big deal anymore. At that stage he privileged first his work and scientific research. I do not remember neither very critical positions towards the PCI [Italian Communist Party] nor any attendance in some party section. They called him for some contribution, more scientific than political, if there was anything to talk about that had to do with science (Interview with Angela Pizzi, Bari, 11 July 2017).

Scalera stated that Selleri was very well-versed in the details of the political dialectics :

[The Institute of] Physics was very active politically but I was not well-versed, I did not distinguish or follow the various positions well. Franco distinguished all the subtleties and understood well how things were. He taught me to distrust those who said: “We are saying the same things” (Interview with Giancarlo Scalera, Rome, 7 December 2017).

Another scholars at Bari University in the field of low-energy particle, Vincenzo Paticchio³¹⁷ stressed some concepts of Selleri’s ideas as already reported in his *La piramide azteca della fisica teorica delle particelle elementari*, as well as remarkable sociological information on Physics Institute in Bari and on physics in general:

When I enrolled in physics, in 1974, it was the reddest degree course in Bari, the leftmost one. It was a constructive bastion, politically speaking. Much of our time was spent in assemblies of both Institute and Faculty. There were various lines and positions. Franco was rigidly tied to the PCI together with Marco Maestro and others. At that time I was more towards the PDUP³¹⁸. Then I joined the PCI through Pino Guarino who was already inside and enrolled me in Bari center, the leading section to which railroad, banks, electricity workers, and many people of the union belonged.

³¹⁷ Vincenzo Paticchio (1956-) is an experimental INFN physicist of the University of Bari who works in the low energy and with hypernuclei. With Selleri he edited *Physics*, a dictionary containing prolusions of Selleri, Narlikar, Tyapkin, Owen, on topics of FQM, Astrophysics, Geophysics and SRT, respectively. Paticchio also collaborated in the FINUDA project, which I refer to in paragraph 2.9.

³¹⁸ PDUP (Partito di Unità Proletaria-Proletarian Unity Party): “The PdUP was founded in the November 1972 by minority factions of two parties: the New PSIUP, led by Vittorio Foa and Silvano Miniati, that gathered the militants of the right wing of the Italian Socialist Party of Proletarian Unity who had not agreed on the decision to join the Italian Communist Party, and Socialist Alternative, led by Giovanni Russo Spina and philosopher Domenico Jervolino, that gathered the militants of the left wing of the Workers' Political Movement who had opposed the merge into the Italian Socialist Party (PSI). Its symbol was the hammer and sickle over the world, inherited by the PSIUP”. See [https://en.wikipedia.org/wiki/Proletarian_Unity_Party_\(Italy\)](https://en.wikipedia.org/wiki/Proletarian_Unity_Party_(Italy)). Accessed 24 September 2020.

The story is what it is, at a certain point it all ended, even regardless of our choices. And to this day it is not clear to me in which direction we have to go now.

Franco was of the idea that physics was not objective, that there were political or ideological flows, in one direction or another, there were trends of a certain kind, imposed fashions. That is, there were centers of power. And all of us who were in his sphere of influence, today we would say in his magic circle, were educated in the same way. In fact, in confirmation of his ideas, this department has become a company, a manufacturing industry in which physics is made by engineers, taking an orthogonal direction to Franco's worldview. In the INFN there was this mutation: many fewer researchers, too many technicians, technologists and engineers who are used to support these big experiments in which the physicist finds himself compressed, is actually an accountant. If you do some data analysis, fine. If you do a little simulation, good. Otherwise, little stuff. In fact, there is a lower participation, a lower choice of students regarding nuclear physics issues. They go into applied physics or physics of matter, where there's still a more direct relationship to reality. Franco therefore had this vision, and I agree with him still today, that physics is guided by political rather than objective choices (Interview with Vincenzo Patricchio, Bari, 29 May 2019. My underline).

According to Patricchio, Selleri had a great political and scientific influence in several students of the institute.

The remarkable interview with Michele Barone has allowed me to shed light on several unreleased Selleri's personal experiences as well as on many debates at stake. In the early years, Franco had an intense political activity, being close to the Direction of the PCI and always looking after the scientific events:

Where I met Franco more closely was in the years 1968-69, with the occupation of the Institute. He sided with the students and also created the university section of the PCI at the Institute of Physics, born in Franco's study, based on the ashes of the student movement and in antithesis to that which existed in Letters Faculty, which was hegemonized by Beppe Vacca. Then the section at the Institute of Physics was expanded to include people who were at the Institute of Chemistry and Genetics. At the time, however, it was more dangerous to join the PCI than to be part of the Student Movement. Meanwhile, Franco from Palese came to Bitonto on Sundays where the PCI section was and together we went to sell the newspaper *L'Unità* on the street. He encouraged me to join the FGCI, the youth section of the PCI, whose secretary at that time was Massimo D'Alema. We managed to mix the students with the workers. We did a lot of work in the Party, organizing scientific conferences.

It was all a fervor of initiatives because at that time Luigi Berlinguer was in the Party's direction and he had surrounded himself with cultured people and had given a boost to science, which had a certain importance within the Festivals of the *L'Unità*, mainly with the debate on the non-neutrality of science. This activity of Franco annoyed the humanist philosophers and philosophers of science, as Vacca, for instance. Because they thought they had a monopoly on everything and they saw us as intruders, invaders of their sector. Selleri made and organized big conferences in Italy and internationally. With the Party, I think when Luigi Berlinguer was director of the university section, they began to bring the debate on the two cultures to the Festivals of the *L'Unità*. Also in the Party schools Berlinguer had introduced the debate on dialectical materialism and historical materialism. And then there were clashes with Geymonat, with Vacca, Giorello etc. I know that Franco had good relations with Luigi Berlinguer and with the whole secretariat. At the Festivals of the *L'Unità* we would go and serve at the tables, on Sundays we would go and distribute the newspaper, there was the militancy and so on. These discussions are no longer fashionable, in fact I do not know how many people are aware of these things, and it is a shame (Interview with Michele Barone, Bitonto (BA), 1 June 2019. My underline).

It emerges the importance of the debates on the non-neutrality of science as well as the struggles between historical and dialectical materialism, between humanists and scientists.

In another interview, Barone answered me dealing with the different approaches and the quarrels linked to historical and dialectical materialism.

It was a big argument. The philosophers of science, with Beppe Vacca, were Gramscians, because historical materialism is rather of Gramscian derivation, while we had sided in favor of dialectical materialism. There was constant debate, even heated debate. Beppe Vacca was at Frattocchie³¹⁹, near Rome, and therefore he was hegemonic from this point of view. There, they went to Party school. He had put science aside. We attacked them by referring to dialectical materialism, which was used in Russia. They saw us as invaders, we (scientists) went to deal with a field that was not ours, on which they pretended to have the truth, and that was that. They were philosophers of science, in historical materialism. Geymonat had married this line. (Interview with Michele Barone, Athens, 21 June 2019).

³¹⁹ At Frattocchie, near Rome, there was the Istituto di Studi Comunisti, or Scuola delle Frattocchie, the central institution of the training of PCI executives and managers from the post-war period to 1993.

An impressive testimony which shed light on some Selleri's traits comes surely by Augusto Garuccio³²⁰, maybe the closest Selleri's colleague:

The first interaction with Franco dates back to 1969, if I remember correctly. During an occupation, we decided to block the computer center, in which there was a powerful IBM, the 3090, which was then a state-of-the-art machine. The idea was to block this machine that made calculations for research and also for the outside world. And it was an IBM, so it was the representative of an American multinational corporation. It was the time of the Vietnam War, the battle against American imperialism. I remember that in the assembly in which we had to decide whether or not to block the computer intervened Franco Selleri who, giving us his complete solidarity, pointed out that in Bari were being processed several data related to an experiment that was in competition with an American one. And that therefore, blocking the computer would have blocked also the opportunity to arrive earlier in this race against the American supremacy in science. And of course this vision prevailed because it brought together our pride as physicists, as Italians, as anti-Americans, and anti-imperialists. And so the prospect of giving the Americans a slap on the wrist was much stronger than the desire to boycott. And so we did not turn off the computer. I have always blamed Franco, in playful moments, for his attitude as a firefighter of revolutionary struggles. But he would justify the fact that he really believed in it and had not gone there to calm us down but was really worried about the opportunity of not getting to the results first. He was in good faith. He did in fact a militant science, not a science detached from the rest of the social and political context. Inside physicists there was a great debate about big science politics. Selleri had already detached himself from the problems of elementary particle physics. He had already had a first baptism in America [in Nebraska] on anti-imperialist positions and then on his return to Italy in 1971 he had perfectly matured his political choices.

³²⁰ Augusto Garuccio (1950-): Author of about 200 publications, more than 100 of which in international journals with referees. Since his graduation thesis he has dealt with various aspects of physical problems related to the Foundations of Quantum Mechanics and Optics. In the early years he dealt with mainly theoretical issues, then analyzing also the experimental aspects. In the last twenty years he has also been interested in the historical and didactic aspects related to this issue and in problems connected with university scientific museology. Summarizing the various research directions: Einstein's paradox, Podolsky and Rosen and Bell's inequality. Dualism wave-corpuscle and de Broglie's pilot wave theory. Experimental problems on correlations in polarization of photon pairs. Bohm's theory of quantum potential. Quantum optics and complementarity. Production and study of entangled states in parametric dissociation type I and II. Interferometry with phase conjugation mirrors. History of modern and contemporary physics. Scientific museology and cataloguing problems. Didactics of modern and contemporary physics. See also: <https://scholar.google.com/citations?user=2LoI4X0AAAAJ&hl=it;> [https://www.researchgate.net/profile/Augusto_Garuccio;](https://www.researchgate.net/profile/Augusto_Garuccio) http://www.icom-italia.org/wp-content/uploads/2020/02/ICOM.Italia.ElezioniCoordinamentiRegionali.Puglia.CV_Garuccio.2020.pdf. All accessed 24 September 2020.

I believe that he had therefore matured this position against American imperialism but also against American hegemony in science (Interview with Augusto Garuccio, Bari, 27 May 2019. My underline and **bold**).

So, Garuccio confirmed the relevant importance of Selleri's stay to Nebraska for his political maturity as well as his predominant interest to a militant science.

Garuccio and Selleri experienced, in different times and on their own, an evocative journey and stay for research in URSS during the 1970s. Garuccio went there in 1974-1975, following a project proposed by Selleri, whereas Selleri himself was in Dubna in 1977. Garuccio deeply explained, during the interview I made with him, the trend and evolution of this project in his interview. And, above all, he shed light from inside on historical diverse interpretation and developments of the approach on QM in URSS as well as some typical behaviour there. The excerpt is long but I think it deserves to be shared, also as a confirmation of the end of the debate on FQM in URSS:

For a series of complex events, Franco had thought that an activity should be done to see the research done abroad.

I had joined the PCI and it was decided to try to see what were currently the studies on QM in the Soviet Union, and above all to see the compatibility between dialectical materialism and QM. This was an ideological notation. In those years a book on the materialistic interpretation of QM was published by Tagliagambe³²¹. A very significant and very rich book, which gave an almost idyllic description of the debate, in a sense. Which, however, told the story of how through the work of Fock on the one hand, who was a physicist; and Omelianosvskij on the other hand, who was a philosopher of science, it had been possible to reconcile QM with dialectical materialism, whereas before they were seen as strongly contradictory due to the idealistic nature of QM. Bohr had then modified his view on complementarity, after an exchange of views with Fock, probably all piloted in the initial phase, so as to make the two positions compatible. In the meantime, I got married, and went to the Soviet Union with my current wife, having to leave in January.

³²¹ Tagliagambe S. (ed) (1972), *L'Interpretazione Materialistica della Meccanica Quantistica. Fisica e Filosofia in URSS*, Milano: Feltrinelli.

Then the “red tape” was so long that we left around May-June 1974. When I arrived in Moscow, I met two characters, Feinberg³²² and Cernanskji, who were two great solid-state theorists, who became my reference points at the Lebedev Institute. We arrived in the summer, on a Sunday afternoon. On Monday or Tuesday we went to the Lebedev Institute, which was under surveillance. To get in, you had to have a badge, but it was not ready until two days later. What did we do then? It was summer, and with Cernanskji we talked on a bench at the entrance to the Institute. After we talked, Cernanskji said, “Yes, we will talk about all these things, but you know, the QM works. Yes, it is true that there is the debate, but here we all think there is no problem”. He said it to me in a very kind, warm, meaningful tone, but that was the concept. “So, we will give you some interesting things to look at, we will put you in touch with the philosophers. But that is a philosophers’ problem, we are interested in other things. From Omelianovskji and Fock on, this debate is over”.”

I prepared a seminar, which I did at the end of my presence, on Bell’s inequality. Bell’s work did not affect anything. It was not known. They did not know him, they did not understand the extent of it. In the end they let me do what I wanted but the interaction with physicists discussing these problems was not there. Nor was the interest. Then, out of courtesy, they made me do this final talk. They told it was such an interesting work, but said with an attitude of indifference. The project was to have an interaction with physicists who faced the problem of causality and locality based on Einstein’s vision and try to understand if there was a more articulated position beyond that contained in the work of Omelianovskji and Fock. The answer was disappointing. No physicist was interested in these issues, they had basically relegated this debate on philosophical interpretation in the light of dialectical materialism to philosophers. What the philosophers said was fine with them and they had no intention of raising any questions.

Then Garuccio managed to know more details on the peculiar approach on FQM in URSS at that time:

By now the criticism phase had ended in 1956, with a series of tears that had occurred. And then I was told. Landau, for instance, was the first to refuse, on the volume of QM written with Lifschitz, to make a premise that said that QM was compatible with dialectical materialism and Stalin’s guiding thought. There was a lot of tension, but then eventually, the premise was dropped. Landau had won, he was very important as a physicist. Obviously he could afford it because Landau, but this act was experienced by the community of physicists as a liberation.

³²² Doing some research, I think the former could be: Evgenii L’vovich Feinberg (1912-2005): See https://en.wikipedia.org/wiki/Evgenii_Feinberg . Accessed 24 September 2020. I found nothing on the latter.

A liberation from the problem of having to constantly emphasize the compatibility of theory with dialectical materialism, and above all from the thought of Stalin, who was dead long ago. From that moment on, they began to treat QM more and more like normal Western physicists, without asking themselves the problem. This story was told to me by an officer who had been the first secretary of the Soviet embassy in Italy. I complained to him that the young people did not make contact with me, which I would have expected from some young people, I do not know, that I was invited home for coffee. And instead my only contacts were the two leaders of the theoretical group. And then I had access to only a part of Lebedev. And he told me that I should not be surprised because the young people were afraid that contacts with me would be interpreted in a negative way by the political commissioner of the Party. Although I presented myself in an articulate manner, as a PCI member in a very precise position, even more maximalist than they were, so giving the maximum guarantees, but still, the fact that I was a foreigner prevailed. And so the possibility of young people going abroad could be compromised. While instead Feinberg and Cernanskji were at the top of their careers, they had articulate authority, they had interests and family in Moscow, and therefore had the authorization to go abroad, without fear that they would not return. They were given the task of managing me.

[...] From a cultural point of view it was a rich experience, I studied many things. From the human point of view, it was very fruitful. I met many people, especially Cubans, foreigners, who studied there. A geologist and two or three physicists with whom I remained in contact for a long time, even though I have now lost contact. From the scientific point of view, and epistemological, nothing. Because maybe the project had been set up wrong. In the sense that you do not make up a debate if there is no debate. Everything about the situation was in Tagliagambe's book. The debate was closed there. Conversely, we thought there was still room (Interview with Augusto Garuccio, Bari, May 27th, 2019. My underline).

Selleri in 1977 sent to Garuccio a disenchanted³²³ and sometimes poignant letter on Soviet everyday life, recalling the great troubles Garuccio experienced in 1974-75, and also including an emphasis on how to behave, in the light of experience, in the future scientific activities. Still confirming his adhesion to dialectical materialism:

[...] Today is a beautiful day with sun and wind. From my window I see the Volga. The first impression the USSR makes is that almost everything is of poor quality.

³²³ In the sense of "Fully aware, attentive to perceive the intimate essence of things".

Except the plane [IL-62M] which compares to the USA and except for the people who seem to me the best in the world, everything else is a bit shabby, incomplete, old, poor, badly made. From the windows the world looks distorted because glasses are uneven.

[...] I just finished reading and summarizing the nine letters you sent me in 74-75. Reading them I also understood why I felt the need for them: it is the continuity between your visit to the USSR and mine. I retraced your entire adventure, from the problem of language, initially dramatic, then gradually resolved [...], to the serious economic situation that [...] forced you to painful savings; from your friendship with the Cubans with the generous help they gave you, to the even sentimental discovery of the [Communist Party] and its politics; from the translation of Cernanskij's articles to the contacts with Feinberg and the preparation of the seminar on Bell's inequality; from the initial isolation, to the reactions to the letters of the comrades: Michele Barone, Marco Matteuzzi, etc. I will try to give a sense of continuity to my visit, to use your experiences to optimize mine. And I will try to set the stage for us to continue our contacts with the USSR.

[...] I think that you have done even too much, enduring very hard coining and smoothing the ground for me at least in the sense that I know how I must not talk about our work: I must not say that there is a deep connection between Dialectical Materialism and the inspiration of certain physics.

[...] P.S. The Dialectical Materialism seems therefore useless in the relationships with people both in Italy, where you throw heavy reactions on yourself; and here, where the connection with physics is imaginary. So we keep it for ourselves as a deep motivation, as an enormous intellectual strengthening. If the others do not want to know how to understand the world and physics, it is worse for them. They give us, exclusively, a powerful means of investigation³²⁴ (Selleri to Garuccio, Dubna, 18 June 1977-AGPA. My underline).

All in all, Russian experience was useful for Selleri, if only for a series of consciousness that could best mediate his vision.

Always concerning Selleri's intertwining with social and political aspects, I decided to report here some papers and articles on newspapers he made on these issues. Actually, together with his wide scientific production on FQM, he wrote several works on these topics. The first paper I would depict here is "Sull'ideologia nella fisica contemporanea", appeared on the special issue of *Critica Marxista*³²⁵, fully dedicated to Marxism and Sciences in 1972³²⁶.

³²⁴ The Italian original text is in Addendum 3, p. 974.

³²⁵ *Critica Marxista* was a magazine founded in 1963 as a theoretical institution of the Italian Communist Party.

I mentioned first a brief excerpt by the interview with Michele Barone, in order to stress the importance and rarity of this magazine:

[...] It was decided in 1972 to dedicate an issue of *Critica Marxista* to Marxism and the sciences. Franco gave a contribution, which is then what he developed, it is the philosophy that underlies all his articles and his life, then updated a bit with the realism of Popper. There are various contributions in the book: Tagliagambe, Bellone, Petruccioli and Tarsitani, Geymonat himself, Giorello, who was in Geymonat's group, and so forth. And then the documents of Lucio Lombardo Radice, with the concept of Marx's derivative in the Appendix. A unique number that can not be found. The books of the publishing house *Editori Riuniti* are no longer found (Interview with Michele Barone, Athens, 18 June 2019).

Thanks to Barone I had the opportunity to have the very rare copy of the magazine, having access to Selleri contribution. Selleri made it to the long-running debate on the so-called two cultures, finding its origin in the period of development of QM and its philosophical approaches, with the renouncement of three cornerstones of knowledge: history, philosophy, physical reality. In addition, he submitted likewise the basis of his beliefs in FQM. Here in the following excerpts I have been obviously banned a lot of statements but Selleri reported in detail a lot of quotations by the founding fathers of QM, being them supporters or opponents of the Copenhagen interpretation. In addition, so as not to overload the thesis with further notes, I have reported all the Italian original text of (Selleri *Critica Marxista* 1972) presented in the following four pages in Addendum 3, pp. 974-977:

[...] The cultural isolation of those involved in science has a twofold aspect: on the one hand, the researcher often does not feel the need to deal with literature, art or politics and prefers to devote all his energies to research driven by the desire to succeed, to "make a great discovery"; on the other hand, the non-scientists who try to approach research find great difficulties and not only for the obvious reason that they often do not have an adequate mathematical background, but above all because the researcher, with all the good will, is not able to explain to them in understandable terms the problem he is dealing with.

³²⁶ Selleri F. (1972), "Sull'ideologia nella fisica contemporanea", in *Sul Marxismo e le Scienze, Critica Marxista*, Quaderni n. 6, pp. 120-150. hereafter referred to as (Selleri *Critica Marxista* 1972).

This division between science and humanistic culture has been discussed at length and is usually presented as the problem “of the two cultures”. The essential causes of this situation are in my opinion to be found in the very structure of contemporary scientific culture and date back to three serious renunciations made by physicists in the period 1925-1936: the renunciation of history, the renunciation of philosophy and the renunciation of physical reality.

(a) The renunciation of history

The attitude of researchers towards the history of science is often of contempt and derision, and the more avant-garde is a laboratory or a university institute, the greater is the intolerance towards references to history and ignorance of it.

The history of science is considered an activity for elderly researchers who are now too much squeezed out and therefore unable to “produce” on the research front.

[...] The reasons for this explicit and voluntary renunciation by physicists and many other researchers to know the contradictions and travails from which their present culture was born are not difficult to identify, since this renunciation has been explicitly theorized and put into practice by some of the most important creators of the QM.

[...] The battle that [...] famous physicists [such as Einstein, Planck, Schrödinger, Ehrenfest, de Broglie] led, without success, against the final formulation of Bohr, Heisenberg, Born, Jordan and Dirac was a battle in defense of the concepts of physical reality, causality, comprehensibility and attack on the principle of complementarity [...].

[...] it will be seen that dialectical materialism accepts the existence of an objective reality independent of man, implies that it is comprehensible to us and affirms the validity of the principle of causality.

The position [of Einstein, Planck, Schrödinger and de Broglie] was [...] consistent with that of philosophical materialism [...]

(b) The renunciation of philosophy

The study of the links between scientists and philosophy is important to understand the historically determined, often very relevant, contents of scientific theories. This study is complicated by the fact that physicists almost always avoid agreeing with some philosophical current.

[...] causality in the microscopic world and the knowability lead to an inequality between measurable quantities that the same quantities, calculated with the QM, do not satisfy. We are therefore now to the point that the contradiction between QM and dialectical materialism can be decided experimentally.

However, the philosophical content of QM is unknown to the vast majority of researchers who use it uncritically believing they are dealing with a superior creation of the human intellect in no way contaminated by prejudice or conditioned by the society of which physicists are a part. There is therefore in the scientific community a rejection of philosophy which is then an unconscious adherence to a very precise philosophy.

If we consider philosophy as a kind of link between the various cultural activities, ignorance of the philosophical content of science can only isolate it from the rest of human culture and prevent an understanding of the social pressures and historical conditioning that favour the birth of modern science (Selleri Critica Marxista 1972, pagg. 120-133. My underline).

In order to avoid the cultural isolation between the two cultures, Selleri underscored the great importance of philosophy.

The third great renunciation is for Selleri that of physical reality. He made an analysis of the themes that we will find again in his scientific works starting from 1972 itself, focused on Bell's inequalities and EPR paradox. He also ranged to Marx, Engels and Lenin's criticism of positivism and the importance of dialectical materialism:

(c) The renunciation of physical reality

[...] Historically, the renunciation of physical reality and its understanding has taken place at different times:

1) In 1926-1927, with the works of Born and von Neumann there was the renunciation of the principle of causality, which is a direct attack on the comprehensibility of the material world [...].

2) In 1927, with the so-called Bohr's principle of complementarity there was the renunciation of the comprehensibility of microscopic phenomena, stating that certain contradictions that physicists had encountered in the study of atoms in the first quarter of the century (mainly the wave-corpuscle contradiction) are not solvable in principle [...].

3) Around 1935, with the "solution" of the famous paradoxes of Schrödinger and Einstein, one takes a hard blow to the same concept of physical reality, noting that QM implies that two objects that have been in contact in the past, however remote, can no longer be assumed to exist individually, however great their distance to the present.

[...] Anyone who knows the circles of scientific research knows that [pragmatism] is the dominant attitude.

[...] Positivism is also widely present in contemporary scientific ideology [...]. On the other hand the very birth of QM was strongly marked in this sense.

[...] Positivism was often attacked very harshly by materialists. One could recall significant statements by Marx on Comte; and by Engels on the positivists of the period 1880-1890, but perhaps it is more important to remember that Lenin dedicated a book to the attack of all forms of positivism with particular attention to that of Mach.

[...] contemporary scientific ideology contains [in addition to positivism and pragmatism] also elements of idealism and existentialism. Idealism is present in the theoretical description of measurement processes, according to which it is an intervention of the physicist's consciousness that carries out the measurement that ultimately determines the properties of the measured system. I cannot document this statement, because we could not avoid talking about state vectors, wave packet collapse and mixtures of first and second species³²⁷.

[...] We have seen that the modern researcher is left with only a narrow cultural corridor in his scientific thinking, since thick barriers have been erected towards society (renunciation of philosophy) and towards nature (renunciation of objective reality). It is also a half-way corridor, having been closed the door to the past by the renunciation of history. The remaining scientific activity therefore consists only of a series of impulses of theoretical and technological inventiveness with which we try to clarify certain experimental facts and incorporate them into some summary mathematical formula.

[...] we can conclude that [contemporary physical research] is heavily conditioned by cultural elements which are not only traditional for the bourgeoisie (idealism, positivism), but also irrationalistic (existentialism, pragmatism) as the bourgeoisie began to adopt as a reaction to proletarian struggles and the advent of socialism in the Soviet Union. Therefore, it seems far from coincidental that the QM was produced from 1924 to 1927 when Europe was shaken by a deep crisis and when in Central Europe the advent of Nazism to power was being prepared³²⁸.

[...] [Another] issue concerns the functionality for the capitalist system of bottling fundamental research in a dead-end corridor.

[...] On the other hand, a further advance in the understanding of the physical world could have reserved who knows what surprises and, perhaps, could have jeopardised the profitability of the investments made or questioned their control [of capitalists and imperialists] of research and technology.

It is therefore understandable that the bourgeoisie did not need to push the fundamental research out of the cul-de-sac it has been in for the last forty years.

The task of the working class is also to change this state of affairs and to encourage the development of productive forces also in the field of fundamental research.

[...] As we have seen [...] the clash that generated the QM took place around the concepts of physical reality, comprehensibility and causality. It is important then to observe that dialectical materialism affirms the existence of an objective reality that evolves in a causal way and affirms that this reality is comprehensible to men.

³²⁷ Here Selleri inserted a note referring to d'Espagnat's famous 1965 book. He also mentioned it here as a privileged reference for basic concepts. This article was published in August 1972. He wrote letters from Gothenburg in October 1972, so there is a further confirmation that, contrary to what Selleri himself quoted at Freire in 2003, and at the same time as what I attested in chapter 1, the knowledge of this book, which took place in Gothenburg, according to Selleri's own words, is previous to 1972 (and dates back to 1966).

³²⁸ I recalled here again that Selleri quoted many times the impressive and controversial work by Paul Forman.

[...] With regard to the problem of knowability, the position of materialism is that the physical world is certainly comprehensible to men, in the sense that they can conceive in their brains images of the objects studied corresponding to the true structure of these objects.

[...] It is therefore evident that for a dialectical materialist the position of the opponents of the QM (Einstein, Plank, Schrodiger, de Broglie,...) is more correct than that of the creators (Bohr, Heisenberg, Born, Jordan,...) (Critica Marxista 1972, pagg. 133-146. My underline).

From this long excerpt follows the essential union between the political vision linked to dialectical materialism and the philosophical choice of Selleri in the FQM and in his entire research activity, close to the positions of Einstein and de Broglie. As we have widely seen so far, Selleri was therefore influenced by the student movement, the PCI and the treatise on Marxism-Leninism published in the USSR and read in Lincoln, Nebraska, in the well-stocked campus Library. He was therefore fascinated by dialectical materialism. In a letter to Angelo Baracca, Selleri stressed on the “process of rebirth of the materialist current in theoretical physics”:

All in all, and given the poverty of others, I believe that we Italiots are quite at the forefront in this process of rebirth of the materialist current in theoretical physics (whether it is historical or dialectical I do not say, so we do not quarrel. To pass this objective fact through the subjectivity of the world's physicists is a difficult thing and I would like your political intelligence to be exercised on this problem³²⁹ (Selleri to Baracca, 24 May 1977-FSPA).

We will now see other important evidences of his beliefs, as well as an evolution of them over the years, at least until 1995, date of my latest findings on the subject.

³²⁹ “tutto sommato, e data anche la povertà altrui, credo che noi italiani siamo abbastanza all'avanguardia in questo processo di rinascita della corrente materialista in fisica teorica (che sia poi storico o dialettico non dico, così non litighiamo). Far passare questo fatto obiettivo nella soggettività dei fisici del mondo è cosa ardua e su questo problema mi piacerebbe che si esercitasse la tua intelligenza politica” (Selleri to Baracca, 24 May 1977-FSPA)

In the same 1972, Selleri wrote, jointly with Garuccio, an unprecedented pamphlet titled “Quantum Mechanics and Society”³³⁰, that I found in his Archive. Being written in English, maybe it was thought for a publication which never happened. At the beginning of the above pamphlet, he and Garuccio stated that:

The present paper intends to discuss those arguments which Kuhn does not touch³³¹, that is to say the connection between a scientific revolution (and its new “paradigm”) and the rest of the human culture. Kuhn limits his analysis to within that science in which the revolution takes place. Here we will try to see whether there was any pressure from outside which favoured the birth of Quantum Mechanics and whether the quantum mechanical paradigm had any influence on the society through its cultural consequences. The answer to both questions will be positive (Garuccio, Selleri 1972, pag. 1).

They aimed at revealing the basic role played by society and its pressure on contents of physical theories. They summarized in this way:

Briefly, our thesis is that the seemingly arbitrary contents of physical theories are a reflection of pressures arising from the society and its cultural currents. Sometimes this reflection is very clear as the choice made by the physicists building a new theory [...]. [This] seems to be the case for quantum mechanics. An effect of society on the birth of [the] new theory has been admitted either by implication or explicitly by some of the founders and of the opposers of Quantum Mechanics (Garuccio, Selleri 1972, pag. 5).

Then they explicated the lack of neutrality in a theory with respect the same society:

There is still another question of great interest. If a theory has socially determined contents, it cannot be neutral, once established, with respect to the actual contradictions of the society. Could it then not be that the theory is used by the scientists themselves, or by somebody else, outside the scientific domain to strengthen those cultural and political currents in reponse to which it was born and to attack the opposite currents? My opinion is that this is also the case in general. In fact, Quantum Mechanics was barely finished that an avalanche of books, papers, newspaper’s articles, radio trasmissions informed the people of the world of several cultural, political, social, religious consequences which

³³⁰ (Garuccio, Selleri 1972) in the following.

³³¹ In his outstanding and remarkable book *The Structure of Scientific Revolutions*.

one could draw from the new science: Materialism had been liquidated, teleology in nature could now be accepted³³², the concepts of God and free will were not contradictory to science anymore, and so on. The founders of the new theory themselves had an active part in spreading the new ideas outside of the scientific milieu. Such ideas were all qualified culturally and politically in the same direction, and constitute an illustration of the fact that no human creation, not even a scientific theory, can have a neutral social role. Briefly stated, the present paper is devoted to present evidence in favour of two main theses:

1. Quantum Mechanics was strongly conditioned, at its birth, by that irrationalistic cultural climate which established itself in Central Europe in the twenties and which favoured the advent of Nazism.
2. Quantum Mechanics was used in many occasions in attempts to restore idealistic conceptions in human culture (religion, vitalism, teleology and so on) and in attacks directed against the socialist societies and the materialistic philosophy (Garuccio, Selleri 1972, pagg. 6-7).

The first point is devoted almost entirely to the analysis of the impressive and controversial work of Paul Forman, *Weimar Culture, Causality, and Quantum Theory, 1918–1927: Adaptation by German Physicists and Mathematicians to a Hostile Intellectual Environment*, written in 1971; whereas the second one is showed with several testimonies against Marxism by Born and Heisenberg. Both sources are not reported here, though.

In this pamphlet is clearly underlined the reasons which took Selleri to a wide criticism of QM, both physical and philosophical, for the rest of his activity. Reasons which are well expressed in the point one and two of the last statement in the above excerpt.

Selleri applied to science the social and political contradictions in several papers written in Italian: “La crisi dei ricercatori”, published in 1971; “Ricerca scientifica, sviluppo economico e parassitismo nel Mezzogiorno: il CSATA”, written jointly with Guarino G. and Matteuzzi M.; “Ricerca scientifica e sviluppo delle forze produttive”, published in 1977; *Nucleare e solare come alternative al petrolio*, a book written with Garuccio A. and

³³² As Christian Harald Lauritz Peter Emil Bohr, the father of Niels Bohr, had always dreamed. This also happened in the wake of the philosophy of Kierkegaard and Høffding, which, together with Cubism and the philosopher William James, had a huge influence on Niels Bohr.

Guarino G. in 1980³³³; and “I sommergibili nucleari, la corsa agli armamenti e i rischi di guerra totale”, in *Scienza, armi e disarmo. 40 anni dopo Hiroshima*, in 1986.

Another paper I would like to analyze is rather a review made by Selleri and Garuccio and published on *Critica Marxista* in 1973, “Materialismo dialettico e meccanica quantistica”³³⁴, also released with the same title by Selleri alone on the Communist newspaper *L’Unità*, on 12 July 1973. They both concerned the book written by Omelyanovskij and Fock and edited in the Italian translation of 1972³³⁵. It sounds interesting because they pointed out how Italian Marxism is traditionally historical and how, in their beliefs, dialectical materialism is starting developing. And also because there was an impressive albeit brief analysis of this development, related to a new connection between working class and scientific research, also requested by the “Movimento Operaio” (Labor Movement) and the work of L. Geymonat. And lastly because there was also an unedited correspondence between Dmitrii Ivanovich Blokhintsev³³⁶, Selleri, Garuccio and the Director of the *Critica Marxista* with regards to some clarifications made by the Soviet physicist and related to the abovementioned paper. As well as the fast correction made by Selleri, in the name of his great fairness and his willingness of a correct information:

This book [...] contains twenty-nine writings by Soviet philosophers and physicists on the relationship between the logical and philosophical structure of quantum mechanics and the dialectical materialism of Marx, Engels and Lenin.

³³³ The book contains a very political analysis in terms dealing with the monoenergetic development based on the oil imposed in the 1960s in Italy, and shows a possible energy diversification for a recovery, even if partial and late, of national autonomy and for the needs of industrial and social development.

³³⁴ *Critica Marxista*, year 11, n. 3-4, May-August 1973, pagg. 303-319. (Garuccio, Selleri 1973) in the remainder.

³³⁵ Omelyanovskij M.E., Fock V.A., e al. (1972), *L’interpretazione materialistica della meccanica quantistica. Fisica e filosofia in URSS*, Milano: Feltrinelli Editore.

³³⁶ Dmitrii Ivanovich Blokhintsev (1908-1979), was a Soviet physicist, distinguished organizer of science, a colleague of Kurchatov in creation, formation and development of nuclear science, technology and nuclear power engineering. He is the author of the quantum ensemble concept. On the basis of this concept he gave an objective treatment of the wave function. This approach, being of large heuristic value, helps removing a number of internal contradictions in the interpretation of quantum mechanics and established a close connection between quantum mechanics and statistical physics. For further information, see also: <http://theor.jinr.ru/people/Blokhintsev/work.htm>, Accessed 24 September 2020.

Undoubtedly this collection arrives in Italy at a very opportune moment, while alongside Italian Marxism, traditionally historicist in its role of considering science, a current close to dialectical materialism is emerging, thanks also to the publication of the six volumes of L. Geymonat's *Storia del pensiero filosofico e scientifico* (History of Philosophical and Scientific Thought) and of the *Quaderno di Critica Marxista* "Sul Marxismo e le scienze" (On Marxism and the sciences), whose editorial success testifies to the growing interest in techniques concerning the relationship between the working class and scientific research. This interest is certainly not accidental, but it must be traced back, in the last analysis, to the need for a new type of economic development of our country, after the exhaustion of the type of development of the 1960s, and to the concrete proposals put forward by the political and trade union organizations of the Workers' Movement, proposals in which a very precise role of scientific and technological research as a vital element is indicated. The renewed interest in dialectical materialism in Italy is also often seen in the studies of L. Geymonat and his school, which led [...] to an understanding of the historical and economic causes of that dogmatic use of dialectical materialism that led to the Lysenko case. The value of the essays contained in this book also lies, in Geymonat's words, in the "[...] vigour with which they reaffirm the thesis of the inseparability of historical materialism from dialectical materialism".

[...] two data seem to emerge strongly: the first is the intense collaboration between researchers of the various disciplines and researchers and philosophers. [...] in the second place the constant commitment to enrich dialectical materialism with the most recent developments in science.

[...] All this may well lead to sad considerations on the Italian situation where not only the split between the two cultures remains deep, but there is even a situation of clear mutual isolation of researchers from different disciplines.

[...] A fundamental value of Tagliagambe's book consists in proposing [...] ideas about the nature of quantum mechanics of [...] Soviet physicists and philosophers. Thus we learn that in 1957 Fock convinced Bohr to change some of his statements, which could be called positivism, without however changing the substance of his positions. We also learn that the famous philosopher Omelyanovsky, who had long been an opponent of quantum mechanics, considered Bohr and Fock's new common position acceptable from the point of view of dialectical materialism. Finally, we learn that despite this important convergence of Fock, Omelianovsky and other physicists and philosophers, there remains in the USSR a strong group of dissenters (Tyapkin, Pakhomov, Isayev, Blokhintsev, Terletski and others) who, while recognizing the great successes of quantum mechanics, consider it incomplete or imperfect or incompatible with dialectical materialism³³⁷ (Garuccio, Selleri 1973, pagg. 303-305. My underline).

Concerning the foregoing correspondence with Blokhintsev I found four letters covering the period August 1976-January 1977.

³³⁷ The Italian original text is in Addendum 3, pp. 977-978.

In the first letter, Blokhintsev disputed the conclusions reached by Selleri and Garuccio and explained well his position, which does not challenge the QM in full but only in the interpretation of Bohr:

Dear colleagues, unfortunately, it is not very long ago that I had an opportunity to read your referee comments on the book “L’interpretazione materialistica della meccanica quantistica” published by Feltrinelli Editore, Milano, 1972, which comprises the works by soviet philosophers on the above problems.

In your comments you include me in the number of those physicists who consider Quantum Mechanics to be “imperfect, incomplete and incompatible with the dialectical materialism” (see “Critica marxista”, Roma, Maggio 1973, p. 303).

In fact I completely disagree with such a point of view and I have never and nowhere published something like that.

My conception of quantum mechanics differs from the Copenhagen one (as well from that of V.A. Fock) as I consider quantum ensembles to be objective reality independent of an observer and whether the observations and measurements are performed or not.

An example of such an ensemble is cosmic rays which represent quantum phenomena and are successfully studied on the basis of quantum mechanics. The role of an observer in quantum phenomena does not differ from that of an observer in classical phenomena. In both the cases his influence must be and may be either excluded or taken into consideration.

The understanding of quantum mechanics on the basis of the conception of quantum ensembles (a specific feature of which is a complementarity of the momentum and coordinate spaces) enables us to explain completely (not only verbally but also mathematically) all the paradoxes of quantum theory of measurements.

I would be very grateful to you if you could publish this letter in order to give a true representation of my ideas. Herewith I apply the references of my works on the concerned theme.

List of references: Criticism of ideas of the Copenhagen school; polemics with V.A. Fock. (Blokhintsev to Selleri and Garuccio, 31 August 1976-FSPA. My underline)

Selleri and Garuccio replied recognizing the misleading and trying to explain how they came to that erroneous conclusion:

We have read some of the papers which you quote in your letter and we think that [...] the quoted sentence makes no justice to your point of view.

We have reread all the statements concerning you on the book. We think they are misleading. To give you an example at page 124 it is written:

“not all the critics of Bohr’s ideas...follow the same path. Blokhintsev, for instance, kept stating the unacceptability, from the point of view of dialectical materialism, of the arguments advanced by the danish physicist and has become the leader of a fight against them [...] on his (Blokhintsev, our note) path advanced several soviet scientists (B.Y. Pakhomov, P.S. Isayev, A.A. Tyapkin, only to mention those who are present on this book) who, examining the solutions to the problem of quantum theory advanced by Fock and Omeljjanovsky, find that they cannot be considered exhaustive”.

It was probably this direct link between your ideas and those of Pakhomov and Tyapkin, more than anything else, that mislead us.

We send therefore your letter to *Critica Marxista* and ask the director to publish it in the next issue.

We hope very much to have the opportunity in the future to meet you personally and to discuss about these interesting problems. Sincerely yours (Selleri and Garuccio to Blokhintsev, 8 November 1976-FSPA. My underline).

Then Blokhintsev, after thanking, asked for the publication of his letter, which Selleri and Garuccio immediately did, proposing at the same time to publish an article by the Soviet scholar:

Thank you for your letter. I am very glad that the misunderstanding concerning my views on the essence of quantum mechanics is cleared.

I am grateful to you for publishing my letter in your journal and should appreciate it very much if you could send me the corresponding issue to our laboratory (Blokhintsev to Selleri and Garuccio, 2 December 1976-FSPA. My underline).

Selleri and Garuccio wrote to *Critica Marxista*:

We send you a letter from Blokhintsev concerning our article ‘Materialismo dialettico e meccanica quantistica’, *Critica Marxista*, year 11, n. 3-4 May-August 1973, which reviewed the book ‘L’interpretazione materialistica della meccanica quantistica’.

After having checked the bibliography proposed to us by Blokhintsev we have concluded that he is right and that he cannot be placed among the enemies of QM but only among those of Bohr’s formulation.

Our misunderstanding was born from the sentences of the book reviewed that speak of Blokhintsev (p. 104, 106-107, 124) and that make him a leader of the opponents (these yes real) of QM. We therefore ask you to publish the two letters [...].

It would not be bad if [Critica Marxista] invited Blokhintsev to present his ideas in more detail in a future article³³⁸ (Selleri, Garuccio to Critica Marxista, 10 January 1977-FSPA. My underline).

Currently I have no news about the possible publication of the paper by Blokhintsev requested by Selleri and Garuccio.

2.4.2-1970s AND BEYOND. SELLERI'S EVIDENCES IN SUBSEQUENT YEARS: 1980s AND 1990s.

I am showing here a collection of excerpts taken by several Selleri's letters I found in FSPA. I also reported here some titles of papers written by him in those years and regarding social and political subjects. Sometimes he changed his view in his attempts at a research based on the priority of a physical truth based on locality and realistic approach.

Five further letters that I found in Selleri's archive show in detail how his thinking on the main philosophical, social and political issues of this section has changed and at the same time they represented a reaffirmation of his belief. The letters are included in the time span from 1986 to 1999.

The first letter is sent to Enrico Bellone and takes up the themes of historicism and scientism:

I completely agree with you that historicism is a degeneration of the analysis of science, which denies its nature of knowledge of objective reality in a radical way. Hence to say that history plays no role in the development of a scientific theory, sounds very different. Sometimes I get the impression that you fall into the opposite trap, that of scientism, but then I remember your beautiful *Mondo di Carta* and try to erase that impression.
[...] If there is a risk that I run, I think, it is that of scientism, certainly not that of historicism.

³³⁸ "Ti mandiamo lettera di Blokhintsev riguardante il nostro articolo 'Materialismo dialettico e Meccanica Quantistica', critica marxista, anno II n. 3-4 maggio-agosto 1973 che recensiva il libro 'L'interpretazione materialistica della meccanica quantistica'.

Dopo aver controllato nella bibliografia propostaci da Blokhintsev abbiamo concluso che ha ragione e che non può essere messo fra i nemici della MQ ma solo fra quelli della formulazione di Bohr.

Il nostro fraintendimento è nato dalle frasi del libro recensito che parlano di Blokhintsev (pag 104, 106-107, 124) e che ne fanno un caposcuola degli oppositori (questi sì reali) della MQ.

Ti preghiamo pertanto di pubblicare le due lettere [...]. Non sarebbe male che [Critica Marxista] invitasse Blokhintsev ad esporre le sue idee in maggiore dettaglio in un futuro articolo" (Selleri, Garuccio to Manager of Critica Marxista, 10 January 1977-FSPA).

I think every day about the great and irreversible achievements of our science. However, I also see how the interpenetrations are as shit. Don't you see any? Everything is pure gold?³³⁹ (Selleri to Enrico Bellone, 5 May 1986-FSPA. My underline)

Here comes then a letter to Tito Arecchi. Selleri pointed out clearly his view, a confirmation of what we will see later, that the locality for him shall have priority over determinism, seen here as something to be challenged:

We agree to reject the scientist ideology. We agree against determinism and reductionism³⁴⁰ (Selleri to Tito Arecchi, 16 January 1989-FSPA).

Another hint comes by my interview to Michele Barone. In his opinion, Selleri had an evolution due to his encounter with Karl R. Popper in the early 1980s:

After the fall of the Berlin Wall, Franco married Popper and realism, which was the materialism of the time. Being a materialist and a Marxist was no longer fashionable (Interview with Michele Barone, Athens, 21 June 2019. My underline).

In support of this sentence by Barone, I found in my researches the following excerpt written by Selleri, completely unknown and belonging to a memo of his thoughts constructed as a chronicle in the mid of 2000s. Here and in the following he stressed several criticism to Marx and Marxism, even speaking about physics:

It is very hard to get rid of a slander after it has been widely disseminated, yet the accusation made against me of old Marxism seems to have lost interest, it seems to have gone out of fashion, no doubt after my prosecutors weighed every page I wrote.

³³⁹ "Io sono completamente d'accordo con te che lo storicismo è una degenerazione dell'analisi della scienza, che ne nega la natura di conoscenza della realtà oggettiva in modo radicale. Di qui a dire che la storia non gioca alcun ruolo nello sviluppo di una teoria scientifica ne corre, ma molta. Delle volte ho l'impressione che tu cada nella trappola opposta, quella dello scientismo, ma poi ricordo il tuo bellissimo *Mondo di Carta* e cerco di cancellare quell'impressione.

[...] Se c'è un rischio che io corro, credo, è quello dello scientismo, certo non quello dello storicismo. Penso ogni giorno alle grandi ed irreversibili conquiste della nostra scienza. Tuttavia vedo anche delle compenetrazioni di un intenso color marron. Non ne vedi tu? È oro puro?" (Selleri to Enrico Bellone, 5 May 1986-FSPA)

³⁴⁰ "Siamo d'accordo nel rifiutare l'ideologia scienziata. Siamo d'accordo contro il determinismo e contro il riduzionismo" (Selleri to Tito Arecchi, 16 January 1989-FSPA).

I can easily call myself a materialist and atheist, but with Marx - real or not real - there is an insurmountable problem, that of historicism. If the electron were the child of society as historicism claims, we would have to expect the existence of its intrinsic variability. The well-known philosopher of science Ludovico Geymonat argued the historicity of the electron, while physics has shown that the electron has a life surely longer than 10^{26} years and therefore enormously longer than the time of evolution of humanity on Earth which is estimated to be 10^6 years. According to Geymonat, there are movements within society that continuously restructure it, for instance movements that slowly modify the structure into social classes. Inevitably the productive activities of such a society will all be modified, including the production of science and the properties of the electron. But then the very existence of the electron itself becomes uncertain and a tendency or the opposite may prevail depending on the concrete circumstances at different times. There is no evidence that what is outlined above is actually the behavior of matter, indeed it should be stressed that the electron finds its place in all modern theories, from the first quantum theory of Bohr-Sommerfeld to the statistical theory of Fermi-Dirac. The electron is not a variable, it is not an optional, but an omnipresent property of matter. **And I am not a Marxist**³⁴¹ (FSPA-My underline and **bold**).

In the 1992 New Year's Eve, Selleri sent a long letter to Fabio Minazzi³⁴², one of his contact with whom he experienced a long correspondence, trying giving some tips for a contest. Here he stressed again a severe criticism on Marxism:

You know how I feel. **Real Marxism was a philosophical break-up that anticipated and prepared the political/economic/civil breakdown of the societies of real socialism.** It would be ridiculous to blame everything on philosophers, but so would forgetting what happened. I have already written to you that I went in search of philosophical alliances between Italian Marxists, French, Russian, etc. and that I did not find one of realists. **Real Marxism is conventionalism and historicism. It's a rip-off.**

If these things are painfully clear to me that I am materialist, anti-imperialist, anti-capitalist, sympathizer of the Rifondazione Comunista Party, try to figure out for the Italian philosophers. They take away my pain and replace it with contempt. For this reason I believe that it is a serious mistake today to qualify as Marxist. You will say that beyond labels the substance of what you write counts. It should in an ideal world.

Inside the cone of shadow, instead, labels count, too³⁴³ (Selleri to Fabio Minazzi, 1 January 1992-FSPA. My underline and **bold**).

³⁴¹ The Italian original text is in Addendum 3, p. 978.

³⁴² Fabio Minazzi (1955-): Italian philosopher who studied with Geymonat and Giorello. See https://www.uninsubria.it/sites/default/files/Curriculum_0.pdf. Accessed 24 September 2020.

³⁴³ The Italian original text is in Addendum 3, p. 979.

In 1995, Selleri wrote a letter to a Greek professor, A.E. Paparodopoulos, in which he stated his preferences for other scholars more than Marx and analysed how the praxis destroyed what for him was probably a perfect vision:

I liked reading the philosophical opinions of Lenin, Engels, Mao. Less so of Marx. In my opinion they raised very interesting points, still worth being considered seriously. Unfortunately the dogmatic and state-controlled repetition of some theses of dialectical materialism by so many people for so long has been a disaster for the general acceptability of this philosophy. I also cannot forget that Stalin, the killer, called himself a dialectical materialist. **But I am 100% materialist, and also consider as interesting the Hegelian/Engelsian dialectics** (Selleri to Paparodopoulos, 23 June 1995-FSPA. My underline and **bold**).

In a letter on 1999 sent to a young girl who has written to him, Selleri regarded himself as:

[...] son of that great mass criticism of scientism that was the student movement, even though I was [already] a young lecturer³⁴⁴ (Selleri to Luciana Negro, October 1999-FSPA).

The above concept already appeared in a 1998 letter sent to Gerardo Marotta, at the “Istituto Italiano per gli Studi Filosofici”:

[...] I can tell you, about me, that for almost thirty years I have been engaged in a battle for the radical renewal of physics. Many have laughed, though now they do so much less. [...] **I would just like to add that my choice was not born from an arrogant attitude, but from a deep adherence to the mass criticism of scientism that became established with the student movement in the late 60s and early 70s**³⁴⁵ (Selleri to Gerardo Marotta, 21 December 1998-FSPA. My underline and **bold**).

³⁴⁴ “[...] figlio di quella grande critica di massa dello scientismo che fu il movimento studentesco, anche se io ero [già] un giovane docente” (Selleri to Luciana Negro, October 1999-FSPA).

³⁴⁵ [...] Posso dirle, di me, che da quasi trent’anni ho ingaggiato una battaglia per il rinnovamento radicale della fisica. Molti hanno riso, anche se ora lo fanno molto meno.

[...] **Vorrei solo aggiungere che la mia scelta non è nata da un atteggiamento arrogante, ma da un’adesione profonda a quella critica di massa dello scientismo che si affermò con il movimento studentesco alla fine degli anni 60 e all’inizio dei 70** (Selleri to Gerardo Marotta, 21 December 1998-FSPA. My underline and **bold**).

As in other parts of this dissertation, the interview held with Freire always allows us to underline some interesting aspects on Selleri political views and beliefs as well as a useful summary. It deserves to be underscored how Selleri spoke with open hearts, deeply explaining his way of behaving with people. In addition, he pointed out how he gave up quite early from the politics, even if he continued to popularize science in all areas, including the cultural and political ones. And, above all, he pointed out, once again, the strong connection between the ideas of renewal of society and of physics. Freire asked Selleri on his “political views, roles and activities and their intersection, if any, with [...] philosophical and physical views” (AIP 2003). His deep answer follows down below:

The time at which I started being interested in the foundations of quantum mechanics were also times of great political unrest in the universities - you know, the famous years 1968 and 1969. So in all universities, in particular in Bari, there were students occupying the institutes and organizing political debates. I was then a young teacher, and for me it was very natural to side with the students. I thought they were right and they should be helped to improve our society. From that started for me a political activity that later for some time was also as a member of a Party. On one side for me it was a very important activity because I felt I made a cultural jump of quality. Before I understood nothing about man, and after I had the feeling that I started understanding how we work, how we reason, how we react - not always rationally, of course. But then it was also very frustrating because I was too naive in my approach. My method was to declare what I wanted to achieve openly, and then my behavior was as much linear as possible to achieve openly those aims. So it was very easy to shoot me down, a thing that happened several times, yes, because some people did not like what I wanted to do and could easily find all the information they needed to contrast my actions.

So from that point of view it was frustrating, but (I think, I hope) it was not really because I was too stupid. I thought that this was the right methodological approach, that you should be completely honest and transparent every time you try to do something. It does not work, I tell you. Anyway, for sure I tried to carry out this political activity. This lasted a few years, it was three years including those spent as director of the Bari INFN section, until I understood that I was going nowhere. I was losing my time and so I gave up. So I gave up politics very soon and I started doing my job, physics, full time teaching and research.

I thought that there was a strong connection, because **it seemed to me that the ideas of renewal of society and of physics had to go together**. There was (and is) a need for a completely different approach in physics and also for a different approach in society. So my approach also in physics has been always to be open and rational and to be honest and so on. But then in physics it is not so dangerous, because few people care, few people understand anyway what you are doing, so it is much more difficult to be shot down in physics than in politics. If the scientific proposals are sound it helps too (AIP 2003. My underline and **bold**).

So, there are a lot of confirmation of the strict intertwining between Selleri's social, philosophical, and political views, and his approach to the scientific research.

Finally, I decided to recall here again the outstanding letter Selleri wrote to Geymonat (see Chapter 1, page 144), for its compelling evidence of the influence linked to Selleri's move from PP to FQM, centered on Student Movement, on PCI, and a book on Marxism-Leninism found in Nebraska during the period 1970-1971, with the subsequent fascination for dialectical materialism:

[...] I am a former elementary particle physicist who saturated himself for ten years with complex angular momentum calculations, with the analytical properties of the S matrix, and other similar junk. Then the influence of the student movement first and later the PCI led me to a self-critical reflection and a crisis. You can't imagine the sense of liberation that one feels in going out of oneself, looking at what one is and what one has done in a new way, seeing its enormous narrowness, and at the same time the anger for having spent so much energy within a very narrow cultural corridor without light [...].

In short, capitalism has to pay for this too, and I have committed myself over the last two years to trying to fill the enormous cultural gaps that I have, because of the sterile super-specialization that I had and was forced to. Another stroke of lightning was the reading (in the USA!) of a Marxism-Leninism treaty published by the Soviet Union. How clear everything became in the light of the materialism-idealism contradiction! So there is no doubt that dialectical materialism fascinates me, even if I do not know how to answer the criticism [...] maybe only [for] a question of ignorance³⁴⁶ (Selleri to Geymonat, 3 October 1972-Archivio Ludovico Geymonat (ALG), box 75, folder 792. My underline).

³⁴⁶ The Italian original text is in Addendum 3, p. 979.

In conclusion, I have analyzed the role of social and political events during the Italian peculiarity of 1960s and 1970s on Selleri's scientific and philosophical choices. In the following I took into account his role at the Directive Board of Italian Physical Society (SIF). I aimed at showing how his choices and proposals inside SIF were fully related to his ideas and beliefs, too.

2.5-SELLERI IN THE STEERING COMMITTEE³⁴⁷ OF THE ITALIAN PHYSICAL SOCIETY: 1969-1970

In the foregoing landscape, the Italian Physical Society (hereafter referred to as SIF), maybe one of the main scientific institutions in Italy at that time, could certainly not stand indifferent or detached from the social and political context surrounding it. In the following, I will show how SIF made a lot of precise positions on political topics at that time.

SIF³⁴⁸ was born in 1897, thanks to “a committee of distinguished scientists, including Riccardo Felici, Augusto Righi, Antonio Pacinotti and Vito Volterra”. SIF deeply referred to the old *Accademia del Cimento* (an association of Galileo Galilei's disciples founded in 1675 by the Prince Leopoldo de Medici), as you can see in its logo³⁴⁹ as well as in the acquiring of the monthly journal *Il Nuovo Cimento*, created in 1844 and donated by Riccardo Felici, its sole owner. In the same year 1897, the journal “became a statutory property of the Society”, and from its January issue in 1897, the journal for the first time contained the subtitle ‘Organ of the Italian Physical Society’. The profound meaning of the concept behind the SIF logo is well represented by the statement of Giovanni Polvani, one (to say the least) of SIF presidents:

³⁴⁷ The Steering Committee participates at the meeting of Presidency Board, from whose excerpts I reported here all information.

³⁴⁸ All these information are present on the remarkable brochure made by SIF for its 120 years in 2017: “120 years and beyond”.

³⁴⁹ “The emblem adopted as the SIF logo reproduces the one printed on the front page of ‘Saggi di Naturali Esperienze fatte nell’Accademia del Cimento’ (Essays on Natural Experiments made at the Accademia del Cimento) (Florence, 1667), the Academy founded by Leopoldo of Tuscany and animated by Galileo Galilei's disciples. [...] A fireplace, three crucibles filled with molten metal and a fluttering decorative ribbon with the motto ‘provando e riprovando’ (‘trying and trying again’) are present in the emblem” (120 years and beyond, pag. 52).

“[...] Cimento in its pregnant meaning is at the same time the trial, the test, the effort, the risk, the peril, the experiment, the comparison, the thirst for knowledge, the extent to which the metal refines in the crucible. The crucible then is the mind, and the two words (‘provando e riprovando’) of the enterprise, mirroring each other, show the route to attain, by trying and trying again, the ‘beauteous truth’. It is the essence of Galilei’s method”³⁵⁰.

As reported in the booklet explicated in the footnote here below, “The aim of Italian Physical Society (SIF) is to promote, favour and protect the study and the progress of Physics in Italy and worldwide. SIF represents the Italian scientific community, in the research, educational and professional fields, both public and private, relevant to all areas of Physics and its applications”. SIF is also remarkable known for the birth, in 1953 (under the presidency of Giovanni Polvani) of the International School of Physics in Varenna, later named “Enrico Fermi”³⁵¹. SIF presidents (17, up to now) were always chosen between the great Italian physicists. I preferred not to bring any back here so as not to do any harm to those I had not mentioned. However, during the Triennium 1968-1970 and 1971-1973, also because for his deep professional relationship and collaboration with Selleri, I would like to underline that the president was Giuliano Toraldo di Francia (1916-2011)³⁵².

I managed to have the opportunity to see a lot of documents on SIF Archives (ASIF in the following), finding a lot of interesting hints. The methodology I used is based on the consultation of SIF Bulletin³⁵³, obviously published later, in which several previous meeting of the SIF Presidency Board as well as some Minutes of the Steering Committee, which attended to the same Presidency Board, are gathered.

³⁵⁰ (120 years and beyond, pag. 52).

³⁵¹ Enrico Fermi exposed an outstanding, unforgettable talk in the first edition of Varenna Summer School. He passed away the following year and the school was immediately named after him.

³⁵² For a brief biography, see, (120 years and beyond, pag. 61).

³⁵³ SIF Bulletin are obviously published much later than the meetings, being summary books.

Selleri had one of his first contact with SIF in 1961, three years after his degree, when he won the Prize of the Italian Physical Society for Young Researchers. On 31 December 1962 Selleri was on the SIF membership lists. His activity inside SIF could be highlighted seeing that at the date of 24 October 1967 he was a member of the seat for the elections of social offices. The entrance of Selleri in the SIF Steering Committee took place in a context in which the social and political events played a key and increasing role. Firstly, a consequence of the breakthrough in the social and political situation on SIF happened at general assembly of SIF shareholders, held in Rome at Physics Institute on 30 October 1968 (as reported on SIF Bullettin n. 63 of 28 November 1968), when the same Assembly was abruptly interrupted and postponed. At the beginning, the President, Toraldo di Francia read his overall report, with a very significant section on the general political orientation, clearly showing how the events already at that time were directly affecting the same SIF and the whole scientists. In my opinion, he gave a compelling and almost prophetic speech and vision, a breakthrough that well represented the strong intertwining between science and society in that years (as underscored by many scholars). For instance, Toraldo stated that:

[...] the historical development of the society in which we live has made it necessary to ask fundamental questions. We wonder precisely whether all the traditional aims are still to be pursued and whether this should be done in a traditional way. We also wonder whether there are also non-traditional goals that are becoming important today and that are worthy of due consideration in any case. [...] It is not easy to give in to a prevailing fashion. It is a matter of not burying one's head in the sand.

It is a matter of warning that in a world in rapid and tumultuous transformation, in which many traditional structures are critically examined and often rightly accused, the community of physicists cannot refrain from making an examination of conscience and taking its share of responsibility in building a new and, hopefully, better society.

[...] how can we not be assailed by serious doubts [...] when we see the competitive and frantic production of new science leading to increasingly frightening military applications and to an approach, implemented with all means on defenceless masses, of ever new artificial needs, which in fact turn into odious oppression?³⁵⁴ (SIF Bulletin n. 63 of 28 November 1968, pag. 13. My underline).

³⁵⁴ The Italian original text is in Addendum 3, pp. 979-980.

Concerning the last statement by Toraldo di Francia, I will recall in the following how in the 1970s several unrest were addressed to those scholars involved on military research. Then, he still continued arguing that:

[...] some positions have already been taken. I will mention, for example, the clear statement that for us compulsory schooling must be formative rather than selective [...]. A public position was also taken with a statement about student unrest and its repression. It was made clear, among other things, that SIF does not believe that the solution of those serious problems should be achieved by heavy authoritarian methods³⁵⁵.

Anyway, suddenly, at the end of the whole President report,

a representative of the Student Movement asks to read a document formulated by the Movement itself. Toraldo recalls that [...] the Statute of SIF does not stipulate that the meeting is secret, therefore he has no difficulty in admitting that non-members attend. However, it is clear that [...] non-members [...] may speak only with the permission of the General Meeting. Although in principle he is in favour of an interview with the students, before any intervention outside the Assembly, statutory reasons force him to complete the items on the agenda approved at the beginning of the meeting. Since the Student Movement with lively protests declares itself against the postponement, Ricci and Castagnoli propose to suspend the Assembly for 15 minutes to listen to the students' brief report. The proposal is put to a vote and approved by a large majority. After 15 minutes, the President reopens the meeting, but after a further intervention of the Student Movement that asks for the suppression of the prizes already assigned by SIF to physics scholars, having ascertained the impossibility to continue the work of the Assembly according to the practice and the Statute, he adjourns the session to a date to be allocated³⁵⁶ (SIF Bulletin n. 63 of 28 November 1968, pag. 10-11).

In light of these events, there will be two further communications in the same day. Firstly, in an emergency meeting it was argued that:

³⁵⁵ “[...] alcune prese di posizione sono già avvenute. Ricorderò, per esempio la chiara affermazione che per noi la scuola dell’obbligo deve essere formativa anziché selettiva [...]. Pubblica posizione fu presa anche con un comunicato a proposito delle agitazioni studentesche e della loro repressione. Fu fatto chiaro fra l’altro che la SIF non ritiene che la soluzione di quei gravi problemi debba avvenire con pesanti metodi autoritari” (SIF Bulletin n. 63 of 28 November 1968, pag. 14)

³⁵⁶ The Italian original text is in Addendum 3, p. 980.

following the forced interruption of the General Assembly of Shareholders on 30 October, the President at 6 p.m. on the same day convenes the Presidency Council in extraordinary session to take stock of the situation and take timely decisions [...].

After extensive discussion [...] the Council unanimously decided to continue the Congress at the Institute of Physics in Rome. With regard to the ceremony on 1 November, the Council unanimously decides to renounce this event in order to avoid that invited personalities, who have no responsibility for the progress of the university, may be exposed to any unpleasant incidents³⁵⁷ (My underline).

Secondly, the previous decisions materialized in a statement to the members:

The Presidency Council of SIF [...] in view of the impossibility to continue the work of the Meeting according to the practice and the Articles of Association of the Company, has updated the meeting to a date that will be communicated to the Members [...]. In view of the appearance of disruptive actions, openly ventilated by the Student Movement, which could give rise to regrettable incidents, the Presidency Council of SIF decided to postpone the official award ceremony³⁵⁸.

Hence, here the SIF was under pressure from the Student Movement, and, moreover, the counselor E. Gatti, left the sitting before the deadline having resigned his dismissal. That is the turning point that will lead to the entry of Selleri.

Indeed, in the 138th meeting of the SIF Presidency Board, dated 16 November 1968, it was approved that the “Extraordinary Shareholders' Meeting” must be held on 14 December 1968 in Florence. The agenda would discuss the replacement of the advisors E. Gatti, who resigned due to a strong disagreement on the political statements; and E. Polacco, who submitted a

³⁵⁷ “A seguito della forzata interruzione dell’Assemblea Generale dei Soci del 30 Ottobre, il Presidente alle ore 18 dello stesso giorno convoca in seduta straordinaria il Consiglio di Presidenza per fare il punto della situazione e prendere tempestive decisioni [...]. Dopo ampia discussione [...] il Consiglio decide all’unanimità di continuare il Congresso presso l’Istituto di Fisica di Roma. Per quanto riguarda la cerimonia dell’1 Novembre, il Consiglio all’unanimità delibera di rinunciare a tale manifestazione per evitare che personalità invitate, non aventi responsabilità nell’andamento dell’università, possano essere esposte ad eventuali spiacevoli incidenti” (SIF Bulletin n. 63 of 28 November 1968, pag. 20).

³⁵⁸ “Il Consiglio di Presidenza della SIF [...] vista l’impossibilità di proseguire i lavori dell’Assemblea secondo la prassi e lo Statuto della Società, ha aggiornato la seduta ad una data che sarà comunicata ai Soci [...]. Nel profilarsi di azioni di disturbo, apertamente ventilate dal Movimento Studentesco, azioni che potrebbero dare adito a deprecabili incidenti, il Consiglio di Presidenza della SIF ha ritenuto soprassedere alla cerimonia ufficiale di premiazione” (SIF Bulletin n. 63 of 28 November 1968, pag. 20).

letter of explanation with his resignation for personal reasons and to facilitate a proper rotation, without any political dissent. The two resignations were then unanimously accepted.

During the shareholders' assembly of 14 December 1968³⁵⁹, there was a huge debate on the President report, always arguing on appropriateness of political claims inside SIF, reported in the draft minutes of the shareholders' meeting:

Morpurgo [...] believes that the Council should have had the sensitivity to hold a referendum among Members before introducing political elements. [...] Declares that he will vote against the President's report because he believes that the entry of politics into Physics is detrimental to SIF. Cortellessa says that it is good [...] to specify what is meant by politics. He agrees that politics in the political Party way should not enter Physics. But he states that he would resign from a Physics Society that refused to discuss the facts of real life and did not make political choices in 1968. Just like he would resign from a partisan SIF. Potenza is associated with Cortellessa believing that one cannot not do politics. Zichichi expresses concerns about the contrast between the two different points of view. He asks for explanations for the terms "politics". A. Romano [...] wants the political aspects to be accentuated [...]. To pose general problems is to have a political attitude. Potenza rejects a purely scientific policy in favour of a more human policy. A. Bettini notes that we can finally talk about politics. Not doing so only means that political choices are made without talking about them. It calls for a greater social awareness of the category. [...] Morpurgo regrets that there is no longer any talk of physics in the SIF assembly but of ideologies and expresses the feeling that at least 50% of the Members aspire to become Honorable. S. Ratti suggests to see if the Statute allows for politics. If this is not the case, either the Statute is changed or politics is not done [...] Selleri agrees with the President's report.[...] Zichichi fears that political choices will be put before scientific and cultural considerations. Cortellessa reports that the President's report was unanimously approved at a meeting of his laboratory. Franzinetti congratulated the President on the realism shown [...]. Castagnoli agrees with Franzinetti and points out that the report appears to be politicised, partly because the Council has a mandate to propose a new Statute.

³⁵⁹ Here there is another example of strange temporal development of the dates of events. The Bulletin number 66 is of May 1969 and reported inside the 142° meeting of the SIF Presidency Board, held in March 1969, where the Assembly is described as well as the outcome of the election. But, these events happened on December 1968 and Selleri, once elected, started, as I will describe later, from 140° meeting of the SIF Presidency Board, held on 11 January 1969.

The Assembly must therefore respond if it wants an aseptic and academic SIF or a committed one. Borsellino recalls the historical-political aspects of SIF, born around the Nuovo Cimento, heir to that Cimento founded in 1844 by physicists who then in 1848 interrupted the publications to participate in the action of Curtatone and Montanara. He invites us not to deny a past to which we are all bound. [...] Salvini [...] enumerates the political problems to which he rightly sees Physics and vice versa other problems in which Physics has nothing to do with. For instance: Physics and Italian Society, yes; Physics and war yes, Physics and scientific value of Stalin no. [...] The President's report was adopted with 208 yes, 1 abstention and one vote against³⁶⁰ (SIF Bulletin n. 66 of 28 May 1969, pagg. 6-8. My underline).

The above debate shows a well-outlined lineup, with different political positions at stake, still corroborating the key role of politics also in the scientific institutions and scholars, with a huge number of scholars asking for a political and social debate.

There was also a long and intense debate (10 pages in length in the Bulletin) concerning the potential new counselors, Giulotto and Selleri, and how to proceed with their possible application and appointment. Indeed, while the members in the Assembly were deciding on how to replace the old advisors, E. Gatti and E. Polacco, suddenly emerged how Giulotto had sent two letters, the first one at the weekly magazine *L'Espresso* against the role of INFN, and the second one directly to Parliament with very serious accusations and “personal criticism of a slanderous and defamatory nature”³⁶¹. In the latter, Giulotto spoke about “a huge large but cleverly camouflaged case of university barony”³⁶² and went on talking about INFN, that:

[...] It is not an institution as everyone more or less could imagine, but an association with an undemocratic character [...]. The heads of this association and in particular Professors Gilberto Bernardini, Edoardo Amaldi and Giorgio Salvini, having been able to obtain for a long period of time relatively substantial funding outside the rules of the University and the CNR, were able to extend and consolidate their dominance over Italian Physics through research and personal assignments to friends, through the control

³⁶⁰ The Italian original text is in Addendum 3, pp. 980-981.

³⁶¹ Morpurgo, (SIF Bulletin n. 66 of 28 May 1969, pag. 9).

³⁶² “un caso di baronia universitaria di dimensioni colossali ma abilmente camuffata” Giulotto, (SIF Bulletin n. 66 of 28 May 1969, pag. 10).

of numerous university competitions and through the boycott of those who dared to openly express their dissent³⁶³.

And Giulotto still continued for the whole letter with this detrimental style, based however on unsubstantiated charges. Of course, that news resulted in a preliminary ruling for Giulotto election as a counselor, with several members who publicly expressed their lack of support for his election. Another debate ratified the need of new votes for the counselors, which resulted in the following (SIF Bulletin n. 66 of 28 May 1969, pag. 23, my **bold**):

29 unused ballots, 198 voters, absolute majority 100 votes. They got votes³⁶⁴:

F. SELLERI	122	G. BOATO	1
A. BONETTI	114	C. DILWORTH	1
G. SCOLES	76	A. ZICHICHI	1
L. GIULOTTO	55	G. MORPURGO	1
A. SONA	6	Blank ballots	4
F. FUMI	2	Spoiled ballots	4

So, after the happening of all these peculiar events, Selleri became advisor of the SIF Steering Committee.

Selleri took part to all meeting of the SIF Presidency Board, from the 140th one, held in 11 January 1969 to the 160th one, of 18 July 1970, being absent only in the 146th, of June 1969; and in 159th, of 12 June 1970. All these meeting are included in the SIF Bulletin going from n. 65 to n. 80. The former absence, still announced in SIF, was perhaps linked to his personal commitments, and it could be related to Selleri's participation to a conference at Lund, in Sweden, soon after the publication of his first paper in FQM,

³⁶³ “[...] non è un istituto come tutti più o meno potrebbero immaginarselo, ma una associazione a carattere non democratico [...]. I capi di tale associazione e in particolare i professori Gilberto Bernardini, Edoardo Amaldi and Giorgio Salvini, avendo potuto disporre per un lungo periodo di finanziamenti relativamente cospicui al di fuori delle norme dell'Università e del CNR, hanno avuto modo di estendere e consolidare il loro predominio sulla Fisica italiana mediante assegnazioni per ricerca e personali agli amici, mediante il controllo di numerosi concorsi universitari e mediante il boicottaggio di coloro che osavano manifestare apertamente il loro dissenso” (Giulotto, SIF Bulletin n. 66 of 28 May 1969, pag. 10).

³⁶⁴ “29 schede non utilizzate, 198 votanti, maggioranza assoluta 100 voti. Hanno ottenuto voti:”

presented there. But it is just my speculation! The latter was maybe due to the last stages of the organization of the International Summer School in Varenna, in which Selleri was the secretary.

Selleri played a remarkable role in contributing to several strong decisions taken in SIF as well as to the widening of the research on FQM, as always recognized by several scholars (see Freire 2014, for instance).

I am going to show the proposal and intervention he made in the nineteen meeting he took part to³⁶⁵.

--In the 140th meeting of the SIF Presidency Board, held in 11 January 1969:

The president opens the meeting by welcoming the two new counselors Bonetti and Selleri who contribute to effectively integrate the work of the SIF Board³⁶⁶.

There was a debate regarding the policy for letters publication in which:

Selleri expresses a favourable opinion on the publication of all those letters deemed interesting and which do not unjustly attack institutions and people³⁶⁷.

Giulotto's letter, already anticipated in the Extract of the Shareholders' Meeting of 14 December 1968, was then read again in this venue. And here Selleri started (documentally speaking) his engagement. And in a real strong way, too. Indeed,

Bonetti submitted to the Council a letter concerning Giulotto's writing, with explicit request for publication by the signatories (A. Bonetti, M. Conversi, C. Franzinetti, A. Romano, F. Selleri, C. Villi). Some Councillors show great perplexity about the advisability of publishing this letter, believing that it merely reopens a discussion now closed and is a repetition of what the signatories themselves have already clearly expressed in the Assembly and whose substance will appear in the minutes of the Assembly itself.

³⁶⁵ The citations relative to SIF Bulletin are present only at the end of the Italian text in the footnotes, unless the original text has been placed in Addendum 3.

³⁶⁶ "Il presidente apre la seduta dando il benvenuto ai due nuovi consiglieri Bonetti e Selleri che contribuiscono ad integrare efficacemente l'opera del consiglio SIF" (SIF Bulletin n. 65 of 14 April 1969, pag. 1).

³⁶⁷ "Selleri esprime parere favorevole sulla pubblicazione di tutte quelle lettere ritenute interessanti e che non attaccano ingiustamente enti e persone" (SIF Bulletin n. 65 of 14 April 1969, pag. 2).

Bonetti will inform the signatories of the objections raised during the discussion, reserving the right to re-submit the letter if they insist on its publication³⁶⁸ (My underline).

They insisted and so the letter, like Giulotto's reply, was published, as we can see in the excerpt of the 141st meeting of the SIF Presidency Board, held in 11 February 1969. It is important to underline how, in a confirming note by Bonetti to President Toraldo di Francia, it emerges how the letter of the six physicists was contemporary to the very day of the Assembly, when Giulotto's public letters were read:

As I had occasion to explain the letter had been agreed on the evening of 14 December 1968. The signatories consider that its publication is still valid. [...] A. Bonetti³⁶⁹.

Here we report only the condemnation letter submitted by Selleri with the other colleagues, whose introduction is written by Bonetti:

Letter from Proff. A. Bonetti, M. Conversi, C. Franzinetti, A. Romano, F. Selleri, C. Villi
To Prof. Toraldo di Francia-SIF President-Via Irnerio 46 Bologna

Dear President,

Following on from what I had the opportunity to explain at the last meeting of the President's Board, I would like to ask you to publish in the issue of the SIF Bulletin under preparation [...] the following statement by a group of SIF members:

We, the undersigned Members of the Italian Physical Society, having taken note of the letter sent by the Member Prof. L. Giulotto to the Italian Parliamentarians on 14 November 1968, which contains irresponsible judgments on the activities of the INFN and, in particular, serious statements of a defamatory nature against Prof. E. Amaldi, G. Bernardini and G. Salvini, accused of having favoured friends by allocating research and personal funds,

³⁶⁸ "Bonetti sottopone al Consiglio una lettera riguardante lo scritto di Giulotto, con esplicita richiesta di pubblicazione da parte dei firmatari (A. Bonetti, M. Conversi, C. Franzinetti, A. Romano, F. Selleri, C. Villi). Alcuni Consiglieri mostrano molta perplessità circa l'opportunità di pubblicare tale lettera, ritenendo che essa non faccia che riaprire una discussione ormai chiusa e sia una ripetizione di quanto i firmatari stessi hanno già chiaramente espresso in Assemblea e la cui sostanza figurerà nei verbali dell'assemblea medesima. Bonetti comunicherà ai firmatari le obiezioni sollevate nel corso della discussione riservandosi di rappresentare la lettera qualora essi insistano nella richiesta di pubblicazione" (SIF Bulletin n. 65 of 14 April 1969, pag. 3).

³⁶⁹ "Come ebbi occasione di spiegare la lettera era stata concordata la sera del 14/12/68. I firmatari ritengono che la sua pubblicazione sia ancora valida. [...] A. Bonetti" (SIF Bulletin n. 65 of 14 April 1969, pag. 47).

we declare that these statements and accusations are completely false, noting moreover how Prof. Giulotto - invited directly to cite the facts on which he had based his accusations - found himself unable to do so;

We condemn the method used by Professor L. Giulotto in spreading these lies by sending the aforementioned letter to political figures without informing those who were directly involved;

We also declare that the work of these professors, whose scientific validity is universally recognized both in Italy and abroad, has been one of the fundamental factors in the development of the many branches of Physics in Italy today.

These statements are in no way an expression of impatience towards criticism documented and correct in form, which, on the contrary, can open a dialogue of mutual interest between the various specializations of research. Florence, 14 December 1968.

The members: A. Bonetti, M. Conversi, C. Franzinetti, A. Romano, F. Selleri, C. Villi³⁷⁰.

In the same meeting, Selleri, who also at the University of Bari will deal with didactics with interesting projects, is here called into question about school policy:

With regard to the problem concerning the University, before [...] a Commission to deal with the Physics curriculum, an urgent mandate is given to Selleri and Ceolin to take a

³⁷⁰ “Lettera dei prof. A. Bonetti, M. Conversi, C. Franzinetti, A. Romano, F. Selleri, C. Villi.
Prof. Toraldo di Francia
Presidente della SIF
Via Imerio 46 Bologna

Caro Presidente,
facendo seguito a quanto ebbi occasione di illustrare nell’ultima seduta del Consiglio di Presidenza, ti prego di voler pubblicare nel numero del bollettino della SIF in corso di preparazione [...] la seguente dichiarazione di un gruppo di soci della SIF:

Noi sottoscritti, Soci della Società Italiana di Fisica, presa conoscenza della lettera inviata dal Socio Prof. L. Giulotto ai Parlamentari Italiani in data 14 Novembre 1968, che contiene giudizi irresponsabili sull’attività dell’Istituto Nazionale di Fisica Nucleare ed, in particolare, gravi affermazioni di carattere diffamatorio nei confronti dei prof. E. Amaldi, G. Bernardini e G. Salvini, accusati di aver favorito amici mediante assegnazioni di fondi per ricerca e personali, *dichiariamo* che tali affermazioni e accuse sono completamente false, constatando per di più come il prof. L. Giulotto - invitato direttamente a citare i fatti sui quali egli aveva basato le sue accuse - si è trovato nell’incapacità di farlo;
condanniamo il metodo usato dal prof. L. Giulotto nel diffondere queste menzogne mediante l’invio della citata lettera a personalità politiche senza informarne coloro che venivano direttamente chiamati in causa;
dichiariamo altresì che l’opera dei suddetti professori, la cui validità scientifica è universalmente riconosciuta sia in Italia che all’Estero, ha costituito uno dei fattori fondamentali nello sviluppo delle molte branche di Fisica oggi esistenti in Italia.

Queste dichiarazioni non sono in alcun modo un’espressione di insofferenza verso critiche documentate e corrette nella forma, che, anzi, possono aprire un dialogo di mutuo interesse tra le varie specializzazioni della ricerca.

Firenze, 14/12/68 I soci: A. Bonetti, M. Conversi, C. Franzinetti, A. Romano, F. Selleri, C. Villi” (SIF Bulletin n. 65 of 14 April 1969, pagg. 46-47. *Italics* in the original letter).

diligent part in obtaining all available information on governmental and parliamentary acts concerning the university reform³⁷¹.

--In the 141st meeting of the SIF Presidency Board, of 11 February 1969, besides the confirmation of the abovementioned letter by Selleri and the other five physicists, there is the confirmation of the commitment given to Selleri, regarding the university professors' career bill.

Indeed, on the problem of University Reformation, "the President invites Selleri and Ceolin to send as soon as possible to all Councillors the documentation they have collected on the subject following the mandate received in the previous Board"³⁷².

--In the 142nd meeting of the SIF Presidency Board, held in 15 March 1969, there is the aforementioned report on General Assembly of 14 December 1968, and, concerning its final minutes, Selleri took another commitment:

With regard to the draft minutes of the Shareholders' Meeting of 14 December 1968, Toraldo recalls that [...] had suggested that Selleri and Cini could have rewritten that part [of point 6] of the minutes or made the necessary corrections to the one drawn up in Florence. Selleri took on the task of compiling a new draft of the minutes and presenting it to the next Presidency Council; therefore, he asked those who had comments to make to send them to him promptly³⁷³.

The point number 6 provided for the replacement of resigning councilors E. Gatti and E. Polacco. The draft presented by Selleri (at the 143th meeting of the SIF Presidency Board) was challenged by Ceolin who received the task to make him another draft, which was then approved.

³⁷¹ "Per il problema riguardante l'Università, prima di [...] una Commissione che si occupi del piano di studi di Fisica, si dà mandato urgente a Selleri e Ceolin di farsi parte diligente per ottenere tutte le informazioni disponibili sugli atti governativi e parlamentari riguardanti la riforma universitaria" (SIF Bulletin n. 65 of 14 April 1969, pag. 3).

³⁷² "il Presidente invita Selleri e Ceolin a inviare al più presto a tutti Consiglieri la documentazione da loro raccolta sull'argomento a seguito del mandato ricevuto nel precedente Consiglio" (SIF Bulletin n. 65 of 14 April 1969, pag. 5).

³⁷³ "Riguardo la bozza del verbale dell'Assemblea dei Soci del 14 dicembre 1968, Toraldo ricorda che [...] aveva suggerito che Selleri e Cini avrebbero potuto stendere di nuovo quella parte [del punto 6] del verbale o apportare a quella redatta a Firenze le correzioni ritenute necessarie. Selleri si assume l'incarico di compilare una nuova bozza di verbale e di presentarla al prossimo Consiglio di Presidenza; pertanto prega coloro che hanno delle osservazioni da fare di fargliele pervenire tempestivamente" (SIF Bulletin n. 66, 28 May 1969, pagg. 1-2).

Regarding the 1969 SIF National Congress, having Bari given its availability through Michelangelo Merlin, the council “unanimously decides that the SIF Congress will be held in Bari from 27 to 31 October 1969. Toraldo proposes that already in the next Board we begin to study how to organize the Congress”³⁷⁴. The dates of the congress will then be modified to the period 28 October-2 November 1969 with a session dedicated to Scientific Research in the University, of which Selleri had collected material, as highlighted at the top of the previous page. And with the General Assembly scheduled for 30 October 1969.

I want to introduce a short parenthesis just to remember that the city of Bari has seen a General Meeting and four Congresses in the history of SIF:

26th General Adunanza, 12-18 October, 1933

XLIX National Congress, 6-13 November, 1963

LV National Congress, 28 October-2 November, 1969

LXVI National Congress, 16-21 October, 1980

XCV National Congress, 28 September-3 October, 2009.

In addition, the SIF Bulletin n. 71, released on 20 October 1969, was dedicated to LV (55th) SIF Congress held in Bari as well as several point in the agenda of the further meeting of Presidency Board.

Selleri submitted in Bari two talks in Particle Physics, a field still active with his works mentioned in Chapter 1 on the angular-momentum nonconservation.

Selleri had an invited paper on “Metrica indefinita, invarianza relativistica e momento angolare” (Undefined metric, relativistic invariance and angular momentum) on 30 October 1969 as well as another intervention on “Invarianza di Lorentz e vuoto non invariante” (Lorentz invariance and non-invariant vacuum).

³⁷⁴ “delibera all’unanimità che il Congresso della SIF sia tenuto a Bari dal 27 al 31 Ottobre 1969. Toraldo propone che già nel prossimo Consiglio si cominci a studiare come organizzare il Congresso” (SIF Bulletin n. 66, 28 May 1969, pag. 2).

Another point discussed in the 142th meeting concerned the journal *Il Nuovo Cimento*. After a great involvement of the Journal, even on an international level, the recent (at the time) birth of several different journals, really diversified on diverse topics, had opened new debates. Anyway, Selleri studied the situation and:

The President reads a letter from Selleri who, having studied the situation of the *Nuovo Cimento* and in particular the comparative evolution of the *Nuovo Cimento* and *Nuclear Physics*, found a significant decrease in experimental work.

Selleri's proposals to try to heal the situation are:

- 1) to accept directly all experimental work coming from major laboratories;
- 2) to triple the cost of subscriptions for rich countries only and to abolish publication costs.

Bassani does not believe that it is the cost of publication (except for the English and Eastern European countries) that influences the number of experimental works, but above all the fact that the *Nuovo Cimento* has gained prestige in the field of theoreticians rather than in the field of the experimental. Toraldo is inclined to believe that the most immediate measure to be taken is to try to shorten publication times as much as possible. Alberigi proposes to give more freedom to the Assistant Managers in the sense that, if they deem it appropriate, the experimental works coming from the great laboratories will be definitely accepted for publication. According to Alberigi, another significant improvement in time could be achieved if the authors gave up proofreading.

At the end of the discussion, as immediate measures, it is decided 1) to give more freedom of action to the Assistant Managers; 2) to inform the members of the Editorial Committee of some countries with currency conversion difficulties that the *Nuovo Cimento* is willing to publish free of charge a certain number of works per year from these countries³⁷⁵ (SIF Bulletin n. 66, 28 May 1969, pag. 3. My *Italics*).

But the 142nd meeting of the SIF Presidency Board takes on also a fundamental significance in my analysis on Selleri's contributions.

Indeed, at item five of the agenda, the Board talked about Varenna. Regarding the Courses for 1970, Toraldo notified the confirmation of the first two, on Geophysics and Critical Phenomena. As regards the third one:

Nuclear Physics is still pending as there is no news from Gigli and Ricci; therefore the decision on this course will have to be postponed until the next Council meeting.

³⁷⁵ The Italian original text is in Addendum 3, p. 981.

Selleri proposes that, from now on, consideration should begin to be given to a course to replace the Nuclear Physics course, should this course not be successful. Bassani considers a course on Biophysics [...] very interesting. Selleri proposes a course on the Foundations of Quantum Mechanics. Also Castagnoli is inclined to consider with interest the course suggested by Selleri. Eventually the direction of this course could be entrusted to Prof. d'Espagnat. Cini promises to study the issue and report back to the next Council³⁷⁶ (My underline).

So, it was Selleri that made the outstanding proposal, bringing forward the choice, without waiting for the next Council! And was Castagnoli³⁷⁷ who suggested d'Espagnat as Director of the School. Cini took on the task of studying the whole situation concerning the Selleri-Castagnoli proposal (as I might call it), and I think that reading the book of d'Espagnat, of course, could be a completion for his analysis. Actually, I can argue that, unlike an established practice which gives Cini a role also in this first decision, Selleri knew the book since 1966, as I demonstrated in Chapter 1, but Cini did not know the book, because he was going to read it, as declared in the following meeting (see down below).

The abovementioned proposal made by Selleri conducted to the breakthrough made by Varenna School (that I will introduce in the following paragraph), a real cornerstone in FQM, as declared by several scholars.

--In the 143rd meeting of the SIF Presidency Board, of 19 April 1969, the debate still continued at point six of the agenda. There is the confirmation of the fact that Cini did not read the book before the 142nd meeting; and the adoption of Selleri-Castagnoli proposal, being Selleri picked as Secretary of the School:

³⁷⁶ “Ancora in sospeso rimane in corso Fisica Nucleare mancando notizie da parte di Gigli e Ricci; pertanto la decisione su tale corso dovrà essere rimandata alla prossima riunione del Consiglio. Selleri propone che fin da ora si cominci a pensare ad un corso atto a sostituire quello di Fisica Nucleare qualora tale corso non dovesse andare a buon fine. Bassani ritiene molto interessante un corso sulla Biofisica [...]. Selleri propone un corso sui Fondamenti della Meccanica Quantistica. Anche Castagnoli è propenso a considerare con interesse il corso suggerito da Selleri. Eventualmente la direzione di tale corso potrebbe essere affidata al Prof. D'Espagnat. Cini si ripromette di studiare la questione e riferire in merito al prossimo Consiglio” (Minutes of the 142nd Steering Committee, pagg. 121-123. My underline).

³⁷⁷ Carlo Castagnoli (1924-2005): SIF vice-president at that time, and Director of the SIF journal *Giornale di Fisica*. See also <https://www.aif.it/fisico/biografia-carlo-castagnoli/>. Accessed 24 September 2020.

[...] With regard to the 1970 courses, Toraldo points out that [...] the third course therefore remains to be decided. Ceolin communicates that Ricci has come to the determination to renounce the course “Nuclear Structures” for 1970, reserving however to propose it again for 1971. The ‘Foundations of Quantum Mechanics’ course [...] is therefore resumed. Cini informs that he read d’Espagnat’s book and that he found it very interesting. After a wide-ranging debate, the Council expresses a favourable opinion on the course and decides to propose the direction to Prof. d’Espagnat, highlighting the interest of a comparison of the opinions of Daneri, Prosperi, Loinger and Wigner, Bohm, de Broglie. Selleri is proposed as Secretary of the Course³⁷⁸ (My underline and **bold**).

It could be really interesting to try to find the “wide-ranging debate” in detail. During my research at ASIF I did not find anything more, though.

In the same 143rd meeting, there were some debates concerning the Congress in Bari, showing the reasons why Selleri could not be the *trait d’union* between SIF and the same Congress:

The President recalled that [...] it is necessary to designate a person residing in the venue where the Congress will be held, who will have to take care of the logistics and who [...] will act as a link between Bari and the Bologna Secretariat. It is proposed to Selleri to take on this task, but he points out that in the months of August and September he will be out of office; therefore the President instructs Selleri himself to ask [...] Prof. M. Merlin to designate the person he considers most suitable for the purpose³⁷⁹ (My underline).

In this meeting, always in view of the Bari congress, it was again represented the long-standing problem of SIF awards, which never found a complete

³⁷⁸ “[...] Per quanto concerne i Corsi 1970, Toraldo ricorda che [...] resta pertanto da decidere il terzo corso. Ceolin comunica che Ricci è giunto alla determinazione di rinunciare per il 1970 al corso “Strutture nucleari”, riservandosi comunque di riproporlo per il 1971. Si riprende pertanto in esame il corso “Fondamenti della Meccanica Quantistica”[...]. Cini informa di aver letto il libro di d’Espagnat e di averlo trovato molto interessante.

Dopo ampia discussione, il Consiglio esprime parere favorevole relativamente al corso suddetto e delibera di proporre la direzione al Prof. d’Espagnat, mettendo in evidenza di ritenere interessante un confronto delle opinioni di Daneri, Prosperi, Loinger e Wigner, Bohm, de Broglie. Quale Segretario del Corso viene proposto Selleri” (Minutes of the 143rd Steering Committee, pagg. 146-148-My underline).

³⁷⁹ “Il Presidente ricorda che [...] occorre designare una persona residente nella sede in cui si svolgerà il Congresso, che dovrà occuparsi della parte logistica e che [...] farà da *trait d’union* fra Bari e la Segreteria di Bologna. Viene proposto a Selleri di assumersi tale incarico, ma egli fa presente che nei mesi di agosto e settembre si troverà fuori sede; pertanto il Presidente incarica Selleri stesso di pregare [...] il prof. M. Merlin di designare la persona da lui ritenuta più adatta allo scopo” (SIF Bulletin n. 67, 30 June 1969, pag. 4. *Italics* in the original text. My underline).

resolution even in future. Selleri showed his opposition to the prizes, still accepting at most only a rewarding for teaching³⁸⁰:

The President [...] recalls that since last year there were several concerns about the appropriateness of the award of the prizes [...] mainly on the part of the then member Prof. E. Polacco, who claimed that the scientific productivity prizes created an incentive to distort productivity and were counterproductive to educational activity. [...] Selleri declared himself opposed to the prizes; if they were to be awarded, he considered that only the teaching activity should be rewarded. Ceolin also expressed its support for the educational prizes. [...] Referring again to the teaching awards, Selleri suggests that the handouts could be rewarded³⁸¹ (My underline).

--In the 144th meeting of the SIF Presidency Board, held in 10 May 1969, during the President communication at the beginning of the meeting, based on the debate on the five-year plan, Selleri recommended that

as already done in previous meetings of the Council [...] the request for more funding for research is motivated by highlighting some concrete benefits that could be obtained by the country such as, for instance, the scientificization of the state administration etc. The President accepts this suggestion [...] ³⁸².

As to the organization of the SIF Congress in Bari, the delegate for the connection between Bologna and Bari was Dr. L. Nitti. Always concerning the matter of SIF awards, Selleri:

reiterates that only educational prizes should be awarded, as this is currently the field that needs the most encouragement. According to Selleri, by assigning exclusively educational awards for three or four consecutive years, SIF would demonstrate that it wants to correct the misconception that has always encouraged scientific productivity in

³⁸⁰ Think, for instance, to the protest of Student Movement as the General Assembly of 30 October 1968.

³⁸¹ “Il Presidente [...] ricorda che fin dall’anno scorso vi furono varie perplessità sull’opportunità dell’assegnazione dei premi [...] soprattutto ad opera dell’allora Consigliere prof. E. Polacco, che affermava che i premi di produttività scientifica creavano una incentivazione alla distorsione produttivistica ed erano controproducenti nei confronti dell’attività didattica.

[...] Selleri si dichiara contrario ai premi; qualora essi dovessero essere assegnati, egli ritiene che si dovrebbe premiare solo l’attività didattica. Anche Ceolin si dichiara favorevole ai premi didattici.

[...] Riferendosi ancora ai premi didattici, Selleri suggerisce che potrebbero essere premiate le dispense” (SIF Bulletin n. 67, 30 June 1969, pag. 3. My underline).

³⁸² “Come già fatto in precedenti riunioni di Consiglio [...] la richiesta di maggiori finanziamenti per la ricerca venga motivata mettendo in rilievo alcuni concreti vantaggi che potrebbe ricavare il Paese come, per esempio, la scientificizzazione dell’amministrazione statale ecc. Il Presidente accoglie tale suggerimento [...]” (SIF Bulletin n. 67, 30 June 1969, pag. 5).

the past, almost ignoring didactics. This way of preceding, Selleri adds, would contribute to shift a political emphasis in the general interest of the country. Cortini joins Selleri in recognizing the extreme need to consider teaching as a primary activity, remarking that at the moment in the University a career is made only for scientific merits³⁸³.

Then, Selleri filed a motion with received no amendments, even though it was rejected together with another one. Toraldo voted only for Selleri's motion, which was the following:

The Presidency Council of SIF, considering the disproportion that has existed so far in the evaluation of teaching and scientific activities in favour of the latter for the purposes of university career, considering that this disproportion is one of the causes of the lack of attraction that the career of secondary school teacher has had and is having on young graduates, a fact that is considered very serious for the social and economic development of the country, decides to emphasize the very high social value of the teaching activity, awarding cash prizes for only didactic merits related to the teaching of Physics for the year 1969. It stresses, however, that this has the meaning of an incentive gesture towards teaching, but that teaching and research are two essential and inseparable moments of university activity³⁸⁴.

Selleri was also unanimously nominated in the Commission that evaluated the applications for the teaching awards that took place in the National Congress of 1969 in Bari.

--In the 148th meeting of the SIF Presidency Board, of 1 October 1969, there was a long debate on the uncertain future of *Nuovo Cimento*³⁸⁵, tightened between various pressures, also of political type. Selleri was fully involved:

³⁸³ "Selleri ribadisce l'opportunità di assegnare *solo* premi didattici, poiché attualmente questo è il campo che ha bisogno di maggiore incentivazione. Secondo Selleri, assegnando per tre o quattro anni consecutivi esclusivamente premi didattici, la SIF dimostrerebbe di voler correggere l'errata mentalità che per il passato ha sempre incentivato la produttività scientifica quasi ignorando la didattica. Tale modo di procedere, aggiunge Selleri, contribuirebbe a spostare un accento politico nell'interesse generale del Paese. Cortini si associa a Selleri nel riconoscere l'estrema necessità di considerare primariamente l'attività didattica, facendo notare che attualmente nell'Università si fa carriera unicamente per meriti scientifici" (SIF Bulletin n. 67, 30 June 1969, pag. 6).

³⁸⁴ "Il Consiglio di Presidenza della SIF, considerate la sproporzione che è esistita finora nella valutazione delle attività didattiche e scientifiche a favore di queste ultime ai fini della carriera universitaria, ritenendo che tale sproporzione sia tra le cause della scarsa attrazione che la carriera di docente di scuola secondaria ha avuto e sta avendo sui giovani laureati, fatto che viene considerato molto grave per lo sviluppo sociale ed economico del Paese, decide di sottolineare l'altissimo valore sociale dell'attività didattica, attribuendo premi in denaro per soli meriti didattici connessi con l'insegnamento delle Fisica per l'anno 1969. Sottolinea tuttavia che questo ha il significato di un gesto di incentivazione verso la didattica, ma che didattica e ricerca sono due momenti essenziali ed inscindibili dell'attività universitaria" (SIF Bulletin n. 67, 30 June 1969, pag. 7).

³⁸⁵ I reported some information on *Nuovo Cimento* at page 90 in this dissertation.

[...] the question of the *Nuovo Cimento* arises in serious financial terms because, as is well known, action is being taken by various parties to try to limit the inflow of work to the *Nuovo Cimento* because of the heavy contributions to printing costs. [...] Selleri says that it is necessary to realize that the situation of the *Nuovo Cimento A* is not a situation that is deteriorating, but that it has already deteriorated, both with regard to the experimental works of high energy, which the authors, under the pretext of excessive contributions to printing costs, publish in other journals, and from the point of view of some foreign physicists, who in a recent meeting with Selleri, have complained about a clear decrease of the *Nuovo Cimento*. Therefore, timely and radical action is needed to save a dramatic situation. According to Selleri, the situation could be remedied by increasing the subscription to the *Nuovo Cimento*, which is much lower than that of other magazines, thus enabling SIF to eliminate immediately and forever the contributions to printing expenses.

Such action, in Selleri's opinion, should not involve excessive risks, considering that the majority of subscriptions come from libraries of large institutions that will not give up the subscription³⁸⁶ (SIF Bulletin n.73, 28 February 1970, pagg. 5-6).

Among the interventions, the counselor Ricci made a deep political analysis of the current crisis of *Nuovo Cimento* at that time:

Ricci believes that it is necessary to objectively see whether the problem of the *Nuovo Cimento* is only related to contributions and subscriptions, or whether there is a more political reason. Considering the *Nuovo Cimento* in its three areas, it can be seen that, as far as elementary particles are concerned, less work has been published since North Holland began publishing high-energy work on *Nuclear Physics*, despite the division of the *Nuovo Cimento* into *A* and *B*. As far as nucleus physics is concerned, the cause of the low demand for publication is to be found in the fact that abroad the *Nuovo Cimento* has not been able to fit among nucleus physicists such as *Nuclear Physics* and the *Physical Review*. Consequently, the works published on the *Nuovo Cimento* are rarely mentioned in the bibliographical references. As for the *Letters*, they have been successful because they have entered at a time when other journals claim letters too short.

The *Letters to the Nuovo Cimento* have a character of greater completeness and for this reason they find the consent of the authors³⁸⁷ (SIF Bulletin n.73, 28 February 1970, pag. 6).

³⁸⁶ The Italian original text is in Addendum 3, p. 982.

³⁸⁷ The Italian original text is in Addendum 3, p. 982.

[...] after extensive discussion, the Council shall hold the following deliberations:
 [...] Immediately take the most effective action possible against the main bodies of government in order to obtain funds for the total abolition of contributions to printing expenses.
 [...] To increase from three to four the monthly files of the *Letters to the Nuovo Cimento*.
 [...] Reinsert the physics of the nuclei in Part A of the *Nuovo Cimento*. Selleri dissociates himself from this decision, believing that it would have been possible to abolish the contributions to printing expenses today by increasing the subscription for libraries in rich countries by two or three times³⁸⁸ (SIF Bulletin n.73, 28 February 1970, pag. 7. My underline).

The journal *Nuovo Cimento* will live a shining period thanks to the diffusion of FQM, with the publication at international level of many articles, turning into a journal well considered for future publications.

--In the 149th meeting of the SIF Presidency Board, held in 11 October 1969, there was a decision concerning the SIF awards for didactics works:

[...] The Council [...] shall decide by an absolute majority to award only one prize for teaching experience.
 [...] Selleri dissociates himself from this decision, believing that the awarding of more prizes would have helped to encourage teaching and to demonstrate in a tangible way SIF's interest in this activity³⁸⁹ (My underline).

But, above all, during this meeting started the issue of NATO funds to Varenna School, a direct proof of the weight that social and political world situation played on institutional choices. The (what should I call this) “NATO funds” debate went through four diverse meeting of the SIF Presidency Board, from 149th to 152nd. Undoubtedly, it had to be a heartfelt theme.

Now I would like to introduce just an aside here on this issue.

It should be pointed out that all over the world, as a consequence of a growing social and political awareness of the non-neutrality of science, there

³⁸⁸ The Italian original text is in Addendum 3, p. 982.

³⁸⁹ “[...] Il Consiglio [...] a maggioranza assoluta delibera di assegnare un solo premio per esperienze didattiche. [...] Selleri si dissocia da questa decisione, ritenendo che l’assegnazione di un maggior numero di premi avrebbe contribuito ad incentivare la didattica ed a dimostrare in modo tangibile l’interesse della SIF per questa attività” (SIF Bulletin n.73, 28 February 1970, pagg. 9-10. My underline).

were several examples of precise stances and boycotts against scientists involved in military research as well as the writing of a wide supported *manifesto* (even in Varenna 1970 and 1972 Schools). Concerning boycotts,

[In 1972] political activism among the young scientists against the Vietnam War had escalated following the revelations of American official documents about the JASON project³⁹⁰. This project had gathered a number of elite American scientists, most of them physicists, to advise the US in defense matters, including the Vietnam War.

[...] On 13 June 1972 protests reached a new level when French activist physicists impeded the Nobel Prize winner Murray Gell-Mann, one of the JASON scientists, from giving a talk at the prestigious College de France. This event motivated a number of similar incidents in Europe and in the US with JASON scientists being publicly targeted and criticized for their activities related to the military.

[...] [For instance], with Gell-Mann again at CERN, in Geneva, Sidney Drell in Rome, and John Archibald Wheeler in Erice, Sicily. One of the most telling vents happened in Trieste, at the International Center for Theoretical Physics during the symposium [...] to honor the 70th anniversary of the physicist Paul A.M. Dirac, held on 18-25 September 1972.

The triggering events were the presence of Wheeler and Wigner, both well known to be JASON members and supporters of the American war in Vietnam. In addition to street demonstrations, including clashes with the riot police, there was the “Trieste Letter”, signed by 450 scientists or students. It is noteworthy that the only public reaction from the conservative quarters came from Wigner, who carried a poster at the symposium opening session with the words “I am flattered by your accusations. They are compliments for me” (Freire 2014, pagg. 217-219).

³⁹⁰ JASON is an independent group of elite scientists which advises the US government on matters of science and technology, mostly of a sensitive nature. The group was created in the aftermath of the Sputnik launch as a way to reinvigorate the idea of having the nation's preeminent scientists help the government with defense problems. It was established in 1960 and has somewhere between 30 and 60 members. Its work became famous, or rather, notorious, as the source of the Vietnam War's McNamara Line electronic barrier to prevent infiltrations, becoming the target of the anti-war movements, being formed also by activist scientists. Although most of its research is military-focused, JASON also produced early work on the science of global warming and acid rain.

JASON members, known informally as “Jascons,” include physicists, biologists, chemists, oceanographers, mathematicians, and computer scientists, predominated by theoretical physicists. Over the years, JASON has included eleven Nobel Prize laureates and several dozen members of the US National Academy of Sciences.

In 1958, a military-issues physics summer study program named *Project 137* was launched by physicists John Archibald Wheeler, Eugene Wigner, and Oskar Morgenstern. In the early 1960s, JASON had about 20 members. By the end of the decade it had grown to over 40 members. The Vietnam War had a significant effect on JASON's membership and research focus. A major initiative of JASON became the McNamara Line electronic barrier. (According to Freeman Dyson, a member of JASON, this research was actually not carried out by JASON, but by a group called JASON EAST). Public attention to JASON's involvement in the Vietnam War led to public criticism and attacks, even against JASON members who were not hawks; for example, MacDonald's garage was burned down and Richard Garwin was called a “baby killer”.

See [https://en.wikipedia.org/wiki/JASON_\(advisory_group\)](https://en.wikipedia.org/wiki/JASON_(advisory_group)). Accessed 24 September 2020.

As for the 1970 Varenna Schools in FQM (29 June-11 July) and the 1972 one in HoP (31 July-12 August), a lot of evening debates happened, out of the official programme; and two documents were produced and distributed, containing a deep political analysis on the connections between science and society; and against the use of modern technology for military purposes, respectively.

As to the former, titled *Notes on the connections between Science and Society. By a group of participants to the 1st course (on FQM) of the international school of physics “E. Fermi”*, it is worth underlying how it did not received a great sharing. There were also several witnesses describing how the statement was especially carried out by the Italian physicists.

The document did not receive any official status, and it was typewritten, duplicated, and distributed *ad personam*.

Anyway, I will mention it in further detail in the 2.6.2 section.

Concerning the latter, titled *Statement on Vietnam*, it was conversely widely shared and signed. The statement was officially approved by a majority of Varenna school participants, showing also a diverse involving with respect to Varenna 1970.

At the end of the statement, 59 signatures followed, to which were later added others, like, for instance 62 scholars of the Free University of Bruxelles and 5 scholars from Bari, included Selleri, who was absent to Varenna 1972 school and joined it at a later stage. Here follows the text of the above *Statement*:

In recent weeks, diplomats, journalists and responsible visitor to North Vietnam have reported the bombing of dykes by the US Airforce. Officials of the US government have acknowledged that several dykes have in fact been damaged by bombing. They have also admitted that the destruction of the system of dykes would inevitably cause the death of thousand of persons during the monsoon season.

The last tactics in the American war has been made possible by a systematic application of discoveries for military purposes. This has included the use of laser-bombs, antipersonnel shrapnels and remote-control fire mechanisms.

These new technologies have been fostered by scientists working in such projects as the JASON programs of the Institute for Defense Analysis. These programs have enlisted more than 30 top-rank physicists, including five Nobel prize winners. The operational use of scientific knowledge in the Indochina war is of particular concern to us as participants in the 1972 Varenna Summer School on the History of Physics. The application of science in modern society has been at the centre of our debates and we cannot overlook the professional participation of scientists in the waging of a war against the people of Vietnam. Our discussions have convinced us that it is no longer possible to separate our attitude on these issues from our professional activities. This is why we express, as scientists, and in the publication and institutions of science, our condemnation of those colleagues who have willingly involved themselves in the waging of this war, we ask that these issues should be honestly faced within the scientific community, wherever it meets. We also call for the immediate ending of the bombing of Vietnam and the total withdrawal of American forces in order to safeguard the independence and freedom of the peoples of Vietnam, Laos and Cambodia.

As concerns the 59 signatures, among others stand the names of F. Balibar, A. Baracca, S. Bergia, M. Cini, R. Cohen, M. De Maria, C. De Marzo, G. Diambrini-Palazzi, G. Ghirardi, J.M. Levy-Leblond, G. Giorcello, W. Goldstein, G. Jona-Lasinio, C. Philippidis, L. Rosenfeld, G. Segré, G. Toraldo di Francia, H.B.G. Casimir, and so on. I recalled them always following a personal and not exhaustive choice.

During the SIF Shareholders Assembly, six motions were even tabled! There were also many entries and for two of them, at least 350 Italian physicists gave their consent, showing, once again, how many scholars were involved in the social and political events of their time. And also stigmatizing the scientists involved in the JASON project.

First motion: “The SIF Shareholders’ Meeting endorses the Statement on Vietnam, approved by a majority of the participants in the 1972 Varenna School on the History of Physics [...], reiterates the condemnation of those physicists who have had, as members of the JASON division, a direct co-responsibility in the systematic application of scientific discoveries for the US imperialist aggression in Indochina and strongly invites SIF to exclude from any of its initiatives schools, congresses, conventions, etc., all scientists whose membership in the JASON division has been proven”.

[...] the second motion stated: “Proposal for a programme of scientific cooperation with the Democratic Republic of Vietnam. [...] we believe that it is within our possibilities to

take initiatives antagonistic to the activities of war physicists. Initiatives that contribute to strengthening defence and reconstruction capacity in the countries of Indochina most brutally affected by the American attack, which will help these countries now and after their victory to define a technical and scientific development programme. The Assembly of Shareholders therefore requests that SIF make direct contact with the scientific institutions of the Democratic Republic of Vietnam and its delegation in Paris for a comprehensive programme of scientific collaboration [...]"³⁹¹.

At SIF there was a very long debate which led to the approval of these two motions, the former unanimously approved, too.

In this new *scenario*³⁹², as a confirmation of this wide awareness to oppose to a science for military use, there was a proposal in 149th meeting, of 11 October 1969, on the possible request to be made for receiving NATO funds in the year 1970. Anyway, Selleri showed how his position was completely against any funding by NATO. He was the only which directly involved in the matter, putting his idea on record:

The President announced that the deadline for submitting the request for contributions to NATO for the 1970 Varenna Courses would expire on 15 October. The Council is therefore called upon to decide in good time whether or not it intends to make such a request. After a careful examination of the matter, the Council, although in majority in favour of not taking advantage for the future of the contributions coming from Institutions that institutionally do not have the purpose to devote themselves to research and teaching, given the impossibility to submit the matter to the Assembly of the Members in consideration of the above mentioned deadline of October 15, deliberates to forward the request in question for 1970, reserving every decision for the following year [...].

Selleri believes that NATO funding could already have been waived for 1970³⁹³ (My underline).

³⁹¹ The Italian original text is in Addendum 3, p. 983.

³⁹² SIF sent also a letter to the President of the USSR Science Academy in order to avert the danger that their colleague Sakharov (whose ideas were not shared by all in SIF nor were his attitudes approved) might be prevented from freely expressing his thoughts. SIF wrote for searching a positive step towards the civil promotion of peoples, to which everyone want to collaborate.

³⁹³ "Il Presidente rende noto che entro il 15 Ottobre prossimo scade il termine per presentare alla NATO la richiesta dei contributi per i Corsi di Varenna 1970. Pertanto il Consiglio è chiamato a decidere tempestivamente se intende inoltrare tale richiesta o meno. Dopo attento esame della questione, il Consiglio, pur essendo in maggioranza favorevole a non usufruire per l'avvenire dei contributi provenienti da Enti che istituzionalmente non hanno lo scopo di dedicarsi alla ricerca e all'insegnamento, vista l'impossibilità di sottoporre la questione all'Assemblea dei Soci in considerazione del suddetto termine del 15 Ottobre, delibera di inoltrare la richiesta in questione per il 1970, riservandosi ogni decisione per l'anno successivo [...].

The matter was also intensely discussed at SIF Congress in Bari, its responses appeared on the minutes of 151st meeting and the complete transcribed debate at the General Assembly was exhibited on the minutes of the 152nd meeting³⁹⁴.

--The 150th meeting of the SIF Presidency Board, of 27 October 1969, was held in Bari during the Congress. The President report, starting with a warm thanks to the Bari organization, was aimed at underlining also the general political orientation of SIF, showing a huge intertwining with political situation all over the world:

Let me first of all express the deep gratitude of SIF to my colleagues in Bari, in particular Prof. Michelangelo Merlin and Dr. Luigi Nitti who directly assisted him, for having accepted such an onerous task as to host an annual SIF Congress. It will not have escaped anyone how much care and intelligence have been spent to make the local organization efficient and to offer to the congressmen a pleasant stay in this nice and beautiful city. Many members still remember the Congress held here six years ago. The fact that Prof. Merlin and his collaborators, not unaware from previous experience of what awaited them, have so willingly hosted this congress recommends them to our deep gratitude. [...] Italian society and mankind as a whole are going through a sharply troubled period on an ideological level.

[...] Between the relatively easy task of identifying and criticizing structures or institutions no longer suitable for our times and the extremely difficult task of indicating in practice what to replace them, too many give in to the temptation to limit themselves strictly to the former. But one cannot honestly hold a scientific society like ours without thinking very seriously about the latter.

[...] The community of Italian physicists had to take an active part and assume its responsibilities in the difficult evolutionary situation of the country, rather than limiting itself to an agnostic and aseptic promotion of scientific knowledge. In other words, it was recognized the need to make “political” choices, while remaining, of course, far from “partisan” positions.

[...] Of course, there is no reason for the debate and political intervention to be strictly limited to our country.

Selleri ritiene che si sarebbe già potuto rinunciare al finanziamento NATO per il 1970” (SIF Bulletin n.73, 28 February 1970, pag. 10. My underline).

³⁹⁴ (SIF Bulletin n. 74, 13 April 1970, pagg. 6-29).

In this regard, I would like to recall, above all, two positions taken in the international arena: the one adopted and made public by the Presidential Council regarding the removal from the University of some physicists in Brazil and the one of solidarity with Czechoslovakian physicists, which I have had occasion to take up through statements, which those brave colleagues have published in the physics journal of their country³⁹⁵ (SIF Bulletin n.74, 13 April 1970, pagg. 1-3).

At the end of his report at the Assembly, Toraldo thanked several members of the Board, including Selleri, seen as a “keen synthesizer of data and facts of essential importance for the direction of the *Nuovo Cimento*”³⁹⁶.

--In the 151st meeting of the SIF Presidency Board, held in 15 November 1969, were displayed the consequences of the wide debate held in Bari General Assembly on NATO funds. The winning tendency was that of Selleri, contrary to the financing already for the year 1970:

The President informs that following the resolution of the Shareholders' Meeting of 30 October, he sent to NATO the letter of renunciation by SIF of the financing of the Varenna school³⁹⁷.

The wide debate as well as the outcome favorable to Selleri and others of the renunciation of NATO funds was not painless and created some damage. Alberigi, the secretary and cashier of SIF, resigned:

Alberigi states that [...] his resignation concerns the post of Secretary-Cashier, and not that of Councillor in this regard [...] Alberigi argues that the grounds relate purely to a budgetary issue but essentially to discrimination in terms of funding: this aspect is purely political. Alberigi states that he no longer feels he has to deal with the administration of the Company, sometimes having to implement decisions (such as NATO financing) on which he does not agree politically.

[...] Toraldo states that at this point all that remains to be done is to note with deep regret Alberigi's decision.

³⁹⁵ The Italian original text is in Addendum 3, pp. 983-984.

³⁹⁶ “acuto sintetizzatore di dati e di fatti di importanza essenziale per la direzione del *Nuovo Cimento*” (SIF Bulletin n.74, 13 April 1970, pagg. 9-10).

³⁹⁷ “Il Presidente informa che a seguito della delibera dell'Assemblea dei Soci del 30 ottobre, ha inviato alla NATO la lettera di rinuncia da parte della SIF al finanziamento relativo alla scuola di Varenna” (SIF Bulletin n.73, 28 February 1970, pag. 11).

He stresses that political dissent remains purely at this level, since mutual esteem is such that no one can deny it³⁹⁸ (SIF Bulletin n.73, 28 February 1970, pagg. 12-13. My underline).

But, concerning the issue of NATO funds, what happened actually at the General Assembly in Bari?

--The answer lied in the 152nd meeting of the SIF Presidency Board, of 16 December 1969, in which the abovementioned debate on NATO funds to the Varenna School during the General Assembly at Bari National Congress of 30 October 1969, was fully revealed.

Although it was not a totally new issue, nevertheless, during the Assembly, Carlo De Marzo³⁹⁹ was the first to raise the question:

The President said that SIF has a moral weight both nationally and internationally. Now moral weight involves certain choices. Looking at the budget, I have seen that a funding of 13.5 million comes to us from NATO. NATO is a military alliance and we know that the problem of the links between scientific research and military applications, with all its implications it entails, is much debated, particularly in the United States. I wonder if the SIF could not do without this money and politely refuse it, for the considerations I have outlined⁴⁰⁰ (My underline).

Then, the answer by Toraldo revealed, in my opinion, a severe problem, the fact that in the past NATO wanted to decide the guidance of the School, luckily giving it up afterwards:

The problem raised by member De Marzo is not new to the Presidential Council. [...] In the past, it has happened a few times that NATO, informed of the courses that were to be held and the directors to be invited, had let SIF know that it was not interested in a given course. SIF complained about this and NATO at one point said: all right, do the courses as you like, allocate the funding as you please.

³⁹⁸ The Italian original text is in Addendum 3, p. 984.

³⁹⁹ Carlo De Marzo (1941-2005): See <https://antares.in2p3.fr/News/Carlo/carlo.html>. Accessed 24 September 2020.

⁴⁰⁰ “Il Presidente ha detto che la SIF dispone di un peso morale sia a livello nazionale che internazionale. Ora il peso morale comporta certe scelte. Scorrendo il bilancio ho visto che un finanziamento di 13,5 milioni ci viene dalla NATO. La NATO è un'alleanza militare e sappiamo che il problema dei legami fra ricerca scientifica ed applicazioni militari, con tutte le implicazioni che comporta, è molto dibattuto, in particolare negli Stati Uniti. Mi chiedo se la SIF non possa fare a meno di questi soldi e cortesemente rifiutarli, per le considerazioni che ho esposto” (SIF Bulletin n.74, 13 April 1970, pag. 16. My underline).

[...] The problem has been constantly presented in the Presidency Board, even very recently. [We would like to] postpone any decision until next year. We have a certain decision in mind, but for it certainly a push from this Assembly has its weight as well⁴⁰¹ (My underline).

As told before, without any postponing, after an intense debate, the position of Selleri will be the winning one. Also Giorgio Cortellessa⁴⁰² played a key role, underlining the need to renounce to the NATO funds and still proposing a resolution to the Assembly:

[...] G. Cortellessa: [...] the Varenna School must remain open, but we can waive the NATO contribution. So I certainly join De Marzo and I do not know if he will present a motion to that effect, because otherwise I will present it. I must remember that other Physics Societies, not in any way suspecting of having very strange political inclinations, have given a decisive cut in contributions of this kind. [...] So I say, let us do without them. (My underline).

[...] G. Toraldo: [...] it is true that in law and in fact today NATO does not place any restrictions on us.

[...] given that [...] we must write on the notice that the school is also financed by NATO, many colleagues from the Eastern countries that we would like to invite may have a heavy apprehension to accept. Thus, participation can be indirectly limited.

[...] [The] motion relating to NATO, signed by De Marzo-Cortellessa, reads as follows: “The SIF Assembly [...] having noted that the budget shows financing from NATO, decides to kindly renounce this contribution in order to clearly establish the independence of SIF from any organization or activity of a military nature” (My underline).

[...] G. Toraldo: [Coming back to NATO], the Presidency Council in its majority, although it may not have been unanimous, would very willingly have considered the decision to do without NATO funds. But, given the importance of the matter, it did not feel like taking action without consulting the Assembly (My underline).

⁴⁰¹ “G. Toraldo: Il problema sollevato dal socio De Marzo non torna nuovo al Consiglio di Presidenza. [...] In passato è successo qualche volta che la NATO, informata dei corsi che si volevano svolgere e dei direttori da invitare avesse fatto sapere alla SIF che un dato corso non le interessava. A questo atteggiamento la SIF ha protestato, e la NATO ad un certo punto ha detto: va bene, fate i corsi come volete, i finanziamenti destinatele come vi pare. [...] Il problema si è continuamente presentato in Consiglio di Presidenza, anche molto recentemente. [Vorremmo] rimandare ogni decisione all’anno prossimo. Abbiamo bensì in animo una certa decisione ma per essa certamente anche una spinta dall’Assemblea ha il suo peso” (SIF Bulletin n.74, 13 April 1970, pag. 16. My underline).

⁴⁰² Giorgio Cortellessa (1929-2018): See <https://www.sif.it/riviste/sif/sag/ricordo/cortellessa>. Accessed 24 September 2020.

[...] G. Bellettini: [...] I will gladly vote for this motion. [...] It is much better [...] to maintain in all cases a complete independence in all its own scientific policy⁴⁰³ (My underline).

Cortellessa still continued explaining his knowledge of NATO policy on funds:

[...] I would like to reject the interpretation that NATO does not place any conditions on contributions. This is a well-known fact, it has been discussed hundreds of times in the newspapers. The technique that is normally followed by NATO, which provides scholarships, awards to scientific societies, is as follows: NATO gives funding for research and asks for more money for its activities. At a time when NATO funding is being questioned, it cuts research funding, resulting in all the people who are in trouble at the time scream because NATO's budget is being cut back⁴⁰⁴.

And he also explained how the contribution had to be stopped immediately:

[...] The way the motion is worded, when it is passed, SIF must write a letter to NATO saying: from the next budget we thank you for your contributions made and we do not want to have them anymore⁴⁰⁵.

C. Ballario exposed a severe story against the NATO policy:

[...] The other thing I have always had concerns about these contributions is the fact that a colleague who won a NATO grant did not get where he wanted to go because there was a veto. The person belonged to a well-defined political party. And in my opinion this was an intolerable thing and I am sorry it happened with the complicity of the Italian authorities⁴⁰⁶ (My underline).

⁴⁰³ The Italian original text is in Addendum 3, pp. 984-985.

⁴⁰⁴ “[...] [G. Cortellessa: Vorrei respingere quella interpretazione per cui la NATO non mette nessuna condizione ai contributi. Questa è una cosa ben nota, è stata discussa centinaia di volte sui giornali. La tecnica che viene seguita normalmente dalla NATO, che fornisce le borse di studio, i premi alle società scientifiche, è la seguente: la NATO dà dei finanziamenti per la ricerca e chiede altri soldi per le sue attività. Nel momento in cui si mette in discussione un finanziamento NATO, questa taglia i fondi della ricerca, ottenendo che tutte le persone che in quel momento vengono messe in difficoltà strillino perché il bilancio della NATO viene decurtato” (SIF Bulletin n.74, 13 April 1970, pag. 25).

⁴⁰⁵ “G. Cortellessa: Per come è formulata la mozione, nel momento in cui è approvata, la SIF deve scrivere una lettera alla NATO dicendo: noi a partire dal prossimo bilancio vi ringraziamo per i contributi dati e non vogliamo più averli” (SIF Bulletin n.74, 13 April 1970, pag. 25).

⁴⁰⁶ “[...] C. Ballario: [...] L'altra cosa per cui ho sempre avuto delle perplessità su questi contributi è il fatto che un nostro collega vincitore di una borsa NATO non è riuscito ad andare dove voleva perché c'è stato un veto. La persona apparteneva ad un ben definito partito politico. E questa secondo me è stata una cosa intollerabile e mi dispiace sia avvenuta con la complicità delle autorità italiane” (SIF Bulletin n.74, 13 April 1970, pag. 26. My underline).

In conclusion, the Cortellessa motion was approved:

[...] The vote, by show of hands, gave the following result: 88 for, 23 against, 41 abstentions. [...] The motion is approved by an absolute majority⁴⁰⁷ (My underline).

As we have seen, after this vote, held in 30 October 1969, Toraldo sent to NATO the letter of renunciation to the funds starting by 1970 Varenna School, notifying it to the Presidency Board on the 151th meeting, of 15 November 1969.

The foregoing debate and solution is another piece that confirmed how SIF was certainly not alien to the political upheavals spread throughout the world. Selleri remained weirdly outside by the direct debate, even though he had expressed very clearly his position during the 149th meeting of the SIF Presidency Board, held in 11 October 1969.

--In the 153rd meeting of the SIF Presidency Board, of 10 January 1970, Selleri made another remarkable proposal, the future 1972 Varenna School in History of Physics, another cornerstone in SIF activity, even if Selleri did not take part in it.

In the meeting, a debate on 1971 Varenna School occurred. After the acceptance of the first two courses, Toraldo stated that:

As far as other courses are concerned, there are currently no further proposals, so, according to Toraldo, it would be a good idea for the Council to take political action and move towards an appropriate choice.

Selleri expresses the opinion that it would be necessary to take into account the need, which emerged at the meeting of SIF Members in Bari, in the 55th Congress, of the deepening of the concept of research.

He therefore proposes to hold a course in Varenna on the History of Science.

Toraldo declares to have thought about a course that could have as an absolutely indicative title "Philosophy and History of Science". Such a course, if well realized, could constitute something really new for Varenna.

Emphasis should be placed on the social implications of study and research in physics, with particular regard to the modern age⁴⁰⁸ (My underline and **bold**).

⁴⁰⁷ "[...] La votazione, per alzata di mano, dà il seguente risultato: 88 favorevoli, 23 contrari, 41 astenuti. [...] La mozione è approvata a maggioranza assoluta" (SIF Bulletin n.74, 13 April 1970, pag. 29. My underline).

The Varenna School in History of Physics took place the year after this debate, i.e., in 1972, and was preceded by very long debates. Nevertheless, I decided to override this topics, due to the absence of Selleri to the debate and at that School. I would only recall how Selleri played a proposing role also in the birth of the 1972 School.

In the minutes of 153rd meeting is reported as, always in Bari, there had been meetings in the particular section on ‘Research in the University’. Selleri also “distributes to each councilor a short document on the ‘Role of Scientific Research in the Italian Society’, prepared following the mandate given to him by the council” (SIF Bulletin n. 73, 28 February 1970, pag. 64).

--In the 154th meeting of the SIF Presidency Board, held in 7 February 1970, Selleri presented his document relating to the role of “scientific research in the University”, stating that in his opinion:

[...] the conference should take the form of “Research taken in a political sense”, i.e., set up as a democratic debate from which recommendations could eventually emerge which, although not committing the SIF, will serve to give a useful orientation to the organizational problems of the Research⁴⁰⁹.

[...] After extensive discussion, the Council declares itself in favour of one or two days of study and gives a mandate to Selleri to rework a new scheme taking into account the suggestions that emerged during the debate⁴¹⁰.

The study days, titled *Study Day on Science in Capitalistic Society*, were then held in Florence on 13-14 June 1970⁴¹¹, and the 161st meeting also proposed

⁴⁰⁸ “Per quanto riguarda altri corsi, al momento attuale non esistono ulteriori proposte, pertanto, secondo Toraldo, sarebbe bene che il Consiglio espletasse un’azione politica orientandosi verso un’opportuna scelta. Selleri esprime il parere che occorrerebbe tener conto della esigenza, emersa nella riunione dei Soci della SIF a Bari, nel 55° congresso, dell’approfondimento del concetto di ricerca. Propone perciò di tenere un corso a Varenna sulla Storia della Scienza. Toraldo dichiara di aver pensato ad un corso che potrebbe avere come titolo assolutamente indicativo “Filosofia e Storia della Scienza”. Tale corso, se bene realizzato, potrebbe costituire qualcosa di veramente nuovo per Varenna. L’accento dovrebbe essere posto sulle implicazioni sociali dello studio e della ricerca in fisica, con particolare riguardo all’epoca moderna” (Minutes of the 153rd Steering Committee, pagg. 182-183).

⁴⁰⁹ “il convegno dovrebbe avere la forma di “Ricerca presa in senso politico”, impostato cioè come un dibattito democratico dal quale potranno eventualmente scaturire raccomandazioni che, pur non impegnando la SIF, serviranno comunque a dare un utile orientamento ai problemi organizzativi della Ricerca” (Minutes of the 154th Steering Committee, pag. 185).

⁴¹⁰ “Dopo ampia discussione il Consiglio si dichiara favorevole ad una o due giornate di studio e dà mandato a Selleri di rielaborare un nuovo schema tenendo conto dei suggerimenti emersi nel corso della discussione” (Minutes of the 154th Steering Committee, pag. 189).

⁴¹¹ The study days are described in (Baracca, Bergia, Del Santo 2017).

the publication of the Proceedings, which were released by the editing house De Donato, addressed in Bari⁴¹².

--In the 155th meeting of the SIF Presidency Board, of 2 March 1970, Selleri started another breakthrough, writing the well-known letter titled *La piramide azteca della fisica teorica delle particelle elementari* (SIF Bulletin n. 75, 4 May 1970. See Chapter 1, pagg. 135-138). As we have seen, it gave birth to an interesting debate thanks to the contribution by E. Fabri (in the 158th meeting and published on SIF Bulletin n. 77, 10 August 1970) and A. Baracca and S. Bergia (in the 164th meeting and published on SIF Bulletin n. 82, 14 June 1971).

In this same meeting, Toraldo di Francia wrote a deep and very remarkable programme letter (a sort of “spiritual legacy”) to the members for the expiry of the term of office of the SIF Presidency Council, emphasizing once more how SIF was deeply influenced by political events as well as his peculiar thought (“It will only take a few things to show where I stand, to those who do not already know”):

[...] we cannot hide [...] the fact that the ideological differences (in the broadest sense) between physicists have never been as lively as they are today. Personally I do not regret this. I believe that admitting frank and broad discussion and accepting the contribution of all, especially the young, is much better than following closely the guidance of a few great enlightened masters. [...] It will only take a few things to show where I stand, to those who do not already know. [About the University] I think the protest was the best thing that bourgeois society has been able to express in recent years. Its effects on the conception of the School and the University in particular, are beneficial and I hope irreversible. The University must be democratized with the participation of all components at all levels (SIF Bulletin n. 75, 4 May 1970, pag. 11. My underline).

[...] I have no doubt that the increased politicisation of SIF, recently occurred, is a good thing. To lock oneself in the ivory tower of studies and lose sight of the reality and interests of Italian society would be pernicious. Surely I would not be interested in a SIF so made⁴¹³ (SIF Bulletin n. 75, 4 May 1970, pag. 12. My underline).

⁴¹² SIF (Ed.), (1971), *La Scienza nella Società Capitalistica*, Bari: De Donato.

⁴¹³ The Italian original text is in Addendum 3, p. 985.

--During the 157th meeting of the SIF Presidency Board, held in 18 April 1970, the whole Summer Courses 1970 of the International School of Physics “Enrico Fermi” at Varenna was submitted.

During the year 1970, thanks to the contributions from the Ministero della Pubblica Istruzione, the Consiglio Nazionale delle Ricerche, the Comitato Nazionale per l'Energia Nucleare, Italian Authorities, Companies and Organizations, the following Summer Courses will be held at Varenna in the Villa Monastero, the use of which has been kindly granted by the Ente Villa Monastero.

Foundations of Quantum Mechanics

1st Course 1970 (IL since the beginning of the School) 29th June-11th July

Subject-matter:

- 1) Conceptual problems raised by the linearity of the quantum laws. (Nonseparability at a distance, nature of the observation act, etc) Measurement of fields.
- 2) Macroscopic bodies. Does irreversibility account for measurement?
- 3) What is an observable?
- 4) Bohr and von Neumann's epistemologies. The informal language of physics.
- 5) Causal theories, distant correlations and possible breakdown of QM. Discussion of current and planned experiments.
- 6) QM without wave reduction. The relativity of state theory.
- 7) Recent developments in the axiomatics of QM (SIF Bulletin n. 76, 8 June 1970, pagg. 4-5)

--In 158th meeting of the SIF Presidency Board, of 9 May 1970, the openness towards the politically dense times experienced by humanity in those years, take the features with the disclosure of publishing a letter on Vietnam sent on May 1970 by forty-five US scientists to President Nixon:

[...] We agree with the stand of many that the health of this nation depends on a swift disengagement from war in Southeast Asia, although the violent tactics used by some to express this are repugnant to us.

[...] We ask simply and urgently [...] a rapid termination of US participation in the Southeast Asian War (SIF Bulletin n. 77, 10 August 1970, pag. 14).

Between the signature, fully aware that the choice is only personal and by no means exhaustive, I would like to draw attention on the names of Luis Alvarez, John Bardeen, Hans. A. Bethe, Felix Bloch, Owen Chamberlain,

Max Delbrück, Richard P. Feynman, Murray Gell-Mann, Donald A. Glaser, Robert Hofstadter, S.E. Luria, Linus Pauling, Edward M. Purcell, Emilio Segrè.

--The 161st meeting of the SIF Presidency Board, of 8 September 1970, officially marked the end of the presence of Selleri in the SIF Presidency Board for his noteworthy stay at Lincoln, Nebraska, from 1 September 1970 to 31 May 1971. He was already absent and Toraldo read a letter:

The President brings to the attention of the Board a letter from Selleri indicating that he will no longer be able to attend Board meetings because he will have to stay in America for a long time, as he had already indicated when he was appointed Councilor. Selleri also announces that he will not stand for election in the next elections⁴¹⁴.

Selleri remained a SIF shareholder and, after his comeback from Nebraska, he managed to express again his political views, later signing for instance the foregoing *Statement on Vietnam* (see page 288) against the use of modern technology for war purposes formulated at Varenna during the 1972 Course on “History of the 20th century physics”.

In this 161st meeting there was also an unusual letter by d’Espagnat who asked for “being the last judge of what will contain the accounts of the Course [of Varenna 1970]”⁴¹⁵.

Of course, the SIF Board answered that “it is a practice constantly followed to give full powers to the Director of the Course with regard to the subject to be published on the course reports”⁴¹⁶.

This strange overzealousness in wanting to hold the reins of the proceedings asked by d’Espagnat could be seen as a further confirmation of the increasing weight of political matter in the world of physics as well as in a peculiar role played in this by Selleri. Indeed, according to Olival Freire Jr.:

⁴¹⁴ “Il Presidente porta a conoscenza del Consiglio una lettera di Selleri che fa presente che non potrà partecipare più alle riunioni del Consiglio dovendo trattenersi lungamente in America, come già aveva fatto presente al momento della sua nomina a Consigliere. Selleri comunica altresì che non porrà la sua candidatura alle prossime elezioni” (SIF Bulletin n. 81, 30 December 1970, pag. 2).

⁴¹⁵ “essere l’ultimo giudice di ciò che conterranno i rendiconti del Corso [di Varenna 1970]”.

⁴¹⁶ “è prassi costantemente seguita dare pieni poteri al Direttore del Corso per quanto riguarda la materia da pubblicare sui rendiconti del Corso stesso”. Both excerpts are in SIF Bulletin n. 80, 26 October 1970, pag. 2.

[d’Espagnat] was afraid of the influence of the political context on the editing process. The source of d’Espagnat’s worries, as expressed in his letter to the president of the Italian Physical Society, came from some of Franco Selleri’s statements (Freire 2014, pag. 212. My underline).

In the abovementioned letter, d’Espagnat wrote to Toraldo that “According to him [Selleri], in fact, a trend would have appeared in [the Italian Physical] Society, whose goal would be to intimately associate scientific activities with activities from a different order”⁴¹⁷.

Toraldo di Francia reassured d’Espagnat, still confirming that “the political context had been influential in the choice of the subject of the school, but that its influence should be restricted to only this. The society’s editing processes would not be influenced by it”⁴¹⁸.

--Eventually, the last official citation of Selleri’s work in SIF lied in the excerpt from the minutes of 164th meeting of the SIF Presidency Board, held in 12 December 1970:

[...] The President warmly greets and thanks the outgoing Councilors Alberigi, Bonetti and Selleri, recalling their excellent cooperation within the Board, whose weight and importance does not need any comments, and expressing that there will be no lack of future and frequent opportunities to meet with them⁴¹⁹.

To sum up, I tried to reconstruct the real intense period, albeit brief, spent by Selleri as an advisor to the SIF Presidency Board, highlighting his great activism, his stances and the vital proposals made, including Varenna 1970 on FQM and Varenna 1972 on History of Physics, but also the lawsuit on NATO funds, as well as the remarkable letter on the “Aztec pyramid”, for instance. The various Selleri’s proposals met the agreement of others members and were included in official decisions.

⁴¹⁷ (Freire 2014, pag. 212). In his footnote is also transcribed the original French text by d’Espagnat to Toraldo as well as the specifications of the letter (d’Espagnat to Toraldo di Francia, 3 September 1970, ASIF).

⁴¹⁸ (Freire 2014), pag. 212.

⁴¹⁹ “[...] Il Presidente saluta e ringrazia affettuosamente i Consiglieri uscenti Alberigi, Bonetti e Selleri, ricordando la loro validissima collaborazione in seno al Consiglio, il cui peso ed importanza non ha bisogno di commenti, ed esprimendo che non mancheranno future e frequenti occasioni di incontro con essi” (SIF Bulletin n. 83, 30 June 1971, pag. 1).

While being at the center of the decisions that will broaden the FQM, he still continued in writing on PP, though.

The activity of SIF concerning very precise social and political positions has continued even after the exit of Selleri, thanks to the presence of other councillors certainly politically aligned, among which there was, in my opinion, A. Bettini, as can be seen by several interventions during several meeting of the SIF Presidency Board. It should also be emphasized that the figure of Giuliano Toraldo di Francia, often in agreement and political synergy with Selleri, has contributed to keep SIF “lined up”.

Indeed, Freire had speculated that in SIF, “there was a kind of political agreement on the board led by Giuliano Toraldo di Francia”⁴²⁰. Toraldo “in his later memoirs [...] defined himself as a left-wing man, whole not a communist, very sensitive to the philosophical dimensions of science”⁴²¹. And, in addition, that he had been forced to bring in politically motivated advisers. Freire rightly brings as evidence an excerpt of the interview he made to Toraldo himself on 1 July 2003⁴²²:

I found myself in a way, as we say, between the hammer and anvil, between the two, because on one part I had my professors, my old teachers who were not fascists, not at all, but certainly reactionary, and the young people who were pro-Communists, but too much.

I said no, you are right here and you are wrong here, and I found myself in a very different position. But later it was acknowledged that by taking that standpoint, I saved the Italian Physical Society. Because it certainly risked being split in two at that time.

I said no, we cannot split, we must continue to do very good research [...]; but also take care of our needs of the society [...] (Freire 2014, pag. 204).

Bringing the aforementioned confirmations, I would like to add that, even before the advent of Selleri in the council, the President Toraldo di Francia, as the whole SIF, was always attentive and close to the social and political problems of the time with very thoughtful interventions and proposals,

⁴²⁰ (Freire 2014, pag. 203).

⁴²¹ (Freire 2014, pag. 203).

⁴²² And deposited at the Center for History of Physics-American Institute of Physics.

sometimes even enlightening for future trends⁴²³. I do not know if, even with the foregoing direct testimony by Toraldo to Freire, there was neither this conscious agreement nor the risk of a splitting by SIF. Several statements I cited in this paragraph showed since 1968 a lot of intense debates, the arrival of young scholars clearly left-wing sided for several years, but always with collective decisions, in the Steering Committee as well as at the General Shareholders' Assemblies. Of course, I had the opportunity to see only a lot of Presidency Board meetings, though. But the arrival of the young new left-wing SIF Members in the Steering Committee was always be voted during the Assemblies, clearly revealing a wide commitment by many scholars. Moreover, the same Toraldo clearly stated his thought. I would like to stress also the fact that there are a number of very precise positions in this regard, which are conducive to a close acceptance of the times by Toraldo, even together with the necessary changes linked to political and social upheavals. For instance, I would like to exhibit a precise political SIF motion against the disgraceful, outrageous and totally unacceptable putsch in Chile. The motion was made during the General SIF Members Assembly during the 1973 National Congress to which Selleri attended⁴²⁴, still confirming again the widening of social and political awareness by many scientists and the precise stance of SIF, even with the use of a language similar to many politically aligned associations against social injustice:

The SIF Assembly, in its session of 30 October 1973, expressed its condemnation of the coup provoked in Chile by domestic and international reactionary forces which, with the support of the American monopoly capital, overthrew the legitimate government of Chile, assassinated the democratically elected President Salvador Allende and arrested, tortured and executed thousands of workers and militants of the Chilean left; condemned the actions carried out by the "coupists" against Chilean students, culture and science, with the shooting of hundreds of democratic students and teachers, the burning of books,

⁴²³ Also Del Santo expressed the figure of Toraldo in this way: "[...] President of SIF, Giuliano Toraldo di Francia (1916-2011) was an open minded physicist, sympathetic with left-wing ideas (though more institutional and less radical than Selleri and Cini), and very sensitive to ethical and philosophical problems of science" (Del Santo 2020, pag. 5)

⁴²⁴ With Toraldo as a President, being him a SIF President in two periods, 1968-1970 and 1971-1973.

documents and paintings and the imposition of military personnel at the head of the universities. It invites cultural forces and teachers to debate the meaning of the Chilean events in their headquarters. It invites the Italian democratic forces to find in the Chilean experience an indication for a decisive policy of reform and elimination of social imbalances in order to marginalize and defeat fascism in our country.

It also calls on the Italian government not to recognize the Chilean military junta and to grant political assistance to Chilean resistance fighters. [...] Approved by a majority with 1 against and 10 abstentions⁴²⁵.

Last but not least, I analyzed several SIF National Congresses, specifically giving a look at the different talks proposed within them (in the specific session of general issue of physics) and still exhibited on SIF Bulletin.

I would like to emphasize how the breakthrough event constituted by Varenna School in 1970 and 1972 dramatically changed the arguments at stake in Italian Congresses of one of the main scientific societies in Italy.

For instance, in the LIV National Congress held in Rome on 28 October-3 November 1968⁴²⁶ there is no trace of work on FQM. The general physics session there was Statistical Mechanics, scattering, proteins and so on, with a very applicative cut of the papers. The section of plasma and quantum electronics contained only plasma, actually. For several Congresses, even the one in 1970, soon after Varenna School, hardly any intervention was focused on FQM. It was evident the total lack of papers thereof. It was possible to find some few technical talks on QM, quite often on the statistical approach.

Already in the 1971 SIF Congress, it was possible to read some talks of History of Physics and FQM.

Carlo De Marzo presented “Il moto degli atomi in Democrito ed Epicuro” and Selleri sent from Nebraska an abstract (which unfortunately arrived late) on “La contraddizione materialismo-idealismo nella meccanica quantistica”.

The best confirmation of this turnaround came from the 1973 Congress, though, where many talks were centered on HoP and FQM, with a glance on

⁴²⁵ The Italian original text is in Addendum 3, p. 985.

⁴²⁶ See SIF Bulletin n. 62, 21 October 1968.

Bell's inequalities and hidden variables: "I problemi della meccanica quantistica" by Garuccio and Selleri; "Considerazioni sull'interferenza di fotoni singoli" by De Marzo; "Statistica delle misure per miscele 'proprie' e 'improprie'", as well as "Osservazioni sul problema della variabili nascoste in meccanica quantistica", both by Baracca and Bergia; "Conseguenze dell'invarianza per rotazioni in una teoria a variabili nascoste locali" by Corleo, Gutkowski, Masotto, Notarrigo and Valdes; "Correlazioni angolari di fotoni di annichilazione per verificare la possibilità di variabili nascoste locali" by Faraci, Gutkowski, Notarrigo and Pennisi; "Attualità della storia della scienza" by A. Rossi; and so forth, including several papers by E. Recami and R. Mignani on the Foundations of Relativity Theory. Several of these scholars will work together with Selleri and Garuccio in the 1980s on empty waves and Bell's inequalities.

2.6-VARENNA 1970, I.E., THE RECOGNITION AND THE RELENTLESS WIDENING OF FQM

In this section, I have taken into account the widespread narrative relating to the subject. I keep on quoting (Freire 2014) and (Baracca, Bergia, Del Santo 2017), because they are among the few who have devoted entire sections and papers to FQM in the period between 1950 and 1990, a historical phase that only very recently begins to be investigated in the HoS. Moreover, my research in the SIF archives has in fact reiterated what they have already expressed, even with some additional data and some odd results to be debated. Therefore, I will not dwell too much on Varenna 1970, but I will not fail to emphasize its epochal role, a real turning point for the study and the widening of FQM. In the 153rd meeting of the SIF Steering Committee, held in 10 January 1970, Selleri suggested that a school in Varenna should be held in HoP, which then occurred in 1972. In the SIF Archives I found two folders linked to the two Varenna Schools, in 1970 and 1972. But Selleri did not take part in the latter, which is why it will not be analyzed in this work. On the other hand, there is comprehensive information about Varenna 1972 in the sources mentioned above.

So, I analyzed the ASIF folder specifically devoted to Varenna 1970: “Cartella archivio anno 1970-corsi di varenna I-II-III”⁴²⁷.

Concerning our topic, I mentioned how in the 142nd meeting of the SIF Steering Committee, held in 15 March 1969, Selleri proposed the implementation of the 1970 Varenna School in FQM. He was the secretary of the School and he also deeply took care of the organizational part, as we can deduce from several letters. He always had a substantive correspondence both with prof. G. Germanà (SIF General Secretary) and Bernard d’Espagnat (School Director) about some decision in the organization of the School.

Some correspondence showed the origin of the assignment of the school management to d’Espagnat as well as the secretariat to Selleri and his role joint with Germanà and d’Espagnat himself. Here below are shown five letters, which revealed some interesting information. In the first letter, Toraldo, introducing the International Physics School “E. Fermi”, held in Varenna every summer, informed Bernard d’Espagnat that SIF choose him as the director, also recalling the tasks:

For the summer of 1970 we are planning, among other things, a course on the Foundations of Quantum Mechanics. We should deal with the questions you set out in your book, also taking into account the contributions of Daneri, Prosperi and Loinger, Wigner, Bohm, de Broglie.

On behalf of the Presidency Council of the Società Italiana di Fisica, I have the honour to invite you to lead this course. The task of the director is not very heavy. He has to choose the lecturers and the topics. He is assisted by a secretary. As secretary we propose: prof. F. Selleri [...]⁴²⁸ (My underline).

d’Espagnat answered in a letter dated 2 May 1969, emphasizing his pleasure in accepting the direction of the school as well as his delighted in having

⁴²⁷ Archive folder year 1970-Varenna courses I-II-III, further referred to as Archive folder 1970 Varenna.

⁴²⁸ Pour l’été 1970 nous envisageons entre autre, un cours sur les Fondaments de la Mécanique Quantique. On devrait traiter les questions que vous exposez dans votre livre, en tenet compte aussi des contributions de Daneri, Prosperi et Loinger, de Wigner, de Bohm, de de Broglie.

Au nom du Conseil de Présidence de la Società Italiana di Fisica, j’ai l’honneur de vous inviter à diriger ce cours. La tâche du directeur n’est pas très lourde. Il doit choisir les conférenciers et les sujets. Il est aide par un segretario. Comme secrétaire nous proposons: prof. F. Selleri [...]. (Toraldo di Francia to d’Espagnat, 23 April 1969-ASIF-Archive folder 1970 Varenna)

Selleri as a secretary, considering him as a well-known physicist yet, in the field of PP, of course, because at the date of the letter no papers on FQM were published by Selleri himself:

[...] It is with great pleasure that I accept your kind invitation to lead a course at the Enrico Fermi International School in 1970 on the Foundations of Quantum Mechanics.

[...] I will naturally be very happy to see entrusted to a scientific secretary who is a well-known physicist - even if he is of a different specialization - all the often very delicate responsibilities of the organization [...]. If our colleague Professor Selleri is willing to take on this task, as you suggested, I would be particularly delighted⁴²⁹ (My underline).

The above letter sounded also really significant for the inclusion of a proposal with no further sequel. In it, d’Espagnat started proposing some scholars for the School, introduced the name of Hugh Everett III, also as the first one of his personal list:

[...] if I interpret your thinking correctly, this course should essentially consist of several seminars of a few sessions each, in which the conceptions of, in particular, Loinger et al., Bohm, L. de Broglie, Wigner would be exposed (and preferably by their promoters).

But I can see that in your mind this list is not limitative, so I will take the opportunity to add Everett III, Shimony, Jauch, Bell, Rosenfeld and others. I think it is very important that the trends they highlight be adequately represented as well⁴³⁰ (My underline).

But, unlikely the other scholars proposed, which attended Varenna School (excepting Rosenfeld who decided to refuse the invitation), in the following

⁴²⁹ [...] C’est avec un très grand plaisir que j’accepte votre aimable invitation à diriger en 1970 un cours de l’Ecole Internationale Enrico Fermi consacré aux Fondements de la Mécanique Quantique.

[...] je serai naturellement très heureux de voir confiées à un secrétaire scientifique qui soit un physicien bien connu - même s’il est de spécialisation différente - toutes les responsabilités souvent très délicates de l’organisation [...]. Si notre collègue le Professeur Selleri veut bien, conformément à votre suggestion, se charger de cette tâche j’en serai particulièrement enchanté (d’Espagnat to Toraldo di Francia, 2 May 1969-ASIF-Archive folder 1970 Varenna. My underline).

⁴³⁰ [...] si j’interprète bien votre pensée, ce cours devrait être essentiellement composé de plusieurs séminaires de quelques séances chacun, dans lesquels les conceptions de, en particulier, Loinger et al., Bohm, L. de Broglie, Wigner seraient exposées (et de préférence par leurs promoteurs). Mais je vois bien que dans votre esprit cette liste n’est pas limitative, j’en profiterai donc pour ajouter Everett III, Shimony, Jauch, Bell, Rosenfeld et d’autres. Il me paraît en effet très important que les tendances qu’ils mettent en avant soient adéquatement représentées elles aussi (d’Espagnat to Toraldo di Francia, 2 May 1969-ASIF-Archive folder 1970 Varenna. My underline).

there will no longer be any trace of Everett, being instead quite immediately chosen the name of Bryce DeWitt for the Everett-Wheeler theory⁴³¹.

In Freire you can read in detail⁴³² Everett's story, what were the reactions to his work, and how he gave up his work as early as 1956, still refusing to speak about it anymore.

DeWitt's intervention at Varenna, titled "The many-universes interpretation of quantum mechanics" was in practice one of his Everett's endorsements, whose ideas were struck by DeWitt himself since 1957. With a publication in *Physics Today*, always in 1970, DeWitt allowed a return of interest in this interpretation of QM that today is among the most followed.

In another letter that I found at ASIF, sent by Germanà to d'Espagnat, there was the confirmation of Selleri as a secretary:

[...] Professor Selleri will be very honored to be your secretary in this course (Germanà to d'Espagnat, 11 June 1969-ASIF-Archive folder 1970 Varenna).

and also an interesting statement, concerning the desire to have a decision-making role on the part of NATO in the approach of the school. I mentioned in the previous paragraph of this chapter how the matter of NATO funding and instructions on Varenna School was widely debated and how SIF Presidency Board finally decided to stop it.

At the time of this letter, the matter was opened, though, being decided only after SIF National Congress in Bari on October 1969.

[...] Please also take note that the NATO Advisory Committee recommended a selection of lecturers as much international as possible (Germanà to d'Espagnat, 11 June 1969-ASIF-Archive folder 1970 Varenna).

After one week, came the answer by d'Espagnat, in a letter full of interesting hints. He proposed the first tentative list of lecturers, he warned of his need to

⁴³¹ It sounded strange talking about the Everett-Wheeler theory in the minutes of Varenna School organization, due to the fact that Wheeler position was always unclear to the same Everett (see Freire 2014, pagg. 131-132).

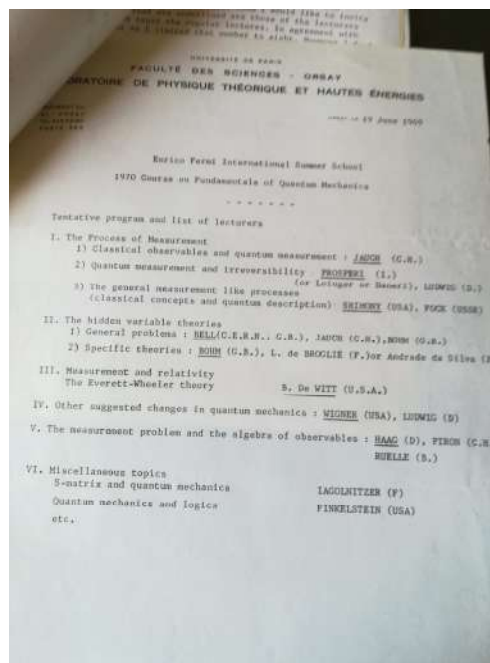
⁴³² In (Freire 2014), Chapter 3, pp. 75-134.

confront others about his choice. And, above all, he emphasized how several fields showed conflicting opinions, the importance of sharing discussions with many people, and the uniqueness of the listed speakers in some fields, which made them hard to replace.

It was a great survey of the situation in FQM before the School, and, by the way, an hint to compare how the situation changed after the School:

[...] I send you herewith a list of the topics and the names of the possible lecturers [...] whom I would like to invite to the School. The names that are underlined are those of the lecturers to whom I think we might trust the regular lectures. [...] I limited that number to eight. However I feel that in that particular field (in which many conflicting opinions are held) we should not make much difference between courses and seminars. I also feel that the success of the course will depend greatly on the opportunities for expression and discussions that it will give to many people. I think therefore that we should try as much as possible to invite, as lecturers for seminars, representatives of all the (consistent) standpoints presently in existence. The necessity of answering you as quickly as possible has prevented me from discussing as yet this program with anybody. It is therefore quite tentative. Its main lines, however, are, I hope, rather firm. [...] Many of the speakers in the list are somewhat alone in the field. I want them to lecture about so that they are not easy to replace. For that reason, the sooner we contact them will be the better (d'Espagnat to Germanà, 19 June 1969-ASIF-Archive folder 1970 Varenna. My underline).

Then he attached the following schedule with the tentative program and list of lecturers. Not all the scholars managed to come to Varenna, though. The following list sounded impressive for the recognisement of the main physicists involved in FQM at that moment:



(d’Espagnat to Germanà, 19 June 1969- Courtesy ASIF-Archive folder 1970 Varenna.

Author’s photo).

It is worth pointing out how Selleri agreed with d’Espagnat’s choices. Always taking care to the funds and the budget decided for the School, he played a key role also in permitting the invitation of more than expected scholars from US, for instance, thus contributing to a better outcome of the School:

Professor Selleri and I have agreed on the trial program and the list of speakers⁴³³
(Germanà to d’Espagnat, 23 July 1969-ASIF-Archive folder 1970 Varenna).

Focusing on Varenna 1970 Ist Course School on “Foundations of Quantum Mechanics” (sometimes found in some 1969 letters as “Fundamentals of Quantum Mechanics”), a lot of documents referred to school organization were present in the (Archive folder 1970 Varenna): tentative programmes of the School, the minutes on the accommodation, diverse phases related to the organization, the lists of registered and the lecturers and so on. Particularly interesting, also for the requests of individual scholars, are the organizational

⁴³³ Le professeur Selleri et moi nous sommes d’accord pour le programme d’essai et la liste des conférenciers (Germanà to d’Espagnat, 23 July 1969-ASIF-Archive folder 1970 Varenna).

correspondences between the General Secretary Germanà and the various lecturers participating in the School like John S. Bell, David Bohm, Eugene Wigner and so forth.

In addition, the choices and the titles for the dissertation to be proposed in the School by the different lecturers changed in time as well as the fact that some lecturers made more than one talk, although not all of them appeared in the Proceedings of the School. This is the case of the H.D. Zeh reprint. In Zeh's letter to Germanà he stated:

Let me repeat that my second contribution (On the Interpretation of Measurement in Quantum Theory) was a reprint and not intended for publication in the proceedings (Zeh to Germanà, 23 July 1970-ASIF-Archive folder 1970 Varenna)

Another example are the three lectures proposed by Wigner in Varenna, maybe all gathered in his unique intervention on the Proceedings. In a letter to Germanà written some days before the beginning of the School he stated:

[...] I have written up in considerable detail the subject of my first lecture. [...] As for the two other lectures, I thought that their bases were given quite well in an address which I gave in 1968 in Trieste. These were published by the IAEA, and I am enclosing a copy for whatever use you wish to make thereof (Wigner to Germanà, 22 June 1970-ASIF-Archive folder 1970 Varenna).

Eugene Wigner⁴³⁴ played a very remarkable role in Varenna 1970 School. Firstly, d'Espagnat asked him several suggestions and still invited scholars which Wigner advised, as Michael Yanase, for instance. "His name was the first that Wigner put forward when I asked him for suggestions" as d'Espagnat stated to Selleri in a letter to ask for some *per diem* economical support in favour of Yanase himself ⁴³⁵. Or as in the case of D.H. Zeh: "The presence of Dr. Zeh is considered as highly advisable by Prof. Wigner and I therefore invited him".

⁴³⁴ Eugene Paul Wigner (Jenő Pál Wigner 1902-1995): <https://www.britannica.com/biography/Eugene-Wigner>; <https://www.nobelprize.org/prizes/physics/1963/wigner/biographical/>. Both accessed 24 September 2020.

⁴³⁵ d'Espagnat to Selleri, 20 February 1970-ASIF-Archive folder 1970 Varenna.

Secondly, Wigner discreetly but firmly and wisely suggested to Germanà the best way to carry out the Varenna lectures:

I just received a letter from Professor Franco Selleri, suggesting that I prepare lecture notes for the course [...] in Varenna. [...] It is, on the whole, not my impression that it is good to distribute lecture notes ahead of the lecture. It is only natural that students pay less attention to the lecturer if they have his notes in hand. As a result, their participation is less intense and they are less likely to ask for clarification of the points which may result unclear. The purpose of the lectures is, on the other hand just to invite this kind of participation. It may be best, therefore, if we distribute the notes just after the lectures. [...] I do not want to alter plans if these are the accepted custom of the Varenna lectures, but I did want to tell you my views (Wigner to Germanà, 22 June 1970-ASIF-Archive folder 1970 Varenna).

Thirdly, Wigner was the indirect reason for the absence of Rosenfeld in Varenna. According to (Freire 2014, pag. 160) Rosenfeld refusal, “reflected the decline of his influence among the physics milieu interested in foundations of quantum physics”.

In the early 1960s, Rosenfeld and Wigner experienced a strong quarrel about the measurement process. Freire described this event very deeply (Freire 2014, Chapter 4, pagg. 141-170).

Indeed, it sounded enormously interesting the short correspondence between Bryce DeWitt and Bernard d’Espagnat that I found in the ASIF folder and regarding Rosenfeld’s absence, communicated to d’Espagnat on 8 January 1970 (as I could read in a d’Espagnat’s letter to Germanà dated 20 January 1970), and the Kalckar participation.

On 27 March 1970, from The University of North Carolina at Chapel Hill, DeWitt wrote to d’Espagnat in order to the organization for his travel to Varenna school and to other organizing stuff. But, the most impressive statement occurred at the beginning of the letter:

[...] I was sorry to see Rosenfeld’s name suddenly missing from the printed announcements. Was he scared off by the presence of Bohm and Wigner? (DeWitt to d’Espagnat, 27 March 1970-ASIF-Archive folder 1970 Varenna-My underline).

d’Espagnat answered to DeWitt on 10 April 1970, still confirming the hypothetical question posed by DeWitt himself. Indeed, he stated:

[...] You were right about Rosenfeld. However Kalckar is a sort of replacing him as a representative of orthodoxy. Also Heisenberg might possibly pop in (d’Espagnat to DeWitt, 10 April 1970-ASIF-Archive folder 1970 Varenna).

Freire pointed out how Rosenfeld had accepted at the beginning to come to Varenna:

As remarked by the historian Anja Jacobsen, Rosenfeld’s biographer, Rosenfeld was invited to speak about the measurability of quantum fields and accepted. However, after reading the invitation letter and the list of speakers, he withdrew and sent Jørgen Kalckar. [...] Rosenfeld’s retraction reflected the ongoing battle between Wigner, Bohm, and Jauch, on the one hand, and himself, on the other hand. This was expressed in his refusal letter with the following words: “Wigner’s talk [in a recent event in Trieste] convinced me that no dialogue is possible and, furthermore, that he is not looking for it. As for Bohm, I just say that I am tired with his last words; and you know what I think of Jauch’s axiomatic prestidigitations” (Freire 2014, pag. 159. My underline).

In a letter to Germanà, d’Espagnat asked for the opportunity to send a letter to the participants before the School. d’Espagnat proposed two versions to be published, the first to be reprinted for corrections, the second as it was, without corrections. The most important thing was to try publishing it, as then happened:

Dear Prof. Germanà

Several of the lecturers have encouraged me to send this letter in advance to the participants. Would it be possible for you either (preferably) to have it typed again with the corrections, and then reproduced and sent or else simply reproduced and sent, without corrections? (d’Espagnat to Germanà, 18 June 1970-ASIF-Archive folder 1970 Varenna).

A possible reason for this letter lied in the will to live a School full of debates and really opened to all views, without any try to convince people that think different. It was a recommendation letter written in brand new times, scientifically and politically speaking.

Maybe d’Espagnat and others felt the new directions in which physics was heading. According to Freire: “d’Espagnat suggested some diplomatic rules to be followed in the invitation letter so as to guarantee a peaceful and creative atmosphere in which to discuss scientific controversies”⁴³⁶.

In the ASIF I found both versions, but these were quite different by the official one. Maybe, because of time constraints, the official version did not present the corrections asked by d’Espagnat, although they were very minimal. I presented here the official version followed by the corrections d’Espagnat asked:

Like the Delphic oracle theoretical physics rests on three legs: experience, mathematics and a workable set of general ideas.

Some would like to cut this third leg away. They hope to increase thereby the stability of the whole construct but we, who will gather in Varenna, know that of course they are quite wrong! We are all convinced that a mere collection of wholly or partially successful recipes - be they even beautifully formal - cannot be a substitute for a genuine understanding.

Unfortunately (or in view of the necessary diversity of human conceptions should we perhaps say “happily?”) the standpoints of the physicists greatly differ from one another on the question of what these workable ideas should be. In our daily professional life this truth is hushed down without damage since we all practically agree on how to apply the quantum recipes. But it will probably become quite clear, in a few days, that under a superficial agreement on how to use the rules we have learnt, we entertain real differences of opinion as to what these rules refer to. Rather hectic discussions can thus be hoped to develop; especially since, purposely, lecturers are invited who represent quite a broad spectrum of ideas. This is an excellent thing. In order that we should take full advantage of it, let me suggest to you the following agreement: that we should not take as our goal the conversion of the heretic but rather a better understanding of his standpoint; that we should not suggest that we consider as a stupid fool anybody in the audience (lest the stupid fools should in the end appear clearly to be ourselves!); that we should try to cling to facts; and that nevertheless we should be prepared to hear without indignation very nonconformist views which have no immediate bearing on facts.

⁴³⁶ Freire 2014, pag. 159.

We shall also enjoy some nice pieces of mathematical formalism. Not many of them, since the danger is that when a man has understood a bit of nontrivial mathematics applied to physics he is so happy with himself that he may forget to try to make clear in his mind what conceptual ideas are underlying the whole thing. However, on mathematics, it is usually possible to agree. These mathematical developments were carefully selected for their relevance to the problems at hand. They will also serve as the pillow on which we can reconcile with one another after having quarrelled on allegedly “trivial” points.

It is not usual that such matters should be discussed at length in courses or in seminars. *The large number of applications that came in has convinced me that the members of the Italian Physical Society who were at the origin of the course had a very good idea after all* (d’Espagnat 1971, XIII-XIV. My underline).

The corrections, written below, are all in the last statement, reported above in *Italics*:

The large number of applications that came in is a convincing proof that the (Italian) originators of the course had a very good idea after all. It also renders even greater the pleasure with which we all look forward to meeting you in Varenna (ASIF-Archive folder 1970 Varenna).

The invitation letter sent by d’Espagnat was a real stroke of genius, a brainwave. He managed to set out some rules to “train scientists on issues where no consensus exists”, dealing with this “by setting the standards of behavior scientists should adhere to in case of controversy”⁴³⁷.

Selleri remained surely greatly impressed by it.

In a letter found in the FSPA, the Organizing Committee of an international conference⁴³⁸ sent to participants a similar statement of purpose. The style recalls Selleri's way of writing, as well as the concepts present inside fit with his thoughts. This makes me lean towards some international conferences in which Selleri was one of the the main organizers, i.e., Perugia 1982 (The wave particle dualism), Bari 1983 (Open questions in quantum physics), Urbino 1985 (Microphysical reality and quantum formalism), Cesena 1991

⁴³⁷ Both sentences are in (Freire 2014), pag 208.

⁴³⁸ In FSPA the reference is not clear at all. The letter is included in a list of conferences without a link to it. I could not manage to find the correspondence between the letter and the conference.

(Bell's theorem and the foundations of modern physics), Trani 1992 (Waves and particles in light and matter), Olympia 1993 (Frontiers of fundamental physics), Athens 1997 (Relativistic physics and some of its applications). But, in the absence of any confirmation and evidence, it is just my assumption, naturally. Anyway, the text is reproduced below:

A CALM COMPARISON OF SCIENTIFIC IDEAS

This conference is intended to be for open-minded friends of Albert Einstein. The members of the Organizing Committee all believe that he was one of the greatest geniuses of all times. Of course this is not the same as saying that whatever he has achieved should be considered as a final truth. Final truth probably does not exist, and even if it did, it would be very unlikely that it had already been obtained in our early scientific times. It is for these reasons that we suggest people having unorthodox views in relativistic physics to participate in the conference only if:

1. They can control their feelings concerning Albert Einstein;
2. They are willing to admit that their own achievements should be seriously and honestly compared with those of other researchers, Einstein in primis.

In this spirit of calm and rational comparison of scientific ideas we hope to have a stimulating conference. The Organizing Committee
(Document found in FSPA).

2.6.1-STARTING THE SCHOOL

In a letter dated 25 June 1970 to Bernard d’Espagnat, Toraldo di Francia apologized for being absent at the opening of the Varenna Summer School, also stating: “Franco Selleri, who is a member of the Board of the *Società Italiana di Fisica* will represent me on that occasion”⁴³⁹ (My *Italics*).

The IL Varenna Course, Foundations of Quantum Mechanics, took place in Varenna on Lake Como from 29 June to 11 July 1970, within the international school of Physics “E. Fermi.

The school was divided into three thematic sessions: Measurement and Basic Concepts, Hidden Variables and Nonlocality, Interpretation and Proposals.

The school was attended by many young scientists and a lot of notable physicists and epistemologist took part to it.

⁴³⁹ Toraldo di Francia to d’Espagnat, 25 June 1970. ASIF-Archive folder year 1970-Varenna courses I-II-III.

There were 84 participants, including observers and students. The official lecturers were the following: J. Andrade e Silva, J. S. Bell, B. D'Espagnat, B.S. De Witt, J. Ehlers, A. Frenkel, K. E. Hellwig, F. Herbut, M. Jauch, J. Kalckar, L. Kasday, G. Ludwig, H. Neumann, G.M. Prosperi, C. Piron, F. Selleri, A. Shimony, H. Stein, M. Vigičić, E. Wigner, M. Yanase, H.D. Zeh. Some of them made more than one talk (like A. Shimony, who played three talks).

Quite curious sounded the matter about the presence of Louis de Broglie and David Bohm.

Olival Freire Jr wrote in his (Freire 2014, pag. 206) that “the 1970 Varenna Summer school was successful due to [...] the diversity of its speakers, including [...] David Bohm, [...] Louis de Broglie [...]”. So, for the Brazilian historian they both attended the School.

On the contrary, according to (Baracca, Bergia, Del Santo 2017), neither de Broglie nor Bohm were at Varenna, although they were present on the Proceedings. Indeed, after naming the lecturers at Varenna, without the two famous scholars, they stated:

[...] However the proceedings contained also chapters from Louis de Broglie and David Bohm (Baracca, Bergia, Del Santo 2017, pag. 70).

I totally agree with Baracca, Bergia, and Del Santo about de Broglie, being aware that Andrade e Silva were at Varenna on behalf on de Broglie himself. And, in addition, I did not found any correspondence by the French physicist inside the ASIF folder destined for Varenna 1970.

Furthermore, I found a letter sent by de Broglie to Selleri in which the former (surely ignoring that Selleri was the secretary for the School, apart from being the inspiring scholar) confirmed that Andrade e Silva would be present at Varenna on behalf de Broglie himself and his group:

[...] I would like to inform you that at the end of next June a Summer School will take place in the Italian town of Varenna (which is, I believe, on Lake Como) where my main

collaborator, M. Andrade e Silva, will give lectures on the double solution and on the work we are carrying out⁴⁴⁰ (My underline).

Nevertheless, I can admit that, concerning David Bohm's presence, the story sounded different, though, and I agree with Freire. It could have happened that Bohm renounced last minute. For instance, there were some telegrams showing how Eriksson confirmed his stay at Varenna on 29 May 1970, and then cancelled it on 16 June 1970. But I do not think of any renunciation concerning Bohm, because his presence was still confirmed by the following letter sent by Germanà on 2 July 1970, during the School itself to the Hotel Royal Victoria in Varenna:

We hereby provide you with a list of the persons staying at your hotel, which are to be considered our responsibility:

Teachers:

[...] Prof. Bell + wife

[...] Prof. Bohm⁴⁴¹.

This is only the fifth letter in the correspondence between Germanà, Bohm, and J. Hood (Bohm's secretary at Birbeck College). The letters went from 16 April 1970 to 29 May 1970. Bohm and then Hood confirmed the Bohm's presence at Varenna until the last letter, as well as the presence of Hiley (Bohm's colleague) as Observer. In conclusion, despite the Bohm's non-participation in the group photo I am drawn to the conclusion that Bohm was at Varenna School.

Unfortunately, during the School, a sad and horrible event happened. The English physicist Victor Rothman disappeared and never found, notwithstanding a long and vain search.

In the 160th meeting of the steering Committee, of 18 July 1970, is stated:

⁴⁴⁰ [...] je veux vous signaler qu'à la fin du mois de Juin prochaine, aura lieu dans la ville italienne de Varenna (qui est, je crois, sur le lac de Como) un Ecole d'été où mon principal collaborateur, M, Andrade e Silva fera des exposées sur la double solution et sur les travaux que nous poursuivons (de Broglie to Selleri, 22 March 1970-FSPA).

⁴⁴¹ Con la presente vi comunichiamo l'elenco delle persona alloggiate presso il Vostro Albergo, che devono essere considerate a nostro carico. Professori:

[...] Prof. Bell + moglie

[...] Prof. Bohm⁴⁴¹ (Germanà to Hotel Royal Victoria, 2 July 1970-ASIF)

The President reported on the serious incident at Varenna Summer School involving the disappearance of young English physicist Dr. V. Rothman. The feverish researches of any nature, undertaken to date, have unfortunately not led to any results.

The staff of the Varenna School did their best to do what was humanly possible, but unfortunately the searches carried out by police, carabinieri, mountain guides and divers have been so far in vain. The Council expresses its deepest regret at this tragic incident and expresses its deepest condolences to the family of the deceased⁴⁴².

In a handwritten letter of 18 August 1970 to Germanà, Rothman's brother, on behalf his family, wanted to:

[...] thank you for the efforts which you made to find my brother Victor. We will always be grateful to you for that, and for your generosity towards me when I came out to Italy. It is most unfortunate, and heart-breaking to us that all these efforts should have been of no avail. We still have no news of my brother or even one clue as to where he went to. But we have not given up hope.

2.6.2-POLITICAL STANDPOINT AND STRESS SOURCES

Varenna 1970 summer school was also a place full of intense political debates and several prospective unrest. A finely-tuned balance that has been able to turn the tensions in a constructive sense. Although there was a hot, turbulent political climate, the workings of the school went on well.

As I stated in the previous paragraph, during Varenna 1970 appeared a typewritten document, titled *Notes on the connections between science and society*, centered on the political and social role of science within society, on his non-neutrality as well as the social responsibility of scientists in capitalistic society. It was probably authored by diverse Italian scholars, just seeing the diverse writing techniques. One of the most compelling consequence I can see by a full reading of this document is the constant reference to the ideas contained in Selleri's paper *La piramide azteca della*

⁴⁴² Il Presidente dà notizia del grave incidente verificatosi durante la Scuola Estiva di Varenna e relativo alla scomparsa del giovane fisico inglese dr. V. Rothman. Le febbrili ricerche di qualsiasi natura, intraprese fino ad oggi, purtroppo non hanno portato ad alcun risultato. Il personale della Scuola di Varenna si è prodigato facendo quanto era umanamente possibile fare, ma purtroppo le ricerche eseguite da polizia, carabinieri, guide alpine e sommozzatori sono state sin qui vane. Il Consiglio esprime il suo più vivo rammarico per questo incidente che ormai ha assunto carattere tragico, ed esprime vive condoglianze alla famiglia dello scomparso.

fisica teorica delle particelle elementari, appeared in March 1970, and to the several hints it includes. I introduced here some excerpts, while the whole document, as found in FSPA, is in Addendum 3, pp. 985-992:

NOTES ON THE CONNECTIONS BETWEEN SCIENCE AND SOCIETY

By a group of participants to the 1st course (on FQM) of the International School of Physics “E. FERMI”.

A group of participants in the school has gathered together several times to discuss problems which confront us as researchers living and working in the present society.

[...] In and out the conference room, problems of philosophical character have been touched upon, but too often in a superficial way. In several occasions it was evident how philosophy, logic, historical knowledge are scarce in the culture of the average physicist. It looks like his professional training has been done at the expenses of a broader cultural basis, one capable of putting him in vital connection with social life.

[...] It was our intention, then, to present these points to the other participants so as to arise their interest and to promote a discussion that could have helped in exchanging ideas and experience and, may be, in reaching some common conclusion. Unfortunately lack of time and a number of adverse circumstances have made this impossible.

[...] GENERAL CONSIDERATIONS ON THE SOCIAL RESPONSIBILITY OF THE SCIENTIST

We recognize first that an important social function of science is to organize consensus and to maintain social stratification. The myth of the neutrality of science, reinforced by the common concept of the intrinsic positive value of all technological progress, is used as a factor of social cohesion to evade confronting the real problems which lead instead to class struggles. [...] It is instead extremely important to realize that science is certainly not neutral, and that the same holds moreover for the very structure of scientific thought. One may recognize the fact that the structure of a scientific theory reproduces the categories of the culture of the dominant classes. The contradictory aspects of this culture find an explanation in the context of the dialectic of class struggle in the widest sense. Modern theoretical physics may be a specific example just for the deductive nature of research, the very structure of the theory (the axiomatic setting of Quantum Mechanics), the functioning of this setting in order to generate a phenomenological research.

This subatomic phenomenology is connected, for instance, with the introduction of complex technologies of economic relevance, with the hierarchic pyramidal structure existing in the research world, with the subjection to chiefs.

[...] The scientific researchers are automatically inserted in a mechanism mainly characterized by competition, individualism and the uncritical acceptance of the social structure.

The last aspect is itself a specific consequence of the extreme competitiveness which completely absorbs the energies of the researchers and leaves an extreme poverty of cultural and political instruments. We have experienced this fact even in the discussions of the School of Physics “E. Fermi” on the Foundations of Quantum Mechanics: the poverty of the philosophical background pushes people toward an attempt to separate Physics from Philosophy with an evidently arbitrary and artificial procedure.

It is consequently necessary to investigate the social role of the scientist, with respects not only to his specific research activity but also with respect to his social position and his relationship to social stratification.

[...] PRE-DECISION ON THE SOCIAL ROLE OF THE SCIENTIST

In this school we have tried to analyse the possible attitudes of scientist with respect to the object of his research, that is, his philosophical position concerning nature.

It seems to us that this analysis has been unsatisfactory.

[...] We have considered to some extent the role of our consciousness in photon polarization experiments, but we have not considered our conscious involvement in phenomena such as the explosion of the atomic bomb.

The limitation of the individual consciousness to laboratory activity, disregarding any judgement of the social application of the research, results in indiscriminate support for all applications of science (e.g., atomic bomb, chemical and bacteriological warfare).

The scientist’s incapacity to control the product of his research facilitates its cultural manipulation and the creation of consensus.

This mystification is formalized in the powerful theory (“scientism”) which assumes the intrinsic ability of science to solve all the human problems.

[...] Whatever the role of consciousness in the measuring process, it seems to us necessary to be aware that no human activity is neutral in the social context and that this is true also for science. As a consequence the scientist should assume all the responsibilities of his behaviour.

[...] Now we conclude that a pre-decision is strongly needed concerning the social structure in which men live and act. That is, a pre-decision on the historical and social role of the scientist, on his responsibility, on the fact that no concept or activity is neutral.

[...] Whether a theory can or cannot make predictions, we stress that our activity as scientists is not at all neutral and that our fundamental task as men is to act upon the social reality and to change it.

ON THE CONNECTION BETWEEN SCIENCE AND THE PRODUCTIVE SYSTEM

[...] We perceive an always deeper division of the scientific work in specialistic fields and real difficulties to put in discussion the foundations and purpose of science. Actually in the scientific community it is very difficult to find the right atmosphere to discuss foundations and purpose of science. This is shown by the relatively very poor scientific production in such field. Obviously we consider this school an exception.

We see a huge production of papers in each specialistic sector of the science.

This production deals with the subject in fashion and mostly fashions change quite quickly in time. So we perceive not only a deep division of the scientific culture from the body of the humanistic culture but also we find our scientific culture deeply divided and fragmented in itself. As a matter of fact such division of culture in separate parts avoids the formation of a really strong critical movement able to reach a new cultural synthesis. The possibility for scientists to change the situation are very few. In principle scientists have complete freedom to decide the kind of research they want to do; in practice they are inserted in a scientific organization which reproduces the structure of the society to which they belong.

[...] careers of researchers depend in a substantial way by the scientific opinions held by the people directing the scientific community.

The above document was mainly produced by Italian students, perhaps those more politicized. According to (Baracca, Bergia, Del Santo 2017):

They gathered every evening and developed deep and animated critical discussions using as starting points the themes presented in the lectures, such as the contradictions and paradoxes of QM, generalizing their perspectives to include the basic concepts of science and knowledge, their nature and function, their evolution and their meaning, and the social responsibility of scientists in capitalistic society. From these discussions they produced a collective 12-page document, that was typewritten, duplicated, and distributed on one of the last mornings at the entrance of the lecture room to all the teachers and participants: *Notes on the connection between science and society* [...].

The aim of the document was to raise awareness among the physicists towards the limits of the restricted conception of pure science (separation between physics, philosophy and society, and the presumed objectivity of science, as an absolute and ahistoric concept) and to point out the strong relation, too often omitted, between science and politics, economy and society (non-neutrality of science, criticism to “scientism” and submission of scientists to the needs of the leading class) (Baracca, Bergia, Del Santo 2017, pag. 70. *Italics in the original text*).

Another confirmation of the conflicts present in the School as well as the key role played by Wigner lied in the incident on 4 July, when Wigner would organize a party for the US Independence Day. According to Freire, who cited several recollections he received by letters (and that I reported in *Italics*):

Indeed, the political episode most recalled is the incident related to July 4th involving Wigner. The incident may be described using various testimonies [...]. Hiley, when asked about political incidents in the school, recalls:

Yes, there was one classic political episode that took place. Wigner decided to throw a party and he happened to choose the 4th July. Remember that was the time of Vietnam and the Italian students, mainly Italian, tried to get the date changed. They were not going to take part in a party on 4th July. Of course Wigner refused to change the day. There was a lot of arguments and protests, a lot of hot air over this, but the party eventually went ahead on the 4th July as planned. It went off without incident.

Another testimony, from the Spaniard Emilio Santos, while essentially similar to Hiley's, suggests a generational divide between those who protested and those who went to the party offered by Wigner:

I remember very well that Eugene Wigner invited people to a party on the occasion of the National Day of USA, July 4, and there were some (mainly young) people who attempted to convince other people not to attend the party because the USA was involved in the Vietnam war at that time. Actually most senior people (myself in particular) attended the party. I remember that Wigner was very kind with the same young people who boycotted the party and there is even a photo of Wigner with them.

The Hungarian physicist Andor Frenkel recalls that the hot political climate was not only related to the date and the Vietnam War, but also to the fact that Wigner's support for the war was well known: "Professor Wigner was a supporter of the war against Vietnam, and many participants of the School wanted the US out of Vietnam." Recollections from Giovanni Prosperi describe some of those who opposed Wigner's initiative more vividly: "On Sunday there was no lesson and as it was USA independence day, Wigner wanted to give participants a party. It was the time of the Vietnam War and Selleri, who was quite left wing, was rather disappointed and tried to prevent it calling Toraldo di Francia in Bologna. However, Wigner was very determined and the party took place."

The party organized by Wigner was the political event most recalled among the attendees, highlighting Wigner as the main conservative pole, both for his professional prominence and his support for the Vietnam War.

However, there was a kind of peaceful coexistence and competition, to use the political jargon, between the political conflicts and Wigner's role as a supporter of the debates around the foundations of quantum physics. Tonietti's reminiscences are evidence of such a coexistence:

The intermingling of political and "technical-physical" arguments was evident, and palpable during these days in Varenna. Still keenly, I remember a direct discussion with E. Wigner at the dinner table. First we talked a little about the best formalism for Q.M.; then Wigner said that students of the '68 movement "must be mad". To that I replied: "They must be clever".

Our conversation stopped. Toraldo di Francia, and Franco Selleri both sided with the Partito Comunista Italiano, I sided with the students' movement against the Soviet Union hoping for a New Left (Freire 2014, pagg. 210-211).

I decided to quote the above excerpts on Wigner because Selleri was always weirdly and peculiarly intertwined to him.

Surely they had a great difference with respect to political ideas and commitment (Selleri being a Communist and against wars and weapons, whereas Wigner was active in JASON⁴⁴³, and in agreement with Vietnam War) as well as on the role of observer's consciousness in QM issue on measurement. Moreover, Selleri criticized the meaning of the so-called "Wigner's friend", a macroscopic description of the Schrödinger cat's paradox. But they exchanged a huge correspondence one another. Selleri asked some advice for the organization of his Conference at Urbino, he studied on the theorem of Araki-Yanase-Wigner for the measurement process applying it to correlated systems, he wrote a paper specifically on Wigner measurement's theory, and he made huge citations of Wigner work in his papers. In addition, Selleri made himself fully available to Mr. David Hamilton, Attorney at Law, when the latter asked some information concerning a person who wanted to denounce (better, blackmail) Wigner himself⁴⁴⁴.

I found in Selleri's archives a letter regarding this latter event:

Dear Mr. Hamilton,

It is incredible to read that prof. Wigner is being sued by Mr. Gerhard Heise. I am however sure that the question will be solved soon and in favour of Prof. Wigner. Perhaps I can give you some piece of information that can help you in understanding better Heise's personality.

1) We met in Vienna in April 1984 in order to discuss the plans of a future conference: he was credible only because he said that he was working under the supervision of Prof. Wigner.

⁴⁴³ See footnote 391 at page 286.

⁴⁴⁴ I found also a letter to Hamilton written by Abner Shimony, not here reported, though.

He told me there a complicated story about all his money having been stolen on the train from Köln to Wien. He asked me some money, but fortunately I had with me only what I needed and I was unable to help him.

2) Some time later Oreste Piccioni told me on telephone that he had given money to Heise and that he had the strong impression that he would never see it again.

3) Still later Basil Hiley told me the same story about Prof. David Bohm having lost money with Heise, money that he had been asked to lend with some excuse.

So it appears that not science but handfuls of dollars are very important to Mr. Heise.

I suggest that you contact both Prof. Piccioni and Dr. Hiley, tell them the story concerning me written above, tell them that I suggested that they might have similar facts to report, ask them if they know of further facts of this nature. Perhaps it is better if you treat otherwise the previous information as confidential.

If I can help prof. Wigner in any way only let me know (Selleri to Hamilton, 6 June 1987-FSPA. Underline in the original text. My *Italics*).

So, beyond political or scientific disagreements, respect is always the basis for relations between Selleri and Wigner. The latter was one of the greatest physicists of XX century, awarded Nobel Prize in 1963 “for his contributions to the theory of the atomic nucleus and the elementary particles, particularly through the discovery and application of fundamental symmetry principles”⁴⁴⁵.

2.6.3-VARENNA 1970 SUMMER SCHOOL AS A REAL TURNING POINT IN FQM. SOME EVIDENCES

What I would like to emphasize is the widespread and well-rooted awareness that, thanks to the original proposal of Selleri and Castagnoli (concerning the choice of the latter of d’Espagnat as a Director), the School established the starting point for a wider debate on FQM, as pointed out by several historians.

For instance, Freire argued “It can be said that the very existence of this school reflected the changing mood among physicists concerning the status of research on foundations of quantum theory” (Freire 2014, pagg. 158-159). According to (Del Santo 2020):

⁴⁴⁵ <https://www.nobelprize.org/prizes/physics/1963/summary/>. Accessed 24 September 2020.

[...] In conclusion, the Varenna school of 1970 represented a gathering of physicists who would soon recognize themselves as a new international community of “dissidents”. Simultaneously, the spirit of the time, with its wind of political renewal, entered the scientific debate and stimulated several young physicists to criticize the traditional values of scientific practice, as well as the content of research. Quantum foundations thus became the starting point for a concrete change.

With respect to Varenna 1970 and the role played by Selleri, in Freire’s interview to Selleri himself, there is an interesting exchange of dialogues:

SELLERI:

When I proposed to keep, to have this school, it was accepted. It was accepted, yes, by the other members, like Castagnoli, Ricci, Toraldo di Francia. I do not remember any real opposition to my proposal; only a certain cautiousness. And they invited the speakers, more speakers than I had thought, and I think they did a good job.

[...] The 1970 Varenna Summer School was accepted very well, especially by young physicists. That was a wonderful moment after the student unrest that had been very active in the physics departments. People were also very strong and motivated towards new ideas.

FREIRE:

If I could say one thing, my idea from historical studies of taking 1970 as a turning point, that idea came exactly when I saw the photo and the list of the names of people who talked and who attended Varenna Summer School.

SELLERI:

Good choice.

FREIRE:

Yeah, yeah, I was very impressed.

SELLERI:

It was a very good choice, but you see, that was a moment in which so to say the foundations of quantum mechanics became, if I may use the expression, a mass activity. It’s not really true, but I mean compared with before (AIP 2003, My underline).

Freire himself recognized the importance of Selleri:

[...] it is useful to enlarge the biographical information about Franco Selleri, because among the Italian physicists⁴⁴⁶ who suggested the school on foundations he would become in the future years a leader in this field of research (Freire 2014, pag. 212).

⁴⁴⁶ Freire reported several scholars as school proponents, whereas Selleri was the first who proposed specifically the FQM as topic of the school and he stressed that it should be decided immediately and not at the following meetings.

Indeed, the 1970 Varenna Summer School was a huge success and a real turning point for a real widening of scholars who, for the most disparate reasons and belief, started to have a lot of researches on FQM.

Firstly was for Selleri, the only Italian lecturer (apart Prosperi) at School, with his intervention “Realism and the wave-function of quantum mechanics,” submitted in the *Interpretation and proposals* section; and in his participation of the roundtable on “Themes of future experiments”, together with Bell, Jauch, Kasday, Shimony and Yanase, scientists with some of whom he had many contacts later.

According to (Freire 2014, pag. 202), at Varenna, “Selleri presented a research program designed to reform quantum physics and in which physics, philosophical and social commitment made up equal parts”.

Selleri also declared to Freire that his turning point was Varenna 1970, although I demonstrated that he switched from PP to FQM quite earlier. But Selleri’s words could be a confirmation of the beginning of a full time and hectic activity on FQM soon after Varenna School, in which it was presumable that he initiated many contacts and collaborations. Selleri focused his studies mainly on Bell’s Inequalities and experiments, on Einstein-Podolski-Rosen paradox, Empty Waves and their observation, and so forth.

But the 1970 Varenna Summer School turned out to be a real spring, in which many seeds contributed to wide the need and the will to do more precise analysis and research. A lot of scholars took advantages and strength to start or continue their efforts.

First of all, in the school maybe started the first huge debate on Bell’s theorem, just introduced by John Stewart Bell in 1964 and 1966 and on the potential experiments related to it. According to Freire:

[...] For Selleri it was the first opportunity to become acquainted with the theme, which would go on to occupy all his professional energy from then on, becoming the most vocal critic of loopholes in these experiments, which according to him would save local realism (Freire 2014, pag. 206).

Freire investigated on this aspect and summarized on the indisputable importance of the School for some participants and the same future of FQM:

The 1970 Varenna Summer School was successful due to the scientific and philosophical discussions it stimulated. Its 84 participants, the quality of their talks, the quick publication of its proceedings (d’Espagnat 1971), the first debates on Bell’s theorem and experiments, the debates on the quantum measurement problem, and the diversity of its speakers [...] created an air of excitement around research into foundations. In the case of this school the subject already had a small but unconnected community and the school helped network these scientists, bringing together most of the physicists who would go on to contribute to the blossoming of this research in the 1970s.

For instance, John Bell, Abner Shimony, Bernard d’Espagnat, and Franco Selleri—all of them worked on Bell’s theorem. In the case of Bell and Shimony they had already been working on this subject, and in the case of d’Espagnat and Selleri they turned their attention to it.

[...] As for d’Espagnat, he was then beginning to work on foundations and the success of the school led him to be regarded as one of the experts in the field.

[...] It was also an important event for Zeh, who needed encouragement for the research he was beginning and which would later lead him to the decoherence effect. The school was also the event at which Bryce DeWitt expressed his recent conversion to Everett’s many-worlds interpretation. It conveyed the feeling of openness towards the existence of a diversity of interpretations of quantum mechanics rather than just the complementarity interpretation. The school was also helpful for some of its attendees who would later become leaders in the research on the foundations of quantum physics. This was the case with Basil Hiley and Emilio Santos. Hiley was then an assistant of David Bohm at Birkbeck College in London and would become the key protagonist in Bohm’s quest for a mathematical treatment for the ideas of wholeness and implicate order. Santos would work on stochastic electrodynamics and implications for the interpretation of quantum theory and animate conferences on foundations in Spain.

Finally, the school strengthened the existence of a quantum measurement problem, and Eugene Wigner, as one of its keynote speakers, presented the diverse proposals to solve it.

[...] In short, the school encouraged physicists to change their research agenda to include these topics and/or encouraged those already working on them, thus contributing to the professional recognition of a research theme in serious need of attention (Freire 2014, pagg. 206-207).

An impressive testimony on the epochal role played by the Varenna school on the widening of awareness, also social and political, as well as the interest

in FQM comes from the young physicist G. Mattioli, winner of the Stanghellini prize at SIF in 1971, which is part of the debate on the research done, together with that on the postponement of Varenna HoP School to 1972, in the 165th SIF Presidency Board, held in January 1971. Some excerpts of Mattioli intervention at SIF are here reported⁴⁴⁷:

I believe, even in my scientific life, that the school of Varenna was a very important singular pole: both because, starting from the school of Varenna, I was able to make a study program that [...] has earned me the Stanghellini prize, but especially for what it has meant, in my scientific life and that of many colleagues, this school.

[...] [in the report for the competition] it was necessary to say how you were planning to use this scholarship, and then I said the physics subjects I have been interested in so far (which are the poles of Regge) and then I said that it was not possible to have gone to Varenna's school without saying that it definitely seemed impossible for me to resume dealing with those things. That is, after discovering that one has lived for 10 years working on the Quantum Mechanics and absolutely ignoring that, upstream, there are such contradictions and paradoxes, so, I don't mean the poles of Regge, but even the S matrix or even more important things - the Field Theory - are things that have at least very questionable and very problematic foundations.

[...] My personal conviction, having in mind the "critical tools" that this School of SIF has given me [...] is that if we look with an objective eye at the Physics we are doing, I believe that few people can say with serenity that they feel that they have all the requisite cultural skills to motivate the research they are doing.

[...] and it seems to me that, at the school in Varenna, the essence that came out of the debate was that, in fact, there were reasons why a cultural debate of general propositions on Quantum Mechanics, which was very much alive around 1930, suddenly disappeared with the school of Copenhagen. It disappeared because, in the end, this Physics must then be carried forward, it is a great mechanism that must churn out publications, it must churn out a great deal of physics to give support and receive support from and to a large amount of experimental physics; is a great deal of experimental physics that is done at the level of accelerators, at ever higher energies [...] therefore [it is] a business of increasingly colossal industrial proportions that is selected and governed, fatally beyond our intentions, by the country that has the opportunity to invest more capital in it. I think Varenna's school also highlighted another fact. That is, in the face of the problems, at the core of the problems, which were cultural problems and also invested philosophical instruments, it came out in a clear way that all of us - from the last student to the first

⁴⁴⁷ The intervention in Italian is in Addendum 3, pp. 992-993.

speaker - on the level of philosophical instruments were really very frail, to the point that the operation that was carried out almost daily was to say: well, since the average physicist does not have these philosophical instruments, then let us do a very simple operation: let us say that between Physics and Philosophy there is no relationship, we cut Philosophy because we do not know it.

And then another fact also emerged: beyond the political malice of the people, as soon as the problems invested also the political aspects (this relationship between science and society, this wondering why in the end the debate on these problems of the Quantum Mechanics had remained at the level of 1930, what interests were at stake), came out an image of the physicist who, even on the political level is extremely unwise, still believes in the neutrality of science, in the myth of progress and so on.

It seems to me that this need should not be buried, which, if I am not mistaken, exploded last year at the SIF Congress. It would really be a great short-sightedness, a missed opportunity to see it finally sink, to ask: 1) what is the validity, but in a global and cultural sense finally, of the research that is done; 2) if this validity is questioned (because I dare to say that the theoretical physics that I and many of my colleagues do, is an absolutely worthless physics: It only consists in doubling the number of parameters on more or less ragged models), well, let us finally ask ourselves why we are forced to do this research that has no cultural value; and thirdly, let us also take some operational decisions in this sense. [...] I do not know, I spontaneously say, to the workers who pay for this university and this research [...] that, from the point of view of the problems that this country has, actual problems [...], we have very little to say and we do research that is worth very little in the sense of the essential problems, for example the problems of the workers (Minutes of the 165th Steering SIF Committee, January 1971, pagg. 9-11 IN SIF bulletin n. 83 del 30/06/1971. My underline).

I decided to cite this passage because many excerpts of Mattioli's intervention recalled the famous *La piramide azteca della fisica teorica delle particelle elementari* by Selleri, as well as highlighting and summarising some issues discussed within the 1970 Varenna school itself. Surely another confirmation of the agreement of several scholars with Selleri's critical attitude towards elementary particle physics and FQM, Selleri's clarity in disseminating science in every field, with a simplicity of language that reached anyone, even and especially workers, and also emphasizing the uselessness of an approach that closes to understanding, drowned by mathematics.

With respect to the widening of study of FQM, Selleri always emphasized how Italy experienced a peculiar approach on it. In his view, this unusual approach is related to a cultural heritage influenced by Enrico Fermi.

In his interview with Freire Jr, Selleri stated first how he had the feeling that:

Italy is more tolerant than other countries for the foundations of quantum mechanics. For example in Italy I am here talking to you after many decades of activity in the foundations of quantum mechanics and of relativity. And Italian physicists have been basically fair with me on the average. Of course you can find unfair physicists here as well. On the average they have been fair. They pay for my publications, they pay for my travels to conferences and so on, and I am free to do my activity. Although basically [...] I am not as strong as I would wish to be — as influential as I would wish to be. I am not. But I don't know if that depends so much on repression as it does on the fact that anybody doing physics is less powerful than anybody doing something else. If you are interested mostly in organization, power and getting money you are more influential than somebody who spends all the time in research (AIP 2003)

Then he claimed how Fermi's thoughts of QM could be a further reason for a greater freedom found in Italy towards a plurality of approaches in the study of FQM. His idea was really appreciated by Freire himself:

SELLERI:

In my opinion the difference between Italy and other countries depends on Enrico Fermi, because Fermi had doubts about quantum mechanics. I definitely would like to give you a copy of this evidence.

FREIRE:

Oh, it's very, very interesting to hear this.

SELLERI:

Yeah. He never published any critical paper on quantum mechanics, but his friends and biographers wrote about his unhappiness. For example he was unhappy when he went in Germany in 1926 and he said that Pauli and Heisenberg had done great work, but really had exaggerated in not trying to understand the physical reality correctly. He said this and several other interesting things as is reported by Pontecorvo, by Segre and by Persico, people who knew him very well and who wrote books about him. And I have collected all the pieces of evidence I could find of Fermi being critical of quantum theory.

FREIRE:

And did you intend to publish something about this?

SELLERI:

No, because it is only five or six sentences by Fermi. I will give them to you and if you want to use them in your publications please feel free to do so.

FREIRE:

Oh, I would like, I would like very much, because from the historical point of view you are talking a very interesting and new thing to reflect.

SELLERI:

So you see Fermi was a great man, was a great physicist with a fantastically powerful brain that never stopped to work.

He was capable of creating physics in different fields. He was a theoretician, he was an experimentalist and he was a technologist, and in every field he produced top research.

I had an experience myself once. I believe it was 1980, I'm not sure. I went to Paris for a conference on the history of elementary particle physics. All the top physicists — Feynman, Gell-Man, Yang, Lee, all the top men in particle physics were there. Well, that conference on the history of elementary particle physics practically became a celebration of Enrico Fermi. Everybody was talking about him, as if there was nothing else interesting to talk about. So I had the dimension of the man. Unfortunately as you know, he died when he was fifty-five. He died very early [...] and that was a tragedy, because all the top people of elementary particle physics are pupils of Enrico Fermi. They are extremely brilliant people, very good, but they lack the good sense that Enrico had. His good sense and his directive role in elementary particle physics for another fifteen years or so could have changed the history of modern physics, if he had survived.

[...] Enrico Fermi was the father of Italian physics, he was the top man who taught to people such as Amaldi, Pontecorvo, Segre and many others who were recognized as leaders in atomic physics. So if he had doubts perhaps you can imagine that his followers were not so dogmatic about those quantum things.

[...] I had no doubt, knowing the situation in countries like the Soviet Union, United States and France, that I would have been destroyed there as a scientist. There was no tolerance for the foundations of quantum mechanics (AIP 2003).

Besides the cues that show the role of Selleri, the profitable research in ASIF has pushed me to show also other ideas that I considered paramount in underlining the importance of the Summer School of Varenna 1970.

After this first remarkable school in FQM, only two more followed at the moment. In 1977, the LXXII Varenna Course, on “Problems in the Foundation of Physics” whose director was Toraldo di Francia; and in 2016, Varenna Course 197, on “Foundations of quantum theory”, whose directors were E.M. Rasel, W.P. Schleich, and S. Wölk.

2.7-SELLERI'S RESEARCH DURING THE 1970s AND THE 1980s: EPR, BELL'S INEQUALITIES, BELL-TYPE EXPERIMENTS

2.7.1-FOREWORD

In the present and in the following sections, 2.8 and 2.9, I will show how Selleri, in the years following Varenna 1970 and up to the early 2000s, jointly followed and carried out several lines of research in FQM. He dealt with Hidden Variables (HV), Bell's Inequalities, Einstein, Podolski, and Rosen (EPR) Paradox, first and second type State Vectors, and Empty Waves (EW). As concerns his activity, in a manifold lists made to summarize his activity, Selleri created these distributions: "wave-particle duality [including Hidden Variables, Bell's inequality and empty waves], 1970-1990; papers on epr paradox, 1980-2001" (FSPA).

I will start introducing the EPR paradox, Bell's theorem and inequality, and the related experiments, drawing (always without any claim to exhaustiveness) the original papers as well as the analysis of several scholars, like, for instance Don Howard, (Freire 2014), (Jammer 1974), (Kumar 2009), and Angela Laurora's PhD thesis⁴⁴⁸, fully devoted to John Stewart Bell and to his role on the debate on EPR paradox.

Then, I am going to analyze Selleri's contribution on Bell's inequality, connecting it to his contemporary debate, as, for instance, to the evolution of Bell's inequalities experiments, or to the controversy that followed Aspect's experiments, thanks to the interviews I carried out and to several letters found in Selleri's archive. In addition, the discovery of some remarkable handwritten notebook allowed me to shed some light on his thought on those years. Selleri's contribution to EPR paradox is also well investigated.

In the next section (2.8) I will introduce Selleri's work on EW during the 1980s.

⁴⁴⁸ "John Bell e le ricerche sul paradosso di Einstein, Podolsky e Rosen". (Laurora 2014) in the following.

Although I already introduced EW and HV in section 2.3, nevertheless I resumed them showing in particular the great work produced by Selleri and the group of Bari for EW detection.

The 1980s were also remarkable for the beginning of Selleri's full professorship at University of Bari as well as for the establishment of his research group, enhancing his busy activity with organized conferences, travels for research, several important papers, but also with many popularization articles and several books in different languages that had a great response.

Finally, in the section 2.9, I will reveal how he applied the EPR to particle physics, writing some very significant works on kaons and ϕ -factories, with the proposal of an *experimentum crucis* able to discriminate between QM and Local Realism (LR).

2.7.2-A GLANCE TO EPR

Among the critics to the Copenhagen interpretation we should certainly remember, in the first period, the widespread activity played by Albert Einstein, who always challenged the inconsistency of the QM.

Indeed, QM led to the disappearance of an objective reality and, with Bohr's principle of complementarity, to the renunciation of a causal and spacetime description of microscopic phenomena as well as a waiver of the wave-corpuscle duality. In Solvay conferences held in 1927 and in 1930, Einstein submitted several *gedankenexperimente* to Bohr and the other members of the orthodox interpretation. Einstein aimed at showing the contradictory nature of QM, giving a remarkable importance to separability and locality. Anyway, without having the expected effect of making the QM inconsistent. His proposals were remarkable but otherwise unprofitable.

Einstein made other several efforts, like the paper written with Richard Tolman and Boris Podolsky, *Knowledge of Past and Future in Quantum*

*Mechanics*⁴⁴⁹. According to Robert P. Crease and Alfred S. Goldhaber this latter was:

another blast at the uncertainty principle using another thought experiment, pointing out that keeping it requires junking the presumably essential qualities of locality and separability (Crease, Goldhaber 2015, pag. 197).

Nonetheless, in 1935, Einstein went instead to try proving the incompleteness of QM. The very famous article he published with Boris Podolski and Nathan Rosen (with more than nineteen thousand citations)⁴⁵⁰, *Can Quantum-Mechanical description of physical reality be considered complete?*⁴⁵¹ went down in history as Einstein, Podolski, and Rosen Paradox⁴⁵². EPR is often used to state the three authors and the paper, too⁴⁵³.

[...] Einstein met Podolski and Rosen in his office to thrash out his idea. Podolsky was assigned the task of writing the resulting paper, while Rosen did most of the necessary mathematical calculations. Einstein, as Rosen recalled later, 'contributed the general point of view and its implications' (Kumar 2009, pag. 303).

Having regard to the impossibility to prove the logical inconsistency of QM, Einstein decided to try disclosing its incompleteness. Firstly, Einstein, Podolsky and Rosen started by arguing on objective reality as it is, as well as our perception of it, and on the parameters to be followed for the correctness and the completeness of a theory, still introducing the completeness condition:

Any serious consideration of a physical theory must take into account the distinction between the objective reality, which is independent of any theory, and the physical concepts with which the theory operates. These concepts are intended to correspond with the objective reality, and by means of these concepts we picture this reality to ourselves.

⁴⁴⁹ See also (Jammer 1974), pagg. 167-169.

⁴⁵⁰ According to Google Scholar. Accessed 24 September 2020.

⁴⁵¹ Einstein A., Podolsky B., Rosen N., "Can Quantum Mechanical Description of Physical Reality Be Considered Complete?", *Physical Review*, 47, pagg. 777-780.

⁴⁵² Further referred to as (EPR 1935).

⁴⁵³ As it happens for truly epochal works, which are often called in the scientific environment with surnames or initials, rather than with the title of the article

In attempting to judge the success of a physical theory, we may ask ourselves two questions: (1) “Is the theory correct?” and (2) “Is the description given by the theory complete?”. It is only in the case in which positive answers may be given to both of these questions, that the concepts of the theory may be said to be satisfactory. The correctness of the theory is judged by the degree of agreement between the conclusions of the theory and human experience. This experience, which alone enables us to make inferences about reality, in physics takes the form of experiment and measurement. It is the second question that we wish to consider here, as applied to quantum mechanics.

Whatever the meaning assigned to the term *complete*, the following requirement for a complete theory seems to be a necessary one: *every element of the physical reality must have a counter part in the physical theory*. We shall call this the condition of completeness (EPR 1935, pag. 777. *Italics in the original text*).

After having introduced the objective external reality, which is not up to the observer and by which the systems have their properties independent of the observations; and after the condition of completeness, Einstein, Podolsky and Rosen introduced the criterion of reality:

The elements of the physical reality cannot be determined by a priori philosophical considerations, but must be found by an appeal to results of experiments and measurements. A comprehensive definition of reality is, however, unnecessary for our purpose. We shall be satisfied with the following criterion, which we regard as reasonable.

If, without in any way disturbing a system⁴⁵⁴, we can predict with certainty (i.e., with probability equal to unity) the value of a physical quantity, then there exists an element of physical reality corresponding to this physical quantity. It seems to us that this criterion, while far from exhausting all possible ways of recognizing a physical reality, at least provides us with one such way, whenever the conditions set down in it occur. Regarded not as a necessary, but merely as a sufficient condition of reality, this criterion is in agreement with classical as well as quantum-mechanical ideas of reality (EPR 1935, pagg. 777-778. *Italics in the original text*).

It was the famous definition to which Niels Bohr strongly opposed, quite refuting it.

It can be inferred how Einstein, Podolsky, and Rosen would like to create a good method to underline the incompleteness of QM, conceiving a thought

⁴⁵⁴ Writing in this way, they took into account the Heisenberg uncertainty relations.

experiment on two correlated quantum objects, considering two physical quantities:

In a complete theory there is an element corresponding to each element of reality. A sufficient condition for the reality of a physical quantity is the possibility of predicting it with certainty, without disturbing the system. In quantum mechanics in the case of two physical quantities described by non-commuting operators, the knowledge of one precludes the knowledge of the other. Then either (1) the description of reality given by the wave function in quantum mechanics is not complete or (2) these two quantities cannot have simultaneous reality. Consideration of the problem of making predictions concerning a system on the basis of measurements made on another system that had previously interacted with it leads to the result that if (1) is false then (2) is also false. One is thus led to conclude that the description of reality as given by a wave function is not complete (EPR 1935, pag. 777).

Angela Laurora pointed out the relationship between realism and causality and between the object and the measuring device behind the reality criterion:

[...] According to this criterion, the element of reality exists even in the absence of an act of measurement and this is consistent with the hypothesis of realism. If the measure is carried out, the aforementioned result of the measure will be determined causally by the element of reality, so that realism and causality are closely linked.

The criterion of reality is associated to the object and not to the measuring device, in the sense that if we can predict with certainty that a device will give a predetermined result by interacting with the object, then the decisive factor that produces the result of the measurement can only be in the object, despite the fact that the device may give results [quite] different from the predetermined one (Laurora 2014, pag. 4. My underline)⁴⁵⁵.

Kumar underscored what was behind the reality criterion:

Einstein did not want to get stuck in the philosophical quicksand, which had swallowed so many, of trying to define 'reality'.

⁴⁵⁵ “[...] Secondo tale criterio, l’elemento di realtà esiste anche in assenza di un atto di misura e ciò è coerente con l’ipotesi del realismo. Qualora la misura sia effettuata, il risultato predetto della misura sarà determinato in maniera causale dall’elemento di realtà, per cui realismo e causalità sono strettamente legati. Il criterio di realtà è associato all’oggetto e non all’apparato di misura, nel senso che se possiamo prevedere con certezza che un apparato darà un risultato predeterminato interagendo con l’oggetto, allora il fattore decisivo che produce il risultato della misura non può che essere nell’oggetto, nonostante l’apparato possa dare risultati diversi da quello predetto” (Laurora 2014, pag. 4. My underline).

In the past, none had emerged unscathed from an attempt to pinpoint what constituted reality. Astutely avoiding a ‘comprehensive definition of reality’ as ‘unnecessary’ for their purpose, EPR adopted what they deemed to be a ‘sufficient’ and ‘reasonable’ criterion for designating an ‘element of reality’ (Kumar 2009, pag. 304).

In his 1974 book, Max Jammer summarized the basic premises for EPR:

The Einstein-Podolski-Rosen argument for the incompleteness of quantum mechanics is based [...] on two explicitly formulated and two tacitly assumed [...] premises.

The formers are:

1) *The reality criterion*. “If, without in any way disturbing a system, we can predict with certainty (i.e., with probability equal to unity) the value of a physical quantity, then there exists an element of physical reality corresponding to this physical quantity” [sufficient but not necessary condition of reality].

2) *The completeness criterion*. A physical theory is complete only if “every element of the physical reality has a counterpart in the physical theory” [necessary condition of completeness].

The tacitly assumed arguments are:

3) *The locality assumption*. If “at the time of measurement...two systems no longer interact, no real change can take place in the second system in consequence of anything that may be done to the first system”.

4) *The validity assumption*. The statistical predictions of quantum mechanics - at least to the extent they are relevant to the argument itself - are confirmed by experience (Jammer 1974, pagg. 184-185. *Italics* in the original text).

[...] The Einstein-Podolski-Rosen argument then proves that on the basis of the reality criterion 1, assumptions 3 and 4 imply that quantum mechanics does not satisfy criterion 2, that is, the necessary condition of completeness, and hence provides only an incomplete description of physical reality.

[...] The authors never regarded their thesis as a “paradox”, neither in the sense of the medieval *insolubilia* nor in the more modern sense of syntactical or semantical antinomies. It was later referred to as a paradox [probably for the first time by Schrödinger] because its claim - the state of one system depends on what the experimenter decides to measure on another (remote) system - was regarded as counterintuitive or contrary to common sense (Jammer 1974, pagg. 184-186. *Italics* in the original text).

QM asserted that the wave function is the only and complete description of physical reality of a system in a particular state.

On the contrary, EPR demonstrated that in the objective reality there are more physical properties than in the wave function, so QM was incomplete. The conclusions of EPR stressed the lack of completeness of QM. The three authors emphasized also about the lackness of restrictiveness of their reality criterion and left open the opportunity of a complete description of the physical reality:

Previously we proved that either (1) the quantum-mechanical description of reality given by the wave function is not complete or (2) when the operators corresponding to two physical quantities do not commute the two quantities cannot have simultaneous reality. Starting then with the assumption that the wave function does give a complete description of the physical reality, we arrived at the conclusion that two physical quantities, with noncommuting operators, can have simultaneous reality. Thus the negation of (1) leads to the negation of the only other alternative (2). We are thus forced to conclude that the quantum-mechanical description of physical reality given by wave functions is not complete. One could object to this conclusion on the grounds that our criterion of reality is not sufficiently restrictive. Indeed, one would not arrive at our conclusion if one insisted that two or more physical quantities can be regarded as simultaneous elements of reality *only when they can be simultaneously measured or predicted*.

While we have thus shown that the wave function does not provide a complete description of the physical reality, we left open the question of whether or not such a description exists. We believe, however, that such a theory is possible (EPR 1935, pag. 780 *Italics in the original text*).

EPR did not indicate any theory but a possible alternative could rest in a causal and spacetime description of microscopic physical phenomena in order to avoid paradox.

Hidden Variables theories could be necessary for a possible completeness of a quantum theory and EPR (as well as Bell's work) had them inside their core, being also strictly connected to them.

Jammer argued on the importance of EPR for HV, still recalling how Einstein was not directly involved in their use:

Although the Einstein-Podolski-Rosen incompleteness argument was undoubtedly one of the major incentives for the modern development of hidden variables theories, it would

be misleading to regard Einstein, as some recent authors do, as a proponent of or even as “the most profound advocate of hidden variables”. True, Einstein was sympathetically inclined toward any efforts to explore alternatives, and as such also the ideas of de Broglie and Bohm, but he never endorsed any hidden variable theory.

[...] No doubt, Einstein’s criticisms, and in particular his work with Podolsky and Rosen, greatly contributed to the development of hidden variable theories [...]; but, as is not uncommon in the history of physics, the intellectual originator of a theory does not necessarily identify himself with its full-fledged development (Jammer 1974, pagg. 254-255).

A complete theory is based on reality and locality, where the former emphasizes that particles exist and have definite physical properties; and the latter affirms that if two systems are separated from each other for a reasonable time and distance, a measurement on the first system can produce no real changes in the second one.

Laurora pointed out the basic role of locality for EPR, bearing in mind, unlike Jammer, the involvement of Einstein in it:

A hypothesis not explicitly stated in the article is that of the locality, an assumption that Einstein later admitted to be the basis of the article’s argument [...].

[...] Einstein’s point of view is that long-distance action does not exist. Therefore the idea of locality is based on the reasonable assumption that two objects interact less and less with increasing relative distance. [...] known interactions tend to zero when the distance between the interacting objects tends to infinity. Therefore the hypothesis of locality can be formulated as follows: “Given two physical objects there exists a relative distance above which their mutual interaction becomes negligible, that is, the objects are separated and the modification of any physical magnitude of the one cannot influence the other”⁴⁵⁶.

⁴⁵⁶ “Un’ipotesi non esplicitamente dichiarata nell’articolo è quella della località, un’assunzione che in seguito Einstein ammise essere alla base dell’argomento dell’articolo [...].

[...] Il punto di vista di Einstein è che l’azione a distanza non esiste. Pertanto l’idea della località si basa sull’ipotesi ragionevole che due oggetti interagiscano sempre meno al crescere della loro distanza relativa. [...] le interazioni a noi note tendono a zero quando la distanza tra gli oggetti interagenti tende a infinito. Pertanto l’ipotesi di località può essere così formulata: ‘Dati due oggetti fisici esiste una loro distanza relativa al di sopra della quale la loro interazione reciproca diventa trascurabile, cioè gli oggetti sono separati e la modifica di una qualsiasi grandezza fisica dell’uno non può influenzare l’altro’ ” (Laurora 2014, pagg. 4-5).

Anyway, several studies started to think of the opportunity to use HV theories in order to find an alternative to QM.

Einstein was unsatisfied also about the EPR paper, actually.

It was factually written by Podolski and Einstein complained the lack of clarity and style he was used to operate when he prepared his papers in German language. Specifically, according to Kumar, Einstein:

...was unhappy that the fundamental role of separability, that the state of one object cannot depend upon the kind of measurement made on another spatially separated object, had been obscured in the paper. Einstein wanted the separation principle to be an explicit feature of the EPR argument and not as it appeared, on the last page, as some sort of afterthought. He wanted to draw out the incompatibility of separability and the completeness of quantum mechanics. Both could not be true (Kumar 2009, pag. 314).

A further confirmation of the Einstein complaining on EPR as well as on the preminent role played by separability and locality in Einstein's thought comes from Don Howard's 2015 book, *Anche Einstein gioca a dadi. La lunga lotta con la meccanica quantistica*⁴⁵⁷, in which four papers⁴⁵⁸ are translated in Italian and collected. In a letter of 19 June 1935 to Schrödinger [...] Einstein wrote:

"[...] For language reasons, [the EPR article] was written by Podolsky, after much discussion. But it still did not come out as well as I would like; on the contrary, the main point was, so to speak, stifled by erudition"⁴⁵⁹.

⁴⁵⁷ Howard D. (2015), *Anche Einstein gioca a dadi. La lunga lotta con la meccanica quantistica*, Fano V., Tassani I., Bompreszi G. (Eds), Carocci Editore. (Howard 2015) in the following.

⁴⁵⁸ Howard original papers are:

"Einstein on Locality and Separability", in *Studies in History and Philosophy of Science*, 16, 1985, pagg.171-201, (Howard 1985) in the following.

"'Nicht sein kann was nicht sein darf?': or the Prehistory of EPR, 1909-1935: Einstein's Early Worries about the Quantum Mechanics of Composite Systems", in Miller A.I.(ed.), *Sixty-Two Years of Uncertainty: Historical, Philosophical, and Physical Inquiries into the Foundations of Quantum Mechanics*, Plenum Press, New York-London, 1990, pagg. 61-111, (Howard 1990) in the following.

"Einstein and 'Eindeutigkeit': A Neglected Theme in the Philosophical Background to General Relativity", in Eisenstaedt J., Kox A.J. (eds.), "Studies in the History of General Relativity", *Einstein Studies*, 3, Birkhäuser, Boston 1992, pagg. 154-243.

"A Peek Behind the Veil of Maya: Einstein, Schopenhauer, and the Historical Background of the Conception of Space as a Ground for the Individuation of Physical Systems", in Earman J., Norton J.D. (eds.), "The Cosmos of Science: Essays of Exploration", *Pittsburgh-Konstanz Series in the Philosophy and History of Science*, 6, University of Pittsburgh Press-Universitätverlag, Pittsburgh-Konstanz 1997, pagg. 87-150.

⁴⁵⁹ "[...] per ragioni di lingua, è stato scritto da Podolsky, dopo molte discussioni. Ma ancora non è venuto così bene come vorrei; al contrario, il punto principale è, per così dire, soffocato dall'erudizione" (cited in Howard

Howard speculated, in his 1990 paper, how Einstein's thought was centered on separability problem since 1905, being the *Leitmotiv* of his research from 1909 to 1935, more than determinism and Heisenberg uncertainty relations. Howard reminded how Einstein wrote in several papers his preminent ideas still renouncing to the elements of reality, the specific subject which in EPR requested a sort of self-justification to possible criticism. Indeed, Bohr criticized the very element of reality to crush EPR, having found nothing wrong with it from the technical point of view.

Howard also stressed how Einstein, in "Quanten-Mechanik und Wirklichkeit", managed to shed light on the difference between separability and locality using rational and clear terms. Referring to the former, Einstein argued:

the concepts of physics refer to a real external world, *i.e.*, ideas are posited of things that claim a 'real existence' independent of the perceiving subject (bodies, fields, *etc.*), and these ideas are, on the other hand, brought into as secure a relationship as possible with sense impressions. Moreover, it is characteristic of these physical things that they are conceived of as being arranged in a space-time continuum. Further, it appears to be essential for this arrangement of the things introduced in physics that, at a specific time, these things claim an existence independent of one another, insofar as these things 'lie in different parts of space'. Without such an assumption of the mutually independent existence (the 'being-thus') of spatially distant things, an assumption which originates in everyday thought, physical thought in the sense familiar to us would not be possible. Nor does one see how physical laws could be formulated and tested without such a clean separation (Einstein 1948, cit. in Howard 1985, pagg. 187-188. *Italics in the text*).

Whereas, concerning to locality, Einstein claimed:

an external influence on *A* has no *immediate* effect on *B*; this is known as the 'principle of local action', which is applied consistently only in field theory.

The complete suspension of this basic principle would make impossible the idea of the existence of (quasi-) closed systems and, thereby, the establishment of empirically testable laws in the sense familiar to us (Einstein 1948, cit. in Howard 1985, pp 187-188. *Italics in the original text*).

2015, p. 227). Howard reported the Italian translation of the letter in the book and I translated this latter in English, as well as the original letter written in German, too (Howard 2015, pag. 286).

Einstein's own incompleteness argument first appears in the foregoing correspondence with Erwin Schrödinger in June of 1935, barely one month after the publication of the EPR paper. And it was emphasized again in a series of papers and other writings between 1936 and 1949⁴⁶⁰.

Howard dismantled the already acquired theses on EPR as the "definitive statement of Einstein's mature misgivings about the quantum theory":

In brief, the argument found there is this. First, a completeness condition is asserted as a necessary condition that must be satisfied by any acceptable scientific theory: "every element of the physical reality must have a counterpart in the physical theory" (EPR 1935, p. 777). Then a sufficient condition for the existence of elements of physical reality (the famous EPR reality criterion) is laid down:

"If, without in any way disturbing a system, we can predict with certainty (i.e. with probability equal to unity) the value of a physical quantity, then there exists an element of physical reality corresponding to this physical quantity" (Einstein, Podolsky, and Rosen 1935, p. 777). And then, finally, by means of a rather complicated argument, it is shown that in an EPR-type thought experiment involving previously interacting systems, elements of physical reality exist corresponding to both of two conjugate parameters for one of the two interacting systems, since the value of either could have been predicted with certainty and without physically disturbing the system on the basis of measurements carried out on the other system. But quantum mechanics holds that conjugate parameters, like position and linear momentum along a common axis, cannot have simultaneously definite values. Quantum mechanics is, thus, incomplete, since it fails to satisfy the completeness condition.

That is the standard account of Einstein's incompleteness argument. But that account is seriously wrong.

Einstein did think quantum mechanics incomplete, but for reasons significantly different from those advanced in the EPR paper. He repudiated the EPR argument within weeks of its publication; and from 1935 on, all of his discussions of incompleteness take a quite different form from that found in the EPR paper.

⁴⁶⁰ Namely, in a letter to Popper in November 1935; in Einstein A. (1936), "Physik und Realität", in *Journal of the Franklin Institute*, 221, pagg. 313-347 and 349-382; in Einstein A. (1948), "Quanten-Mechanik und Wirklichkeit", in *Dialectica*, 2, pagg. 320-324, (Einstein 1948) in the following. And in several writings collected in the remarkable Schilpp P.A. (1949), *Albert Einstein: Philosopher-Scientist*, The Library of Living Philosophers, Evanston, pag. 1; pagg 3-49; pagg. 665-688. In (Howard 1985) are all clearly and deeply introduced and illustrated.

He continued to be concerned with the peculiar way in which quantum mechanics describes interacting systems; but he never invoked the EPR completeness condition, he never invoked the reality criterion, and he never invoked the uncertainty relations. Moreover, what he does say makes far clearer than the EPR paper the connection between his critique of quantum mechanics, on the one hand, and his commitments to field theories and realism, on the other (Howard 1990, pagg. 63-64).

Howard highlighted how Einstein “wanted to show the quantum theory to be incomplete, and he wanted to do this by showing that the assumption of separability (plus the assumption of locality) is incompatible with the claim that the theory gives a complete description of individual systems” (Howard 1990, pagg. 103-104).

The separability was a necessary condition because the incompleteness of QM was strictly connected to its intrinsic non-separability, plainly evident in the fact that joint probabilities do not thus factorize. That is the famous problem of second type state vector. So,

the quantum mechanical denial of the independence of interacting systems necessarily yields an incomplete description of physical events if one seeks to apply it to systems assumed to satisfy a strict separability principle (Howard 1990, pag. 62).

QM did not satisfy it, unlike the field theories like General Relativity. So, Einstein decided to search for an unified field theory in order to overcome the QM.

Einstein was committed to separability because of his deeper commitment to field theories, and their associated way of describing interactions in purely local terms, a description that rests fundamentally on the assumption that every system, indeed, every point of the space-time manifold, has its own separate state in the form of well-defined values of the fundamental field parameters like the metric tensor (Howard 1990, pag. 105).

[...] Einstein claims that the incompleteness of quantum mechanics--in the special sense of its assigning different theoretical states to one and the same real state--follows inevitably if we insist upon the principles of locality and separability.

[So] we understand at last why Einstein's commitment to the program of field theories forced him to repudiate quantum mechanics.

For as Einstein himself later explained, both locality and separability, but especially the latter, are built into the ontological foundations of field theories.

[...] Einstein had not inconsiderable (if not ultimately compelling) arguments--methodological, epistemological, and metaphysical--for retaining both locality and separability, which helps to explain his dogged commitment to the field theory program as an alternative to quantum mechanics (Howard 1990, pag. 65).

A link with Selleri is given by the fact that Howard presented for the first time his ideas “on the [...] Einstein’s distinction between locality and separability”⁴⁶¹ at the conference held in Cesena in 1991, Bell’s Theorem and the Foundations of Modern Physics, co-organized by Selleri. Howard’s paper was then included in the volume edited by van der Merwe, Selleri and Tarozzi⁴⁶².

I would like to recall how Selleri was always firmly persuaded (since the beginning of his activity in FQM) of the validity of local realism, based on locality and separability.

He was in a great agreement with Einstein, one of his main source of inspiration⁴⁶³. Concerning the foregoing topic explained by Howard, Selleri wrote a paper in 1982, “Generalized EPR Paradox”⁴⁶⁴, highlighting the weakness of reality criterion and proposing a probabilistic one, in a peculiar phase of his research. Moreover, Selleri pointed out also his rejection of laplacian determinism, still preferring locality and separability. Stressing how the original proof by Bell himself of his own inequality was based on a deterministic scheme, Selleri confirmed to reject determinism and to prefer locality, studying a lot to create other inequalities, distinguishing between strong and weak ones. Specifically, according to Selleri’s formulation:

⁴⁶¹ “sulla [...] distinzione di Einstein tra località e separabilità” (Howard 2015, p. 13).

⁴⁶² Howard D., “Locality, Separability, and The Physical Implications of the Bell Experiments: A New Interpretation”, in Van der Merwe A., Selleri F., Tarozzi G. (eds) (1992), *Bell’s Theorem and the Foundations of Modern Physics*, Palazzo del Ridotto, Cesena, 7-10 October 1991, Singapore: World Scientific pagg. 306-314.

⁴⁶³ Even if Selleri as usual, never had a blind adhesion, even to Einstein, contesting his philosophical approach related to the introduction of the theory of special relativity.

⁴⁶⁴ Selleri F. (1982), “Generalized EPR Paradox”, *Foundations of Physics*, 12 (7), pp.645-659. (Selleri EPR Paradox 1982) in the following.

weak [or original Bell's] inequality [is] deduced from the sole assumption of local realism and violated by quantum mechanics in the case of nearly perfect instruments. Strong inequality [is] deduced from local realism and from *ad hoc* additional assumptions [...], and violated by quantum mechanics in the case of real instruments (Selleri EPR History 1988, pag. 25. My underline)⁴⁶⁵.

Some excerpts sound really impressive with respect to the above concepts:

A generalized reality criterion which attributes physical properties to statistical ensembles is used in order to deduce an inequality which is violated by quantum mechanics in realistic conditions. This new form of the Einstein-Podolsky-Rosen paradox is developed exclusively from the reality criterion and from separability without any use of the previously introduced restrictive assumptions about the probabilistic scheme.

[...] [The] famous “criterion of physical reality” proposed by Einstein, Podolsky, and Rosen in 1935 is a very weak criterion of reality, very carefully formulated and extremely general. It appeared to them (and it still appears today) “far from exhausting all possible ways of recognizing a physical reality” (Selleri EPR Paradox 1982, pag 645. My underline).

[...] one weak point in the [...] reality criterion [...] is in the predictability *with certainty* of the considered physical quantity: how can one ever be absolutely certain of anything in a concrete experimental situation? The predictability *with certainty* appeared in 1935 to be a consequence of quantum theory for certain types of state-vectors; the situation has however changed today, after the Wigner-Araki-Yanase theorem which proved the necessity of imperfect measurements, even in principe, if quantum theory has to be considered correct for all physical interactions including measurements. Insisting on the predictability with certainty has therefore no justification and leads furthermore to a narrow deterministic scheme which has unappealing features also for people who hold fast to a causal outlook. This is so also because in the last decades the philosophers of science have done a very good job in criticizing the laplacian determinism. The necessity of going beyond determinism has been felt immediately after the discoveries of Bell's inequality, whose original proof was still based on an oversimplified deterministic scheme (Selleri EPR Paradox 1982, pag 646. *Italics* in the original text. My underline).

⁴⁶⁵ Selleri F. (1988), “History of the Einstein-Podolsky-Rosen paradox”, in *Quantum Mechanics versus Local Realism. The Einstein-Podolsky-Rosen paradox*, London/New York: Plenum. (Selleri EPR History 1988) in the following. This work belongs to the best review papers Selleri wrote in his career, well centered on the state-of-the-art and full in historical representation and attempted solutions.

Again in his 1985 paper, “On the Notion of Probability for Pairs of Correlated Systems”⁴⁶⁶, in which he pointed out the importance of Popper’s propensity, Selleri stated in favour of locality, more preferable to determinism:

[...] The problem is that determinism very probably does not apply to the quantum domain and that modern epistemology has enriched scientific thought with the important distinction between causality and determinism, the latter appearing as a narrow and particular form of the former. Therefore it is possible today to defend causality but almost impossible to believe that determinism is universally valid: from this point of view it is finally decided that laplacian determinism is dead, that Brownian motion is an objectively causal phenomenon, and that objective causality is part of causality. In the quantum domain we have every reason for believing that a deterministic description is not universally applicable (Selleri notion 1985, pag. My underline).

Selleri he underscored the intrinsic weakness of the reality criterion.

Going back to the EPR article, although it was “officially” victoriously refuted by Bohr, remained unaffected in its possible indication of QM incompleteness as well as in the possibility of a new, more general theory, based on the HV.

Due to the importance of von Neumann’s theorem outcome as well of the cultural and sociological weight of Bohr, the EPR conclusions remained a metaphysical topic for approximately twenty years. According to Laurora,

The debate on the question opened by Einstein, Podolsky and Rosen, however, remained limited to a philosophical level and its development, not linked solely to the personal beliefs of the protagonists, was made cautious by an earlier work by von Neumann, “Mathematical Foundations of Quantum Mechanics”, which in 1932 had shown the impossibility of completing quantum mechanics in causal and local terms proposed by Einstein. In 1952, however, David Bohm [...] showed the feasibility of such completion of Quantum Mechanics and reformulated the terms of the paradox. It is precisely the reading of Bohm’s work that struck the interest of John Bell (Laurora 2014, XII)⁴⁶⁷.

⁴⁶⁶ Selleri F. (1985), “On the Notion of Probability for Pairs of Correlated Systems”, in *Determinism in Physics*, Bitsakis E. et al. (eds), Athens: Gutenberg, pp. 275-288. (Selleri notion 1985) in the following.

⁴⁶⁷ “Il dibattito sulla questione aperta da Einstein, Podolsky e Rosen, tuttavia, rimase limitato a un piano filosofico e un suo sviluppo, non legato unicamente alle convinzioni personali dei protagonisti, fu reso prudente da un’opera precedente di von Neumann, *Mathematical Foundations of Quantum Mechanics*, che nel 1932

2.7.3-BOHM RAISES THE STAKES AGAIN, THEN ENTERS BELL. A TURNING POINT FOR FOUNDATIONS OF QUANTUM MECHANICS

After EPR, at least passed seventeen years in which there seemed to be no longer any opposition to the dominant QM paradigm (mainly for the von Neumann theorem (which denied the opportunity of a causal and local completion of quantum theory as thought by Einstein), as well as its endorsement by several physicists, including the founding fathers of QM, like Bohr, Heisenberg, Born, Pauli and Jordan. Then, criticism took shape again in the 1950s and 1960s with the work of Bohm (in 1951 and 1952), Bohm and Vigier (in 1954), Everett (in 1956-1959) and Wigner (in 1961-1963). David Bohm was undoubtedly the first to rekindle the spotlight on the issue⁴⁶⁸. Unlikely Einstein, Podolski and Rosen, who considered two physical quantities of a particle, position and momentum, David Bohm in 1952 simplified the case for EPR using only the spin as a property of the quantum system, i.e., a molecule, formed by two atoms, in a state with the resulting spin equal to zero. Bohm made the first⁴⁶⁹ attack to von Neumann theorem, doing in fact what von Neumann's theorem was supposed to forbid: provide a causal formulation of the existing quantum theory, even though in a nonlocal sense⁴⁷⁰.

The two atoms of the quantum system can separate and reach distances where there is no longer any interaction. But for the conservation of the total angular momentum of spin, there will still be correlation between the two individual spin values.

aveva mostrato l'impossibilit  di completare la meccanica quantistica in termini causali e locali proposto da Einstein. Nel 1952, tuttavia, David Bohm pubblica *A Suggested Interpretation of the Quantum Theory in Terms of Hidden Variables* in cui mostra invece possibile tale completamento della Meccanica Quantistica e riformula i termini del paradosso. E' proprio la lettura del lavoro di Bohm che colpisce l'interesse di John Bell" (Laurora 2014, XII).

⁴⁶⁸ Some impressive works (Freire 2004, Freire 2014, for instance) dealt with David Bohm's key role in directing towards concrete experiments the conclusion reached by EPR. The data of the former are: Freire Jr. O. (2004), "The Historical Roots of 'Foundations of Quantum Physics' as a Field of Research (1950–1970)", *Foundations of Physics*, 34 (11), pagg. 1741-1760.

⁴⁶⁹ For the sake of exhaustiveness, it should be stated that Grete Hermann in the mid-1930s was the first to show a mistake in von Neuman theorem, later resolved by Bell. But her work was completely unnoticed.

⁴⁷⁰ This latter approach was crucial for Bell. On reading Bohm's two papers (published in 1952), Bell said that he "saw the impossible done".

As there is no interaction, you can get information about every single spin component of an atom without disturbing it. Therefore, for the EPR reality criterion, the spin components are elements of reality, one for each direction.

As reported in Laurora 2014, Bohm thought that:

[...] the experimenter is always free to redirect the apparatus in an arbitrary direction while the atoms are still *in flight*, and therefore to obtain a well defined, though not predictable, value of the spin component in any direction chosen by the experimenter. Since this can be done without in any way disturbing the second atom, we conclude that, if the reality criterion is applicable, precise defined elements of reality must exist simultaneously in the second atom, in correspondence of all three spin components. Since the wave function can specify, at most, only one of these components at a time with complete precision, we are therefore led to conclude that the wave function does not provide a complete description of all the elements of reality existing in the second atom.⁴⁷¹ (*Italics in the original text*).

Bohm introduced the element of spin and the reorientation of the measuring apparatus while the atoms are in flight, two variations with respect to EPR which were fundamental for future experiments. Unlike EPR, Bohm predicted a non-local theory, even if Hidden Variables re-emerged as a fundamental aspect for the completion of the theory, where instead EPR hoped for a new theory without making any hypothesis, also for Einstein's probable non-adherence to HV.

Bohm's contribution to EPR and to physics in general was really impressive and Selleri, who also experienced a correspondence with him, showed his great appreciation in the interview with Freire Jr., mainly for the role Bohm played in supporting the causal approach against the Copenhagen interpretation:

⁴⁷¹ “[...] lo sperimentatore è sempre libero di riorientare l'apparato in una direzione arbitraria mentre gli atomi sono ancora *in volo*, e quindi di ottenere un ben definito, anche se non prevedibile, valore della componente di spin in una qualsiasi direzione scelta dallo sperimentatore. Poiché questo può essere effettuato senza disturbare in alcun modo il secondo atomo, concludiamo che, se il criterio di realtà è applicabile, nel secondo atomo devono esistere simultaneamente precisi elementi definiti di realtà, in corrispondenza di tutte e tre le componenti dello spin. Poiché la funzione d'onda può specificare, al massimo, solo una di queste componenti alla volta con precisione completa, siamo quindi portati a concludere che la funzione d'onda non fornisce una descrizione completa di tutti gli elementi di realtà esistenti nel secondo atomo (Laurora 2014, pagg. 15-16).

SELLERI:

I have a very high opinion of David Bohm, because he was a sort of a hero as he was alone in defending the causal outlook in the early fifties.

In the late '40s and the early '50s (the hardest times) there was only David Bohm and nobody else. Not even Louis de Broglie, because you know that de Broglie was basically converted to the orthodox way of thinking from 1928 to 1952.

So I think he was a very courageous and very honest and a great physicist. His papers in *Physical Review* are very nice (AIP 2003).

Then Selleri highlighted the importance of 1957 joint paper written by Bohm and Aharonov in the struggle for the right approach to EPR paradox, even though he stressed how Bohm decided for the non-locality in the end:

SELLERI:

The turning point in my opinion for David Bohm was a paper in 1957 if I remember well in collaboration with Aharonov, but not the paper on the Bohm-Aharonov effect, not that one.

FREIRE:

It's a paper in which it is discussed what kind of experiments could give information about the EPR paradox.

SELLERI:

Yeah, yeah, yeah, exactly, exactly⁴⁷².

FREIRE:

They analyzed the experimental data maybe obtained by Madame Wu.

SELLERI:

Yes, yes. Exactly that. Exactly that. Because in that moment he understood that the experiments were not compatible with what we now call the factorable quantum mechanics. You know, there are three different approaches: (1) there is orthodox quantum mechanics, (2) there is a mutilated quantum mechanics in which all state vectors of correlated systems become factorable, (3) there is the causal approach.

Some people call point (2) above Furry theorem or Bohm-Aharonov theorem. And then with (3) you have of course a completely different approach in which quantum mechanics is reinterpreted as much as possible according to causality. Now the second line was the one that obviously Bohm was hoping to develop, and when he found that it was not possible because it was not compatible with experiments, he gave up basically and accepted non locality - which was a little bit of a tragedy. Of course he had the right to do what he wanted.

⁴⁷² Bohm D., Aharonov Y. (1957), "Discussion of Experimental Proof for the Paradox of Einstein, Rosen, and Podolsky", *Physical Review*, 108 (4), 1070-1076.

He had done so much in the field that I am very ready to forgive him, if it means anything. But it is a pity, because nobody else really defended local realism to the very end. In my opinion Louis de Broglie did not understand very well the problem, so he did nothing on this. And Bohm gave up. d’Espagnat was against, so really there was a time before 1970 in which research favorable to local realism was not at all carried out (AIP 2003. My underline).

Selleri’s statement is another confirmation of the crucial status which locality has always played for him.

Concerning the role of David Bohm, Laurora stated in his thesis that:

The contribution of Bohm’s research to the discussion raised by the paradox of Einstein, Podolsky and Rosen was to formulate the hypothetical experiment proposed by the authors in a form that would later stimulate the researchers’ reflection to place on an experimental level a topic that had been confined to a pure philosophical discussion; moreover, he had reopened the research on the question of hidden variables as a way to complete the description of physical reality given by the wave function, a suggestion that would be welcomed by John Bell⁴⁷³.

As told before, Bohm’s research was seminal for John Stewart Bell. On reading Bohm’s two papers (published in 1952), Bell said that he “saw the impossible done”.

Since his stay at University, Bell did not appreciate the fact that the QM courses did not teach reflections on the foundations but that only mathematical problems could be solved. Even though QM was just a hobby for him, within his work at CERN on accelerators, Bell wrote two pivotal papers, “On the problem of hidden variables in quantum mechanics” (written before but released after) and “On the Einstein-Podolski-Rosen paradox”⁴⁷⁴.

⁴⁷³ “Il contributo delle ricerche di Bohm alla discussione sollevata dal paradosso di Einstein, Podolsky e Rosen fu quello di formulare l’esperimento ipotetico proposto dagli autori in una forma che in seguito avrebbe stimolato la riflessione dei ricercatori a porre su un piano sperimentale un argomento che era stato confinato a una pura discussione filosofica; inoltre egli aveva riaperto la ricerca sulla questione delle variabili nascoste come modo per completare la descrizione della realtà fisica data dalla funzione d’onda, un suggerimento che sarebbe stato accolto da John Bell” (Laurora 2014, pag. 20).

⁴⁷⁴ Bell J.S. (1966), “On the problem of hidden variables in quantum mechanics”, *Reviews of Modern Physics* 38 (3), pagg. 447-452. (Bell 1966) in the following. Written and sent out as early as 1964, was only published in 1966. For a detailed information on the delay in the publication, see (Jammer 1974), pag. 303, and (Laurora 2014, pagg. 40-41).

Bell J.S. (1964), “On the Einstein-Podolski-Rosen paradox”, *Physica Physique Fizika*, 1 (3), pagg. 195-200. (Bell 1964) in the following.

In the former, he asserted:

The demonstrations of von Neumann and others, that quantum mechanics does not permit a hidden variable interpretation, are reconsidered. It is shown that their essential axioms are unreasonable. It is urged that in further examination of this problem an interesting axiom would be that mutually distant systems are independent of one another (Bell 1966, pag. 447).

In the latter, Bell argued in the Introduction:

The paradox of Einstein, Podolsky and Rosen was advanced as an argument that quantum mechanics could not be a complete theory but should be supplemented by additional variables. These additional variables were to restore to the theory causality and locality [2]. In this note that idea will be formulated mathematically and shown to be incompatible with the statistical predictions of quantum mechanics. It is the requirement of locality, or more precisely that the result of a measurement on one system be unaffected by operations on a distant system with which it has interacted in the past, that creates the essential difficulty. There have been attempts [3] to show that even without such a separability or locality requirement no “hidden variable” interpretation of quantum mechanics is possible. These attempts have been examined elsewhere [4] and found wanting.

Moreover, a hidden variable interpretation of elementary quantum theory [5] has been explicitly constructed. That particular interpretation has indeed a grossly nonlocal structure. This is characteristic, according to the result to be proved here, of any such theory which reproduces exactly the quantum mechanical predictions (Bell 1964, pag. 195).

It is the seminal Bell’s Theorem: any theory with HV whose predictions are in accordance with the QM shall be non-local. Or, similarly, a local HV theory cannot reproduce all statistical predictions of QM. Quite impressive were also the references Bell made on this excerpt:

[2] “But on one supposition we should, in my opinion, absolutely hold fast: the real factual situation of the system S_2 is independent of what is done with the system S_1 , which is spatially separated from the former.” A. EINSTEIN in *Albert Einstein*,

Philosopher Scientist, (Edited by P. A. SCHILP⁴⁷⁵) p. 85, Library of Living Philosophers, Evanston, Illinois (1949);

[3] J. VON NEUMANN, *Mathematische Grundlagen der Quanten-mechanik*. Verlag Julius-Springer, Berlin (1932), [English translation: Princeton University Press (1955)]; J.M. JAUCH and C. PIRON, *Helv. Phys. Acta* 36, 827 (1963).

[4] J. S. BELL, to be published

[5] D. BOHM, *Phys. Rev.* 85, 166 and 180 (1952).

The note n. 2 was the famous definition of Einstein's locality; the note n. 4 contained Bell's first paper, not yet published at this time, in which he showed the unreasonableness of von Neumann's axioms; the note n. 5 was the Bohm paper that Bell appreciated a lot, basically his starting point.

Bell managed to introduce his pivotal theorem to discriminate between the two opposing philosophical worldviews, namely Einstein's and Copenhagen orthodox QM interpretations, respectively.

According to Laurora, Bell's work became remarkable only after five years, when Clauser, Horne, Shimony and Holt made a joint proposal for an experimental test:

[...] After some mild reactions to his pioneering work in 1964, there was a renewed interest in FQM research following the publication in 1969 of the paper by Clauser, Horne, Shimony and Holt on the proposed experimental tests to verify Bell's inequality, and the contributions and discussion at the Varenna Conference in 1970⁴⁷⁶.

Indeed, the paper "On the Einstein-Podolski-Rosen paradox", although published on a journal that shut down after four years (1964-1968), became a topic that oriented the research on FQM and currently has 14460 citations, thus corroborating its absolute value; whereas "On the problem of hidden variables in quantum mechanics" currently has 3118 citations⁴⁷⁷.

⁴⁷⁵ The right surname is Schilpp, but of course I transcribed the original note by Bell himself.

⁴⁷⁶ "[...] Dopo alcune tiepide reazioni al suo pionieristico lavoro del 1964, si ha un rinnovato interesse verso la ricerca sui FQM a seguito della pubblicazione nel 1969 dell'articolo di Clauser, Horne, Shimony e Holt sulla proposta di test sperimentali per la verifica della disegualianza di Bell, e dei contributi e della discussione al Convegno di Varenna nel 1970" (Laurora 2014, pag. xii-xiii).

⁴⁷⁷ Both indexes of citation are referred to Google Scholar. Accessed 24 September 2020.

In his original work, Bell recalled the EPR as described by Bohm and Aharonov and introduced the Einstein locality:

With the example advocated by Bohm and Aharonov, the EPR argument is the following. Consider a pair of spin one-half particles formed somehow in the singlet spin state and moving freely in opposite directions. Measurements can be made, say by Stern-Gerlach magnets, on selected components of the spins σ_1 and σ_2 . If measurement of the component $\sigma_1 \cdot \mathbf{a}$, where $\mathbf{a}$ is some unit vector, yields the value + 1 then, according to quantum mechanics, measurement of $\sigma_2 \cdot \mathbf{a}$ must yield the value -1 and vice versa. Now we make the hypothesis [of Einstein], and it seems one at least worth considering, that if the two measurements are made at places remote from one another the orientation of one magnet does not influence the result obtained with the other. Since we can predict in advance the result of measuring any chosen component of σ_2 , by previously measuring the same component of σ_1 , it follows that the result of any such measurement must actually be predetermined. Since the initial quantum mechanical wave function does *not* determine the result of an individual measurement, this predetermination implies the possibility of a more complete specification of the state (Bell 1964, pag. 195. *Italics in the original text*).

In the paper conclusion, Bell stressed the importance of the changements of settings during experiments:

In a theory in which parameters are added to quantum mechanics to determine the results of individual measurements, without changing the statistical predictions, there must be a mechanism whereby the setting of one measuring device can influence the reading of another instrument, however remote. Moreover, the signal involved must propagate instantaneously, so that such a theory could not be Lorentz invariant. Of course, the situation is different if the quantum mechanical predictions are of limited validity. Conceivably they might apply only to experiments in which the settings of the instruments are made sufficiently in advance to allow them to reach some mutual rapport by exchange of signals with velocity less than or equal to that of light. In that connection, experiments of the type proposed by Bohm and Aharonov in which the settings are changed during the flight of the particles, are crucial (Bell 1964, pag. 199).

Bell, at 1970 Varenna Summer School, played a very impressive lecture (*Introduction to Hidden-Variable Question*)⁴⁷⁸ in which he recalled some

⁴⁷⁸ Bell J.S. (1971), "Introduction to the hidden-variable question", in D'Espagnat B. (Ed), Società Italiana di Fisica, *Foundations of Quantum Mechanics. Proceedings of the International School of Physics "Enrico*

statements by his paper and yet also enhanced them, proposing some experiments. At the beginning he stated on the provisional nature of QM:

[...] nobody knows just where the boundary between the classical and quantum domain is situated. [...] the movability of the boundary is of only approximate validity; demonstrations of it depend on neglecting numbers which are small, but not zero, which might tend to zero for infinitely large systems, but are only very small for real finite systems. A theory founded [...] on arguments of manifestly approximate character, however good the approximation, is surely of provisional nature. It seems legitimate to speculate on how the theory might evolve. But of course no one is obliged to join in such speculation (Bell 1971, pag. 171-172).

Concerning the measurements on the selected components of the spin, already described in 1964, Bell reinforced the idea of Hidden Variables and the locality, and in addition proposed an experimental road. He stated:

[...] It is this possibility, of a homogeneous account of the world, which is for me the chief motivation of the study of the so-called "hidden variable" possibility.

[...] Once the incompleteness of the wave-function description is suspected, it can be conjectured that the seemingly random statistical fluctuations are determined by the extra "hidden" variables - "hidden" because at this stage we can only conjecture their existence and certainly cannot control them.

[another] motivation is in the peculiar character of some quantum-mechanical predictions, which seem almost to cry out for a hidden variable interpretation. This is the famous argument of EINSTEIN, PODOLSKI and ROSEN.

[Concerning the measurements on the selected components of spin] this strongly suggests that the outcomes of such measurements, along arbitrary directions, are actually determined in advance, by variables over which we have no control, but which are sufficiently revealed by the first measurement so that we can anticipate the result of the second. There need then be no temptation to regard the performance of one measurement as a causal influence on the result of the second, distant, measurement. The description of the situation could be manifestly "local". This idea seems at least to merit investigation.

We will find, in fact, that no local deterministic hidden-variable theory can reproduce all the experimental prediction of quantum mechanics. This opens the possibility of bringing the question into the experimental domain, by trying to approximate as well as possible the idealized situations in which local hidden variables and quantum mechanics cannot

agree. However, before coming to this, we must clear the ground by some remarks on various mathematical investigations that have been made on the possibility of hidden variables in quantum mechanics without any reference to locality (Bell 1971, pagg. 172-173. My underline).

Bell at Varenna spoke very explicitly, giving a huge amount of interesting hints. His talk opened a lot of activities and researches and surely was a turning point for the consideration that people gave to his work.

Bell recalled Bohm-de Broglie wave mechanics, introducing an additional (hidden) variable λ , a single three-vector, then he explained the difficulties for locality in a three-space, just arriving to the conclusion that the quantum-mechanical result cannot be reproduced by a HV theory which is local.

But always trying to find a solution, because maybe he would have appreciated a new theory better than QM and possibly local. And he underscored again the importance of being experiments. But, he already thought to ideal experiments, sure that the departures from an ideal situation would have permitted to QM to violate the inequality. Selleri debated on this for his whole life, always explaining the difference between ideal efficiency and the real situation, creating weak and strong inequalities. Bell also proposed to work with photons or kaons systems, more promising than two spin $\frac{1}{2}$ particles. And, to sum up an outstanding talk, Bell gave a “prophecy” on the future attitude that physics met with the three generation of experiments on Bell’s inequality. Namely, the strong discouragement after the confirmation of QM predictions against the HV theories ones.

Bell argued:

[...] the behaviour of a given variable λ_1 is determined not only by the conditions in the immediate neighbourhood (in ordinary three-space) but also by what is happening at all the other positions $\lambda_2 \lambda_3, \dots$. That is to say, that although the system of equations is “local” in an obvious sense in the $3n$ -dimensional space, it is not at all local in ordinary three-space.

As applied to the Einstein-Podolski-Rosen situation, we find that this scheme provides an explicit causal mechanism by which operations on one of the two measuring devices can influence the response of the distant device⁴⁷⁹. This is quite the reverse of the resolution hoped for by EPR, who envisaged that the first device could serve only to reveal the character of the information already stored in space, and propagating in an undisturbed way towards the other equipment.

The question then arises: can we not find another hidden-variable scheme with the desired local character? It can be shown that this is not possible [7-9]⁴⁸⁰. The demonstration moreover is in no way restricted to the context of nonrelativistic wave mechanics, but depends only on the existence of separated systems highly correlated with respect to quantities such as spin (Bell 1971, pag. 178. My underline).

[...] the quantum-mechanical result cannot be reproduced by a hidden variable theory which is local [...].

This result opens up the possibility of bringing the questions that we have been considering into the experimental area. [...] the situation envisaged is highly idealized. It is supposed that the system is initially in a known spin state, that the particles are known to proceed towards the instruments, and to be measured there with complete efficiency.

The question then is whether the inevitable departures from this ideal situation can be kept sufficiently small in practice that the quantum-mechanical prediction still violates the inequality $|P(a, b) - P(a, b')| + |P(a', b) + P(a', b')| \leq 2$ [where a, b are directions where devices measure spin components; and a', b' are alternative settings of the instruments].

[...] In this connection other systems, for example the two-photon system or the two-kaon system [13], may be more promising than that of two spin $\frac{1}{2}$ particles. A very serious study of the photon case will be reported to this meeting by Shimony. The experiment described by him, and now under way, is not sufficiently close to the ideal to be conclusive for a quite determined advocate of hidden variables. However, for most a confirmation of the quantum-mechanical predictions, which is only to be expected given the general success of quantum mechanics, would be a severe discouragement (Bell 1971, pag. 180. My underline).

Surely a veritable goldmine of cues. Selleri found a huge field to try enhancing his ideas. The reference [13] in Bell's paper sounded very remarkable for the 1983 (and further) Selleri's proposal on an experiment

⁴⁷⁹ i.e., *entanglement*. Causality without locality.

⁴⁸⁰ Three notes in the original text: [7] J.S. BELL: Physics, 1, 195 (1964); [8] J.F. CLAUSER, M.A. HORNE, A. SHIMONY and R.A. HOLT: Phys. Rev. Lett., 26, 880 (1969); [9] E.P. WIGNER: Am. Journ. Of Phys, 38, 1005 (1970).

with K_0 and $K_{0\text{bar}}$ at ϕ -factory, still showing two authors that already in 1961 started studying kaons decay:

[13] T.B. DAY: Phys. Rev., 121, 1204 (1961); D.R. INGLIS: Rev. Mod. Phys., 33, 1 (1961). Note that the spontaneous decay times of the two kaons, because they cannot be set at the will of the experimenter, are not to be regarded as analogous to the setting a and b of the Stern-Gerlach magnets. The thicknesses of a pair of slabs of matter placed in the lines of flight would be more relevant. I am told by Prof. B. d'ESPAGNAT that the rapid decay of the short-lived kaon is a major obstacle to devising a critical experiment (Bell 1971, pag. 181).

After 1970 Varenna Summer School, Bell's work was widely acknowledged and today is accepted as maybe the most influential contribution on the debate on FQM in the twentieth century.

One of the main consequences of his research lied in the opportunity to make concrete physics in laboratories, being now the EPR not only a philosophical issue. A lot of physicists started finding interest for the matter. A good summary of Bell's work is given in Kumar:

Bell's stroke of genius was to discover a way out of the impasse by changing the relative orientation of the two spin detectors. If the spin detectors measuring electrons A and B are aligned so that they are parallel, then there is a 100 per cent correlation between the two sets of measurements – whenever spin-up is measured by one detector, spin-down is recorded by the other and vice versa. If one of the detectors is rotated slightly, then they are no longer perfectly aligned. Now if the spin states of many pairs of entangled electrons are measured, when A is found to be spin-up, the corresponding measurement of its partner B will sometimes also be spin-up. Increasing the angle of orientation between the detectors results in a reduction in the degree of correlation. If the detectors are at 90 degrees to each other and the experiment is once again repeated many times, when A is measured along the x-direction as spin-up, only in half of these instances will B be detected as spin-down. If the detectors are orientated at 180 degrees to one another, then the pair of electrons will be completely anti-correlated. If A's spin state is measured as spin-up, then B's will also be spin-up. Although a thought experiment, it was possible to calculate the exact degree of spin correlation for a given orientation of the detectors predicted by quantum mechanics. However, it was not possible to do a similar calculation using an archetypal hidden variables theory that preserved locality.

The only thing that such a theory would predict was a less than perfect match between spin states of A and B. This was not enough to decide between quantum mechanics and a local hidden variables theory. Bell knew that any actual experiment that found spin correlations in line with the predictions of quantum mechanics could easily be disputed. After all, it was possible that in the future someone might develop a hidden variables theory that also exactly predicted the spin correlations for different orientations of the detectors. Bell then made an astonishing discovery. It was possible to decide between the predictions of quantum mechanics and any local hidden variables theory by measuring the correlations of pairs of electrons for a given setting of the spin detectors and then repeating the experiment with a different orientation. This enabled Bell to calculate the total correlation for both sets of orientations in terms of the individual results predicted by any local hidden variables theory. Since in any such theory the outcome of a measurement at one detector cannot be affected by what is measured at the other, it is possible to distinguish between hidden variables and quantum mechanics. Bell was able to calculate the limits on the degree of spin correlation between pairs of entangled electrons in a Bohm-modified EPR experiment. He found that in the ethereal realm of the quantum there is a greater level of correlation if quantum mechanics reigns supreme than in any world that depends on hidden variables and locality. Bell's theorem said that no local hidden variables theory could reproduce the same set of correlations as quantum mechanics. Any local hidden variables theory would lead to spin correlations that generated numbers, called the correlation coefficients, between -2 and $+2$. For certain orientations of the spin detectors, quantum mechanics predicted correlation coefficients that lay outside of the range known as 'Bell's inequality' that ran from -2 to $+2$ (Kumar 2009, pagg. 344-346. My underline).

[...] Bell derived the inequality from just two assumptions. First, there exists an observer-independent reality. This translates into a particle having a well-defined property such as spin before it is measured. Second, locality is preserved. There is no faster-than-light influence, so that what happens here cannot possibly instantaneously affect what happens way over there. Aspect's results mean that one of these two assumptions has to be given up, but which one? Bell was prepared to give up locality. 'One wants to be able to take a realistic view of the world, to talk about the world as if it is really there, even when it is not being observed', he said (Kumar 2009, pag. 350. My underline).

On the Bell's activity and on his attempts on preserving locality, Kumar stated:

Bell decided to find out if non-locality was a peculiar feature of Bohm's model or if it was a characteristic of any hidden variable theory that aimed to reproduce the results of quantum mechanics. "I knew, of course, that the Einstein-Podolski-Rosen setup was the critical one, because it led to distant correlations", he explained. "They ended their paper by stating that if you somehow completed the quantum mechanical description, non-locality would only be apparent. The underlying theory would be local".

Bell started out trying to preserve locality by attempting to construct a 'local' hidden variable theory in which if one event caused another, then there had to be enough time between the two to allow a signal travelling at the speed of light to pass between them. "Everything I tried didn't work", he said later. "I began to feel that it very likely couldn't be done". In his attempt to eliminate what Einstein decried as "spooky actions at a distance", non-local influences that were transmitted instantly between one place and another, Bell derived his celebrated theorem (Kumar 2009, pagg. 341-342)

The outstanding role played by Bell and his theorem for the acceptance by physicists of an argument otherwise considered merely philosophical is always well underlined by Kumar:

[...] For the vast majority of Bell's colleagues, young and old alike, the dispute between Einstein and Bohr over the correct interpretation of quantum mechanics was more philosophy than physics.

[...] Bell's theorem changed that. It allowed the local reality advocated by Einstein, that the quantum world exists independently of observation and that physical effects cannot be transmitted faster than the speed of light, to be tested against Bohr's Copenhagen interpretation. Bell had brought the Einstein-Bohr debate into a new arena, experimental philosophy.

If Bell's inequality held, then Einstein's contention that quantum mechanics was incomplete would be right. However, should the inequality be violated, then Bohr would emerge the victor. No more thought experiments; it was Einstein vs. Bohr in the laboratory (Kumar 2009, pagg. 346-347. My underline).

A good conclusion, linking the Bell's work with the next experiments, came from Laurora. Already in 1964 Bell suggests an experiment that will be actually carried out by Aspect, Dalibard and Roger in 1982:

[Bell] attention focused on the topic that interested him most: the non locality of Bohm's theory. As a supporter of Einstein's position, he decided to resume the EPR experiment in Bohm's version, believing that he could contribute to progress in research on the Foundations of Quantum Mechanics if he managed to rigorously establish a local theory

with hidden variables that reproduced quantum correlations. [...] In his reasoning Bell takes Bohm's version of the EPR argument. [...] However, he examines a more general experiment than the one considered in the EPR argument and also in Bohm: analysing the measurements of spin components along arbitrary directions (and not only the components along the z-axis or the x-axis, as had been done until then). [...] Then Bell had the idea to rotate the [Stern-Gerlach's] magnets so that the magnetic fields formed a certain angle between them (or even to keep the measurement direction constant on one side of the apparatus and vary the direction on the other side). It was a fairly simple idea, but until then no one had thought of it. He assumes that "if the two measurements are made in places distant from each other, the orientation of one magnet does not influence the result obtained with the other", thus admitting the hypothesis of locality. Then he observes that, since it is possible to predict the result of any spin component of particle 2 by measuring the same spin component of particle 1, we can conclude that the result of such a measurement is predetermined and that "this predetermination implies the possibility of a more complete specification of the state". This [is] the existence of hidden variables, is hypothesized and moreover realized by means of λ parameters, which can indicate a single variable or a set of variables, be discrete or continuous. [...] He then reaches his inequality: $1 + P(b,c) \geq |P(a,b) - P(a,c)|$.

[...] Bell shows therefore how the quantum expectation value does not satisfy this inequality, derived from the hypothesis of local realism, thus demonstrating the result: the incompatibility of a theory with hidden variables with the quantum mechanics predictions.

[...] At the end of his work, Bell emphasizes the opportunity that Quantum Mechanics is of limited validity, whose predictions can be applied "only to those experiments in which the preparations of the instruments are made sufficiently in advance, so as to allow them to reach some reciprocal relationship by means of an exchange of signals having a speed less than or equal to that of light. In this sense, the experiments of the type proposed by Bohm and Aharonov (1957), in which the preparations of the experimental apparatus are modified during the flight time of the particles, are fundamental"⁴⁸¹ (Laurora 2014, pagg. 42-46).

2.7.4-BELL-TYPE EXPERIMENTS: THREE GENERATIONS AND MORE

We can distinguish at least three different generations of experiments on Bell's inequality, strictly related also to peculiar experimental devices and techniques.

⁴⁸¹ The Italian original text is in Addendum 3, p. 994.

As there is also an excellent and exhaustive bibliography (Kumar 2009, Laurora 2014, Freire 2014 but surely I could mention other numerous sources), I will only outline the different periods and the obtained outcomes. As early as 1964, Bell challenged the experimentalists to put his inequality to the test. As a theorist, he claimed that “it requires little imagination to envisage the measurements involved actually being made”. But creating an experiment is something more difficult and only after five years, John Clauser, Abner Shimony, Michael Horne, and Richard Holt, published a remarkable paper⁴⁸², Proposed Experiment to Test Local Hidden-Variable Theories⁴⁸³, which gathered the results they had found independently and which today, as often happens with really famous works, is always quoted with the initials of the authors’s surnames: CHSH 1969. In the abstract they argued that: “A theorem of Bell, proving that certain predictions of quantum mechanics are inconsistent with the entire family of local hidden variable theories, is generalized so as to apply to realizable experiments.

A proposed extension of the experiment of Kocher and Commins, on the polarization correlation of a pair of optical photons, will provide a decisive test between quantum mechanics and local hidden-variable theories” (CHSH 1969, pag. 549).

In (Laurora 2014, pages 49-55) the testimonies by Shimony and Clauser on the origin of the paper are reported. Reading them it is clear how their collaboration produced a profound analysis of the state-of-the-art and how their theoretical and experimental skills, together with the desire to solve the long-standing problems inherent in the QM, produced the first real proposal of experiment, made three years later by Clauser and Freedman.

Shimony at Boston University proposed to Horne the reading of Bell’s paper and the latter started to examine the literature on the production of correlated

⁴⁸² currently it has received 7467 citations, according to Google Scholar. Accessed 24 September 2020.

⁴⁸³ Clauser J.F., Horne M.A., Shimony A., Holt R.A. (1969), “Proposed Experiment to Test Local Hidden-Variable Theories”, *Physical Review Letters* 23, 880, Published 13 October 1969; Erratum *Physical Review Letters* 24, 549 (1970). (CHSH 1969) in the following.

linear polarizations of photon pairs, in particular of pairs produced in cascade and studied by Kocher and Commins. Both being theorists and not experimentalist, they contacted Pipkin and Holt at Harvard.

Laurora had a correspondence with Shimony and the recollections of the latter are here recalled⁴⁸⁴:

[...] some friends advised us to consult Pipkin at Harvard, who had performed experiments on photon correlation. One of his students, Richard Holt, understood the meaning of the conflict between Quantum Mechanics and the locality condition of special relativity for photon pairs produced in cascade and agreed to perform an experiment to see if the correlations verified Bell's inequality (as special relativity predicted) or if the experiment revealed the quantum-mechanical correlations⁴⁸⁵.

Clauser, at Columbia University was the first to introduce theoretically an experiment in the 1969 spring in a meeting to Washington. Always according to Shimony recollections:

I called Clauser at Columbia University to tell him about my and Mike's work, and suggested that we publish a detailed article together. It was not by chance that Clauser was not interested, because of the priority of his communication. However, when he heard that Holt had the apparatus to perform the experiment and was planning to do so, he agreed to join Horne, Holt and me for a joint article, as he deeply desired to be part of the group that could perform the first experimental test on Bell's inequality. The four of us wrote a joint article in the summer of 1968 under unusual circumstances: Clauser wrote his contribution on board his sailboat on the way from New York to Florida, while Horne, Holt and I wrote our part in Boston. [...] The article was sent as a reprint or preprint to Bell. This work proposed the experimentation of Bell's hypothesis of locality in opposition to Quantum Mechanics, and he was enthusiastic about it. It was the first communication between John Bell and our group, and Bell encouraged us to perform the experiment we had proposed, which was later carried out by Clauser and his student Freedman in California⁴⁸⁶.

⁴⁸⁴ "Bell's Theorem", Shimony e-mail to Laurora, 2011. (Shimony 2011) in the following.

⁴⁸⁵ "[...] alcuni amici ci consigliarono di consultare Pipkin ad Harvard, che aveva eseguito esperimenti sulla correlazione di fotoni. Uno dei suoi studenti, Richard Holt comprese il significato del conflitto tra la Meccanica Quantistica e la condizione di località della relatività speciale per coppie di fotoni prodotti in cascata e fu d'accordo a eseguire un esperimento per vedere se le correlazioni verificavano la Diseguaglianza di Bell (come prevedeva la relatività speciale) oppure se l'esperimento rivelava le correlazioni quanto-meccaniche" (Shimony 2011, cit. in Laurora 2014, pag. 50).

⁴⁸⁶ "Chiamai Clauser alla Columbia University per dirgli del lavoro mio e di Mike e suggerendogli di pubblicare insieme un articolo dettagliato. Non a caso Clauser non fu interessato, a causa della priorità della sua

Therefore, already in 1968 they joined forces to be the first to test Bell's inequality in the laboratory, confirming in practice how in a few years after publication and even before Varenna 1970, Bell's work was known and ready to be tested in the laboratory.

Clauser recollections⁴⁸⁷ are really valuable, also for his analysis of the state-of-the-art and his narration of the genesis of the experiment. He searched for some paper with experimental outcomes, finding only the 1950 Wu and Shakhnovich experiment with the annihilation of positronium. Then, he studied a problem in 1964 Bell's outcome, that is, its real application only in extreme conditions, namely, an idealized configuration, hardly to be verified experimentally. Then Clauser declared to have found a tested result:

[...] once I comprehended what the 1964 [Bell] paper actually said, I was astounded by its result. However, I was not yet willing to accept the paper's far-reaching implications until I finally saw some experimental evidence that decided between its two significantly different predictions. Since Bell's paper is conspicuously vague on the experimental status of its prediction (but crystal clear on everything else), I suspected that Bell was bluffing and started to search through the literature for experimental results. I found none, other than that by Wu and Shakhnovich.

With regard to experimental testing, I saw at least one serious problem with Bell's 1964 result. That problem is that Bell's form for his result applies *only* to his idealized configuration, and thus cannot be directly tested experimentally. Bell (being a theorist) assumes an ideal apparatus and an ideal two-particle entangled-state preparation. However, his proof then relies on this idealization, and further uses it to show that a perfect correlation between the two particle's polarizations occurs for at least one setting of the polarization analyzers.

comunicazione. Tuttavia, quando seppe che Holt aveva l'apparato per eseguire l'esperimento e stava progettando di realizzarlo, egli fu d'accordo a unirsi a Horne, Holt e a me per un articolo congiunto, poichè egli desiderava profondamente far parte del gruppo che avrebbe potuto eseguire il primo test sperimentale della Diseguglianza di Bell. Noi quattro scrivemmo un articolo congiunto nell'estate del 1968 in circostanze inusuali: Clauser scrisse il suo contributo a bordo della sua barca a vela durante il percorso da New York in Florida, mentre Horne, Holt e io scrivemmo la nostra parte a Boston. [...] L'articolo fu inviato come reprint o preprint a Bell. Questo lavoro proponeva la sperimentazione dell'ipotesi di località di Bell in opposizione alla Meccanica Quantistica, ed egli fu entusiasta. Fu la prima comunicazione tra John Bell e il nostro gruppo, e Bell ci incoraggiò a eseguire l'esperimento che avevamo proposto, realizzato in seguito da Clauser e dal suo studente Freedman in California" (Shimony 2011, cit. in Laurora 2014, pagg. 50-51).

⁴⁸⁷ Clauser J.F. (2002), "Early History of Bell's Theorem", in *Quantum (Un)speakables: from Bell to Quantum Information*, Bertlmann R.A., Zeilinger A. (eds.), Springer, pagg. 61-98. (Clauser 2002 in the following).

Unfortunately, even an infinitesimal departure by the considered system from this idealized behavior then invalidates the applicability of this proof to it, since then the required single point with a maximal correlation no longer exists. Correspondingly, his result then applies only to ideal systems and not to real ones! (Damn theorists!) Obviously, in any real experiment, Bell's required perfect correlation cannot and will not obtain.

[...] I thus asked myself whether or not Bell's assumption of an idealized apparatus and system is, in fact, necessary for an alternative proof of his Theorem to obtain. Looking further, I found that this assumption can indeed be avoided. Thus, I succeeded in deriving a new more general form for his result that then *can* be tested experimentally (Clauser 2002, pagg. 78-79. *Italics* in the original text).

It is worth underlining how Selleri, however, will always pointed out how the several additional assumptions presented in almost all Bell-type experiments prevented them of being resolved against the locality and the HV and in favour of QM. In Selleri's thought, the locality loophole remained always open and unresolved yet. And the need of ideal apparatus (or even, of a polarizers efficiencies better than the 10% one present in the experiments) was not an assumption but rather a real need and should resolve Bell's inequalities in favour of HV and against QM. Experiments made at that time choose always additional assumption to be performed, almost violating Bell's inequalities and being in agreement with QM. But Selleri stated how the use of additional assumption violated only the so called strong Bell's inequalities. Going back to Carl Kocher and Gene Commins, they measured the linear polarization correlation of photon pairs emitted in an atomic cascade, using a beam of calcium atoms. Laurora recalled that:

Kocher and Commins observed how [...] photons are emitted with wavelengths in the visible range, so it is possible to detect the correlation between polarizations by linear polarizers of the Polaroid type and usual photomultipliers.

[...] The reported results clearly indicate the difference between the counting rates for parallel and perpendicular orientations of the polarizers, [...] for which the correlation between the two photons is consistent with a correlation of the shape predicted by Quantum Mechanics⁴⁸⁸.

⁴⁸⁸ "Kocher and Commins osservarono come [...] i fotoni sono emessi con lunghezze d'onda nel range del
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Clauser discovered how the experiment made by Kocher and Commins was not adequate, still pointing out how the need of better polarizers at different angles was to be desired:

[...] contrary to Kocher's claim, I discovered that their experiment was totally inadequate as a test. Kocher and Commins had only measured coincidences at 0° and 90° relative analyzer orientation. As per Bell's assumption, these data points are located exactly where the two theories can give the same prediction. Also, as per my own generalization of Bell's result to provide an actual experimental prediction, I noted that their polarizer efficiencies were nowhere high enough for the experiment to be conclusive, in any case. Thus, their experiment provides no data at all for a test of hidden-variable theories. What is clearly needed at that point are data taken at angles intermediate between their measured point with an experiment that is similar to theirs, but that uses much better polarizers (Clauser 2002, pag. 79).

Clauser recollected also Bell's enthusiastic reaction to his proposal of repeating Kocher-Commins experiment with improved polarizers at intermediate angles. And described the steps with his coauthors, which led to the publication of the above CHSH paper and inequality.

Now I am going to introduce the Bell-type experiments, starting by CHSH and its result.

The three generations were linked to a diverse technological development in the devices necessary to carry out the experiments, in addition to the different solutions found to try to overcome the loopholes of the locality and the efficiency of the detectors.

The first generation of Bell experiments, according to a chronological arrangement made in (Laurora 2014), went from 1972 to 1976 and was basically grounded in atomic cascades. Actually, the 1969 CHSH paper was just the pathfinder, whereas the first real experiment made in a laboratory and ready to give outcomes belonged to Clauser and Freedman in 1972.

visibile, per cui è possibile rivelare la correlazione tra le polarizzazioni mediante polarizzatori lineari del tipo Polaroid e fotomoltiplicatori usuali. [...] I risultati riportati indicano chiaramente la differenza tra i rate di conteggi per le orientazioni parallele e perpendicolari dei polarizzatori, [...] per cui la correlazione tra i due fotoni è consistente con una correlazione della forma prevista dalla Meccanica Quantistica" (Laurora 2014, footnote n. 10, pag. 52).

According to (Laurora 2014, see pag. 55-60), CHSH generalized Bell's inequality by including real systems even if they did not use the P correlation probability formula.

The apparatuses, that have a polarizing filter and a photomultiplier, allow to see the counting rate. But the very low photoelectric efficiencies of photomultipliers prevent the experimental application of inequality.

The choice is between transmitting or not the photons in the filters. P is the correlation between photon transmission. To have an experimental prediction, the authors make an additional hypothesis, the so-called CHSH hypothesis, which Selleri introduced in this way:

given that a pair of photons emerge from two regions of space where two polarizers can be located, the probability of their joint detection from two photomultipliers is independent of the presence and the orientation of the polarizers (Selleri 1988, pag 259)⁴⁸⁹.

Therefore, they introduce four observation probabilities related to transmission and absorption by polarizers.

They thus obtain a formula using the detection rates of coincidences with both polarizers and with one polarizer inserted and one removed.

Also Freire gave a deep and impressive description of CHSH, emphasizing their results as well as stressing the three additional assumptions they made. In addition, Freire quoted how Selleri was already involved in the HV research:

The CHSH paper is a fine piece in physics literature both for its concision and breadth. [...] The authors fulfilled three goals. They modified Bell's theorem in order to make it usable for real, not idealized experiments, they showed that data available from previous experiments did not produce evidence against local hidden-variable theories, and they suggested a viable test with optical photons suggesting optics as the privileged bench for tests of Bell's theorem.

⁴⁸⁹ Selleri F. (1988), "Is Bell's Inequality Violated in Nature?", in *Problems in Quantum Physics; Gdańsk '87. Recent and Future Experiments and Interpretations. Gdańsk, Poland 21-25 September 1987*, Kostro L., Posiewnik A., Pykacz J., Żukowski M. (eds.), Singapore: World Scientific, pp. 256-268.

They showed that the experiment by Madame Chien-Shiung Wu and I. Shaknov, performed in 1950 using the annihilation of positronium for measuring polarization correlation of γ photons, was not adequate to test Bell's theorem because "the direction of Compton scattering of a photon is a statistically weak index of its linear polarization," and there are no good polarizers for high-energy photons. They also showed that the Kocher and Commins experiment measuring polarization correlation of photons emitted in an atomic decay cascade of calcium, performed in 1967, was in principle adequate for testing Bell's theorem but that these experimentalists had only measured angles 0° and 90° in which there is no conflict between quantum mechanical predictions and local hidden-variable theories. For this reason the proposed experiment was essentially a revision of the Kocher and Commins experiment with angles 22.5° and 67.5° , in which there is a maximum conflict of predictions.

[...] In order to make Bell's theorem testable, CHSH authors introduced [three] additional assumptions that were determined by the type of experiments they wanted to do. The three additional assumptions were related to: avoiding Bell's assumption of perfect correlation between the pair of particles; considering rates of emergence or not of photons from the filters instead of their detection (a change due to the small efficiencies of the available photoelectric detectors); and taking the probability of joint detection as being independent of the orientation of the polarization filters. They considered that the latter assumptions, which can be called a fair sampling assumption, "could be challenged by an advocate of hidden-variable theories in case the outcome of the proposed favors quantum mechanics;" but they did not assess them a flaw in the proposed experiment because "highly pathological detectors are required to convert hidden-variable emergence rates into quantum mechanical counting rates". As necessary as these assumptions were for the time we are analyzing, to perform experiments relaxing them has been the holy grail for physicists involved with foundations of quantum mechanics.

The CHSH paper awakened interest among Europeans physicists, such as Bell, Bernard d'Espagnat, de Broglie and Franco Selleri, who were already involved with hidden-variables theories (Freire 2014, pagg. 254-256. My underline).

In 1978⁴⁹⁰, Clauser and Shimony wrote a massive and impressive paper on this issue, deeply analyzing all experiments made so far, at least eleven diverse derivations. By the way, they quoted in their references the Selleri 1978 paper "On the Consequences of Einstein Locality"⁴⁹¹, as well as a preprint by L. Schiavulli which probably was the future joint paper written

⁴⁹⁰ Clauser J. F., Shimony A. (1978), "Bell's theorem. Experimental tests and implications", *Reports on Progress in Physics*, 41, pagg. 1881-1927. (Clauser, Shimony 1978) in the following.

⁴⁹¹ Selleri F. (1978), "On the Consequences of Einstein Locality", *Foundations of Physics*, 8 (1-2), pp. 103-116.

with Selleri that appeared in 1979 as “Further Consequences of Einstein Locality”⁴⁹².

Concerning CHSH, in the foregoing paper, Clauser and Shimony pointed out how:

First, they showed that his analysis can be extended to cover actual systems, and that experimental tests of this broad class of theories can be performed. Second, they introduced a very reasonable auxiliary assumption which allows tests to be performed with existing technology. Third, they specifically proposed performing such a test by examining the polarisations of photons produced by an atomic cascade, and derived the required conditions for such an experiment (Clauser, Shimony 1978, pagg. 1883-1884).

In the first phase of Bell experiments, four main works were introduced, and I summarized them below using Selleri’s explanation, took from two of his books, *La fisica del Novecento. Per un bilancio critico*, published in 1999⁴⁹³; and *La fisica tra paradossi e realtà*, published in 2001⁴⁹⁴:

- FREEDMAN and CLAUSER experiment, April 1972⁴⁹⁵.

In this experiment the radiation of an arc lamp hit a beam of calcium atoms and exciting part of it to the state $3d4p^1P_1$.

About 10% of these went to the lower level $4p^2\ ^1S_0$ which is the initial state of the cascade (0.1.0) which generates photons of 551.3 nm and 422.7 nm. On each side of the source, the photons are collected and collimated by a lens, then they affect a filter and a polarizer (one-way) and eventually reach a photomultiplier. They used approximately 1 m long polarizers consisting of ten parallel glass plates inclined at the Brewster angle. The impulses of the photomultipliers were sent to a coincidence circuit and coincidence measurements were made during time intervals of 100 s, alternating the intervals at which the plates were removed at intervals at which they were present. The results obtained by varying the relative directions of the optical axes of the polarizers were found in good agreement with the quantum-mechanical predictions, and in clear violation of the strong inequality⁴⁹⁶ (Selleri Fisica 900, 1999, pagg. 208-209; Selleri Paradossi, 2001, pag. 155).

⁴⁹² Schiavulli L., Selleri F. (1979), “Further Consequences of Einstein Locality”, *Foundations of Physics*, 9 (5-6), pp. 339-352.

⁴⁹³ (Selleri Fisica 900, 1999) in the following.

⁴⁹⁴ (Selleri Paradossi, 2001) in the following.

⁴⁹⁵ Freedman S.J., Clauser J.F. (1972), “Experimental test of local hidden-variable theories”, *Physical Review Letters*, 28 (14), pagg. 938-941.

⁴⁹⁶ The Italian original text is in Addendum 3, pp. 994-995.

- HOLT and PIPKIN experiment, 1973.

In this experiment, photons with 567.6 nm and 404.7 nm wavelengths emitted in the cascade (1.1.0) of the ^{198}Hg isotope of mercury with nuclear spin zero were observed. Since the final level of the cascade used is not the fundamental state of the atom, no precautions were necessary to avoid resonant trapping of photons, an effect that was observed in Freedman and Clauser's experiment and reported in Freedman's PhD thesis. To produce the photon pairs, mercury vapors were excited at level 9^1P_1 by a 100 eV electron beam. Calcite polarizers were used. These polarizers are significantly better than parallel plate polarizers [...]. The experimental result did not violate [the strong] inequality and disagreed with the quantum mechanics prediction. This discrepancy has never been explained, indeed there were those who attributed it to the use of (good) calcite polarizers⁴⁹⁷ (Selleri Fisica 900, 1999, pagg. 209-210. My underline).

In (Freire 2014) it is well analyzed the outcome of Holt and Pipkin and their lackness in repeating the experiment:

Holt and Pipkin did not trust their result, but they were not able to identify the source of errors. They circulated it as a preprint that was never published.[...] Pipkin only took it up again in 1976, when Clauser and Fry were announcing the new results [...]. Pipkin then stated, "a careful study was made of systematic effects which could account for the deviation from the quantum mechanical prediction but no candidates were found. In view of the result reported by Freedman and Clauser [...] it was concluded that the experiment should be repeated with a somewhat different configuration of the apparatus." However, they had not tried and they did not try any repetition (Freire 2014, pagg. 262-263).

Nevertheless, Holt received his PhD degree with the approval of this work. At the beginning, Holt ventured to state in his thesis that⁴⁹⁸:

"The polarization correlation results present a far more puzzling (and perhaps) exciting prospect. The statistical accuracy is certainly great enough to allow us to say that a discrepancy with quantum mechanics exists. [...] all the obvious sources of systematic errors have been examined" (Holt 1973, cit in Freire 2014, pag. 260).

⁴⁹⁷ The Italian original text is in Addendum 3, p. 995.

⁴⁹⁸ Holt R.A. (1973), *Atomic Cascade experiments*, PhD dissertation, Harvard University. (Holt 1973) in the following.

But quite soon, he attenuated his claim, arguing that: “On the other side are two arguments for disbelieving the results”. I took into account the second argument, firstly, maybe the main reason for Holt’s sudden caution:

“the second argument against our results is the enormous success that has been enjoyed by quantum mechanics in the correct prediction of experimental results” (Holt 1973, cit in Freire 2014, pag. 260).

Apparently, being against the trust in QM, being quite the only scholars to find a disagreement with QM predictions, could be a reason for their withdrawal in repeating the experiment.

Surely the first argument was the choice to wait for the Clauser-Freedman results. Indeed, Holt and Pipkin, (according to Clauser 2002) got the first results, while Clauser-Freedman experiment was still in progress:

Unfortunately, their results disagreed with the quantum-mechanical prediction, but did agree with the local hidden-variable prediction by the CHSH inequality they decided to be cautious, however, and not to publish their results until they saw what our experiment at Berkeley revealed. Once we announced our results, they then opted not to publish theirs formally. Instead, they distributed an informal preprint that outlined what they had done and observed. In the end, they concluded that their result was due to an underlying systematic error (Clauser 2002, pag. 81).

In (Clauser 2002) his attitude was really clear and well-intentioned to shaking QM, being a good motivation to perform the 1969 CHSH proposal:

[...] I naturally wanted to ‘shake the world’. Since I already believed that hidden variables may indeed exist, I figured that this was obviously the crucial experiment for finally revealing their existence. But if they do exist, then quantum mechanics must be verifiably wrong here, with its error having gone undiscovered heretofore. [...] To me, the possibility of actually experimentally discovering a flaw in quantum mechanics was mind-boggling (Clauser 2002, pag. 80. My underline).

In the joint paper (Clauser, Shimony 1978), the two authors, after highlighting that

Holt and Pipkin took great care to check possible sources of systematic error: the contamination of the source by isotopes with non-zero nuclear spin, perturbation by external magnetic or electric fields, coherent multiple scattering of the photons (radiation trapping), polarisation sensitivity of the photomultipliers, and spurious counts from residual radioactivity and/or cosmic rays, etc” (Clauser, Shimony 1978, pag. 1910),

conjectured that the systematic source of error in Pipkin and Holt experiment was to be ascribed to

the form of stresses in the walls of the Pyrex bulb used to contain the electron gun and mercury vapour. Estimates of the optical activity of these walls were then made, and the results were corrected correspondingly. [...] It is noteworthy, however, that only the retardation sum for both windows was measured, for light entering the cell from one side and exiting through the opposite side. On the other hand, in the present experiment in which light exits from both windows, the relevant quantity is the retardation difference (Clauser, Shimony 1978, pag. 1910).

The experiments carried out by Holt and Pipkin was repeated by Clauser at Berkeley, who found stresses in a lens:

It is also noteworthy that in the subsequent experiment by Clauser [...], a correlation was first measured which agreed with the results of Holt and Pipkin. Stresses were then found in one lens which were due to an improper mounting. (These were too feeble to be detected by a simple visual check using crossed Polaroids.) The stresses were removed, the experiment was re-performed, and excellent agreement with quantum mechanics was then obtained. On the other hand, Holt and Pipkin did not repeat their experiment when they discovered the stresses in their bulb (Clauser, Shimony 1978, pag. 1910. My underline).

As for his previous experiment with Freedman, Clauser obtained experimental results that contradicted his hopes. According to Freire:

He reported them to Wheeler in the following terms: “Dr. Henry Stapp [...] has told me that you were interested in the latest results from my experiments. These were attempting to reproduce the results observed by Holt and Pipkin at Harvard. Unfortunately, I have failed to do so, and obtained more or less good agreement with the quantum mechanical predictions”. Freedman and Holt also thought of repeating Holt and Pipkin’s experiment, but this project did not happen (Freire 2014, pag. 267 My underline).

Anyway, regardless of the possible reasons, Holt and Pipkin did not repeat the experiment and in time left the subject, especially the latter. Selleri stressed how: “Holt and Pipkin experiment was never sent for publication, despite the authors’ belief that it was correct”⁴⁹⁹.

- CLAUSER experiments, June 1976⁵⁰⁰.

Clauser tried to repeat Holt and Pipkin’s experiment using the same cascade although in the ²⁰²Hg isotope of mercury. Also, instead of calcite polarizers, he used parallel plate polarizers. This experiment [violated] the strong inequality and [was] in excellent agreement with the quantum mechanics prediction. In an extension of the same experiment, Clauser measured the polarization correlations by inserting quarter-wave plates between each polarizer and the source. Quarter-wave plates were obtained by applying compression to commercial-type quartz plates⁵⁰¹ (Selleri Paradossi, 2001, pag. 156).

Selleri emphasized how the second attempt of the Clauser experiment of Holt and Pipkin one, despite a mistake three times greater than Clauser’s first attempt, “was published in the *Nuovo Cimento*, which is significant given the triumphant and immediate publication of each experiment in accordance with the [quantum] theory on the *Physical Review Letters*”⁵⁰².

- FRY and THOMPSON experiment, August 1976⁵⁰³.

These authors used photons with 435.8 nm and 253.7 nm wavelengths, emitted in the cascade (1.1.0) of the ²⁰⁰Hg isotope of mercury with nuclear spin zero. The 7³S₁ state of a mercury beam was populated with a two-step process: (i) excitation of the 6³P₂ metastable state by electronic bombardment; (ii) after all short-lived states had decayed, resonant absorption of radiation from an adjustable laser. The laser bandwidth was narrow enough to selectively excite the ²⁰⁰Hg isotope of mercury. The polarizers used were of the parallel plate variety and the magnetic field in the interaction volume was artificially reduced to less than 5 mG.

⁴⁹⁹ “L’esperimento di Holt e Pipkin non fu mai inviato per la pubblicazione, nonostante la convinzione degli autori che fosse corretto” (Selleri Fisica 900, 1999, pagg. 209-210).

⁵⁰⁰ Clauser J.F. (1976), “Experimental investigation of a polarization correlation anomaly”, *Physical Review Letters*, 36 (21), pagg. 1223-1226.

⁵⁰¹ The Italian original text is in Addendum 3, p. 995.

⁵⁰² “fu pubblicato sul *Nuovo Cimento*, il che è significativo data la trionfale e immediata pubblicazione di ogni esperimento in accordo con la teoria [quantistica] sul *Physical Review Letters*” (Selleri Fisica 900, 1999, pag. 210).

⁵⁰³ Fry E.S., Thompson R.C. (1976), “Experimental test of local hidden-variable theories”, *Physical Review Letters*, 37 (8), pagg. 465-468.

Since the initial state of the cascade had angular momentum $J=1$, it was necessary to take into account the opportunity of different populations and relative coherence between the initial Zeeman sub-levels, which Fry and Thompson did by measuring the polarization of the 435.8 nm radiation. [...] The experiment [was] in excellent agreement with the theory and in violation of the strong inequality⁵⁰⁴ (Selleri Fisica 900, 1999, pagg. 210-211).

On the devices used by Fry, Clauser recalled how:

When we began our experiments at Berkeley, tunable lasers were not yet commercially available. Thus, all of the Berkeley experiments were done without their benefit. As a result, the count rates were very low, and the experiments were very slow and tedious. Finally, Fry and his student Thompson performed a test of the CHSH prediction using one of the first commercially available dye lasers. Given the short counting periods afforded by their use of this laser, they could more readily search for possible systematic errors. Their experiment confirmed our “Berkeley” results, but disagreed with Harvard’s results. The score was now three to one, in favour of quantum mechanics (Clauser 2002, pag. 82. My underline)⁵⁰⁵.

So, three experiments in this first period agreed with the predictions of QM, whereas one agreed with the local realistic theories predictions, obtained via Bell’s theorem. Indeed, other experiments in a field diverse that optics, specifically nuclear physics at low energies, were performed in those years: Kasday, Ullman, and Wu at Columbia University (1970,1975)⁵⁰⁶; Faraci, Gutkowski, Notarrigo, and Pennisi (1974) at Catania⁵⁰⁷, where in the early 1980s, Selleri and Garuccio collaborated in several experiments for the detection of Empty Waves; Wilson, Lowe, and Butt (1976) at Birbeck College in London⁵⁰⁸.

⁵⁰⁴ The Italian original text is in Addendum 3, pp. 995-996.

⁵⁰⁵ Tunable lasers permitted an improved speed of accumulating data, passing from the 200 h needed for collecting data in Clauser or Holt experiments, to 80 min in Fry and Thompson experiment.

⁵⁰⁶ Kasday L.R., Ullman J.D., Wu C.S. (1970), “The Einstein-Podolsky-Rosen argument: positron annihilation experiment”, *Bullettin of American Physical Society*, 15, pagg. 586; Kasday L.R., Ullman J.D., Wu C.S. (1975), “Angular-correlation of Compton-scattered annihilation photons and hidden variables” *Nuovo Cimento*, 25B, pagg. 633-661. Kasday was also present to Varenna Summer School in 1970 and published in the Proceedings as: Kasday L.R. (1971), “Experimental test of quantum predictions for widely separated photons”, in D’Espagnat B. (ed), Società Italiana di Fisica, *Foundations of Quantum Mechanics. Proceedings of the International School of Physics “Enrico Fermi”*, pagg. 195-210.

⁵⁰⁷ Faraci G., Gutkowski S., Notarrigo S., Pennisi A.R. (1974), “An Experimental Test of the EPR Paradox”, *Lettere al Nuovo Cimento*, 9, pagg. 607-611.

⁵⁰⁸ Wilson A.R., Lowe J., Butt D. K. (1976), “Measurement of the relative planes of polarization of annihilation quanta as a function of separation distance”, *J. Phys. G: Nucl. Phys.* 2, pagg. 613-624.

All of them tried to repeat the Wu and Shalnov experiment using the electron-positron annihilation process in two γ photons. As recalled by Freire, they:

[...] arrived at conflicting results. Kasday's and Wilson's results confirmed quantum mechanics while Faraci's results confirmed Bell's inequalities. At Saclay, France, M. Laméhi-Rachti and W. Mittig performed an experiment with spin correlation in proton-proton scattering, confirming quantum mechanics. In the 10 years between Freedman and Clauser's result in 1972 and the three experimental results Aspect and his collaborators would publish in 1981 and 1982, we had 11 experiments and 8 teams involved with tests of Bell's theorem (Freire 2014, pag. 259. My underline).

The second generation of experimental tests went from 1976 to 1982 and were dominated by Alain Aspect, who started in 1974 with the reading of Bell's paper and still continued in 1981 and 1982 with the publishing of three different experiments. As summarized in Laurora:

When in 1974 Alain Aspect read Bell's article on the EPR paradox, he focused his attention on the experimental investigation of the theorem, in particular on the 'loophole' related to the locality. In the first-generation experiments the axes of the polarizers were kept fixed during the measurements, so there may be the chance of an exchange of information between the polarizers at a lower speed than the speed of light. To eliminate this opportunity, it is therefore necessary during the experiment to change the polarizers configuration in a shorter time than the flight time of the photons from the source to the polarizers⁵⁰⁹.

Let us go describing the two experiments, made at Orsay by Aspect in collaboration with his undergraduate student Philippe Grangier and the research engineer Gérard Roger, as well as the third one, made by Aspect in collaboration with Jean Dalibard and Gérard Roger, using Selleri's words:

- ASPECT, GRANGIER, ROGER⁵¹⁰

These authors used 551.3 nm photons and 422.7 nm photons from the calcium cascade (0.1.0). In this case the calcium atoms were excited at $4p^2\ ^1S_0$ level by a two-photon

⁵⁰⁹ The Italian original text is in Addendum 3, 996.

⁵¹⁰ Aspect A., Grangier P., Roger g. (1981), "Experimental tests of realistic local theories via Bell's theorem", *Physical Review Letters*, 47 (7), pagg. 460-463.

absorption non-resonant process, using a 406 nm laser beam and a 581 nm laser beam, both laser beams being perpendicular to the calcium atomic beam emitted by a tantalum furnace. The laser beams were located in the interaction region to obtain a roughly cylindrical source of about 60 microns in diameter and 1 mm in length [with high efficiency]. During the experiment the atomic density in this “source” ranged from $3 \cdot 10^{10} \text{ cm}^{-3}$ to 10^{10} cm^{-3} , which generated a flow of cascades of no less than $4 \cdot 10^7 \text{ s}^{-1}$. The cascade photons were analyzed with one-way parallel plate polarizers and filters in the same way as in previous experiments. The high atomic density of the source generated up to 100 coincidence events per second, which allowed a statistical accuracy of 1% in just 100 s of data collection. These authors obtained [a] result [...] in accordance with quantum mechanics and in violation of the strong inequality for more than 13 standard deviations⁵¹¹ (Selleri Paradossi, 2001, pag. 156).

Freire remarked that:

[...] as the Paris group separated source and polarizer by 6.5 m, they could rule out Furry’s conjecture, which suggested that the quantum non-locality would vanish after the photons travel a distance of the order of the coherence length of their associated wave packets; which meant a distance of 1.5 m in this experiment. In mathematical terms, a pure state would evolve towards a mixture of factorizing states (Freire 2014, pag. 277).

The second and third experiments were:

- ASPECT, GRANGIER, ROGER⁵¹²

In 1982 these authors carried out an experiment, originally suggested and studied by Garuccio and Rapisarda⁵¹³, based on two-way polarizers.

Each was a polarizing cube, manufactured using the properties of a thin dielectric foil and coated against reflection, and which could be rotated around the axis of observation. The apparatus made it possible to measure a quantity proportional to the correlation function using a quadruple coincidence technique for each of the [...] four relative orientations of the polarizers [...]. The authors found experimentally [a result] in violation of Bell’s [...] limit [...] but in accordance with the quantum mechanical prediction⁵¹⁴ (Selleri Paradossi, 2001, pagg. 156-157).

⁵¹¹ The Italian original text is in Addendum 3, p. 996.

⁵¹² Aspect A., Grangier P., Roger g. (1982), “Experimental realization of Einstein-Podolsky-Rosen-Bohm Gedankenexperiment - a new violation of Bell inequalities”, *Physical Review Letters*, 49 (2), pagg. 91-94.

⁵¹³ Garuccio A., Rapisarda V. (1981), “Bell’s inequalities and the four-coincidence experiment”, *Nuovo Cimento* 65A (3), pagg. 269-297. Aspect, Grangier, and Roger did not cite this work, even if they were in contact with Garuccio and Selleri. For more details, see my interview to Garuccio in section 2.7.6.

⁵¹⁴ The Italian original text is in Addendum 3, pp. 996-997.

- ASPECT, DALIBARD, ROGER⁵¹⁵

In all the experiments described so far, the optical axes of the polarizers were kept fixed during each set of measurements. So in principle there was the opportunity for an exchange of information between the polarizers at a speed lower than the speed of light. This possibility, although very unlikely, given that we do not see exactly what mechanism could generate such an exchange, could still be eliminated if the polarizers configuration was changed [during the experiment] in a shorter time than the flight of light from the source to the polarizers. In the experiment carried out by Aspect, Dalibard and Roger, an optical switch quickly redirected the photons coming from the source to one or the other of the two [polarizers]. Unlike the previous experiment, only the transmission channels of the polarizing cubes were used. The light deviation was obtained with a Bragg reflection from a stationary ultrasonic wave in water. The light was fully transmitted when the stationary wave amplitude was zero, while it was deflected by 10 mrad when the amplitude was maximum. Every 10 ns the switching from one channel to another took place, and since this time, as well as the average life of 5 ns of the intermediate level of the cascade, was lower than L/c (40 ns), where $L=12$ m was the separation between the two switches and c the speed of light, no signal could connect a detection event on one side and the corresponding change of orientation on the other. In this experiment only a few coincidences per second were revealed, with an accidental background of about 1 coincidence per second.

The authors found experimentally a clear violation of the strong inequality and an agreement with the quantum mechanics prediction. The switches on both sides had periodic rather than random operation, but were driven by two different generators with different frequencies, and the authors, therefore, believed that they operated independently. However, it was pointed out by [Anton] Zeilinger⁵¹⁶ that the situation may hide a conceptual difficulty that makes the experiment inconclusive.

Finally, it should be remembered that the most important criticism of Orsay's experiments was based on the very likely presence of a resonant trapping of radiation in the source due to the great atomic density of the calcium beam. An unconvincing response to these criticisms was given by Aspect and Grangier⁵¹⁷ (Selleri Paradossi, 2001, pag. 157. My underline).

On the other hand, Freire remarked the great level of Aspect's experiments, the final word they put on the research on Bell's theorem, according to many

⁵¹⁵ Aspect A., Dalibard J., Roger G. (1982), "Experimental test of Bell inequalities using time-varying analyzers", *Physical Review Letters*, 49 (25), pagg. 1804-1807.

⁵¹⁶ Zeilinger A. (1986), "Testing bell's inequalities with periodic switching", *Physics Letters*, 118A (1), pp. 1-2.

⁵¹⁷ Aspect A., Grangier P., (1985), "About resonant scattering and other hypothetical effects in the orsay atomic-cascade experiment tests of Bell inequalities: A discussion and some new experimental data", *Lettere al Nuovo Cimento*, 43, pagg. 345-348. The Italian original text is in Addendum 3, p. 997.

scholars. And the world by Aspect himself, which enhanced the belief that the word “end” had been put on the topic:

All the [Aspect] experimental results violated Bell’s inequalities and strongly confirmed quantum mechanics’ predictions.

[...] The third experiment [...] was the result that most resonated in the physics community, but its accuracy was less than the previous ones, [...] violating Bell’s inequality by five standard deviations in runs which lasted 200 min.

[...] The first [Orsay] paper received 764 citations, the second one, 1,039, and the third one, 1,555 citations (Freire 2014, pag. 277).

Aspect’s early 1980s experiments on Bell’s theorem were so convincing that in subsequent years nobody bothered to replicate them (Freire 2014, pag. 290).

[...] After these experiments he considered that there was no foreseeable groundbreaking achievement to be obtained with new experiments on Bell’s theorem and moved to other subjects (Freire 2014, pag. 278).

[...] The reasons for this were related to the perceived unfeasibility of new experimental breakthroughs, as remarked by Aspect: “I do not see further meaningful progress can be made in the domain of Bell’s inequalities, at least with our apparatus. We have exploited its maximal possibilities. Sure, an additional decimal could be obtained, but would it be worthy?” (*apud* Deligeorges 1985, p. 137) (Freire 2014, pag. 290).

The main enhancement brought by Aspect and collaborators were undoubtedly the very high level of the whole apparatus in their laboratory, really difficult to reproduce elsewhere and consequently the opportunity to realize an experiment in which the polarizers would be rotated while the photons were in flight.

Even if he managed to realize it only in his third experiment.

But, unlike the Freire and Aspect words concerning the unfeasibility of new experimental breakthroughs, the third generation of Bell’s inequality experiments started at the end of 1980s until the early 2000s. The turning point came from the change of the source. Instead of photons from atomic cascades used for the production of pairs of photons with entangled polarizations, the new tests used the so-called *Parametric Down-Conversion* (further referred to as PDC). This phenomenon is observed when the light coming from a high frequency laser affects nonlinear optical crystals.

For a small fraction of the incident photons, each of them splits into two photons, still storing the energy and the momentum.

Garuccio was deeply intertwined with these ideas. He had basic contacts and collaboration with both the places and the research groups active at that time and during the 1990s, i.e., Maryland group, with Carroll Alley and Yanhua Shih; and Rochester group, with Leonard Mandel and Zhe-Yu Ou. In both US universities, they reached the same conclusion: photon pair sources based on PDC could be used in order to produce correlated photons in greater number than that obtained by the atomic cascade method. However, as explained by Laurora:

it was later demonstrated that the PDC, used in these experiments and called type I, is not able to produce states of the type of EPR and that therefore the experiments conducted with this type of photon pair sources are not significant for the debate on Bell's inequality⁵¹⁸.

Garuccio proposed⁵¹⁹ (quite ignored at the beginning) a new kind of source based on parametric down-conversion of type II.

After some quarrels (that I will introduce later), his work was finally recognised and Zeilinger group in Innsbruck and Shih group in Baltimore managed to create this new source.

The different generation of Bell's experiments benefited from the technological improvement which made possible realizing new experiments for Bell's inequalities, passing by the atomic cascades to PDC of the first and the second type (PDC I and PDC II respectively). PDC permitted the creation of pairs of photons during the interaction between a laser beam and nonlinear optical crystals⁵²⁰. According to Freire,

⁵¹⁸ "in seguito si è dimostrato che la tecnica della dissociazione parametrica, usata in questi esperimenti e detta di tipo I, non è in grado di produrre stati del tipo di EPR e che quindi gli esperimenti condotti con questo tipo di sorgenti di coppie di fotoni non sono significativi per il dibattito sulla diseguglianza di Bell" (Laurora 2014, pag. 167).

⁵¹⁹ Garuccio A. (1995), "Parametric down-conversion sources, beam-splitters, and Bell's inequality", in *Fundamental Problems in Quantum Theory*, Greenberger D. (ed), New York: New York Academy of Sciences.

⁵²⁰ PDC was first verified by David Burnham and Donald Weinberg in 1970.

Interferometry experiments using PDC photon pairs were pioneered by two teams. According to Daniel Greenberger, Michael Horne, and Anton Zeilinger, who are key actors in this field, “real experiments commenced when Carroll Alley and Yanhua Shih at the University of Maryland first used down-conversion to produce an entangled state and when Ruba Ghosh and Leonard Mandel at the University of Rochester first produced two-particle fringes without using polarizers” (Greenberger et al. 1993, p. 22). PDC source became the standard resource for all the experiments with entangled photons from 1995 on (Freire 2014, pag. 292).

Freire wrote a remarkable summary about the period between 1950 and 1990 emphasizing the above improvement:

In hindsight, the 1950s may be seen as the time when challenges to the dominant complementarity interpretation began to appear, through Bohm’s causal interpretation and Everett’s relative states presentation of quantum theory. The 1960s were dominated by the emergence of the quantum measurement issue as a problem, that is, the quantum measurement problem, with the dispute between Wigner and Rosenfeld marking the first clash inside the orthodoxy in quantum physics. The 1970s was the decade of the experiments on Bell’s theorem and the early expectation of their results. In the 1980s the agenda diversified. New foundation experiments enabled by technical advances flourished even in the absence of Bell’s theorem experiments, which were only resumed at the end of the 1980s. New interpretations appeared and the old ones were renewed.

Theoretical breakthroughs were made in at least two different areas: the transition from the quantum description to the [one] through the idea of decoherence, and the early ideas related to the use of quantum physics to improve computers and computer science.

[...] They were the years when technical advances made the implementation of different thought experiments possible. [...] In no other time in the history of research on the foundations of quantum mechanics had new technical possibilities played such a role.

[...] The plethora of new experiments testing quantum predictions in domains never touched before mobilized physicists at large, including many who were not interested in foundations of quantum mechanics in itself (Freire 2014, pag. 288).

[...] Interferometry experiments using PDC photon pairs were pioneered by two teams. According to Daniel Greenberger, Michael Horne, and Anton Zeilinger, who are key actors in this field, “real experiments commenced when Carroll Alley and Yanhua Shih at the University of Maryland first used down-conversion to produce an entangled state and when Ruba Ghosh and Leonard Mandel at the University of Rochester first produced two-particle fringes without using polarizers” (Freire 2014, pag. 292).

The question has remained open for a long time, thinking that, despite the many announcements about the violation of Bell's inequalities for huge numbers of standard deviations, still halfway through the 2000s, there were experiments to test the Bell's inequalities yet and that possible non-local models still remained. Kumar stated:

Whereas Aspect's team and others who tested Bell's inequality ruled out either locality or an objective reality but allowed a non-local reality, in 2006 a group from the universities of Vienna and Gdansk became the first to put non-locality and realism to the test. The experiment was inspired by the work of the British physicist Sir Anthony Leggett. In 1973 [...] Leggett had the idea of amending Bell's theorem by assuming the existence of instantaneous influences passing between entangled particles. In 2003, the year he won the Nobel Prize [...], Leggett published a new inequality that pitted non-local hidden variable theories against quantum mechanics.

The Austrian-Polish group led by Markus Aspelmeyer and Anton Zeilinger measured previously untested correlations between pairs of entangled photons. They found that the correlations violated Leggett's inequality, just as quantum mechanics predicted. When the results were published in the journal *Nature*, in April 2007, Alain Aspect pointed out that the philosophical 'conclusion one draws is more a question of taste than logic'. The violation of Leggett's inequality implies only that realism and a certain type of non-locality are incompatible; it did not rule out all possible non-local models (Kumar 2009, pag 354. My underline).

Selleri contributed a lot to the long-standing issue of the resolution of Bell's inequalities, currently considered violated in support of the absolute validity of the QM, especially after the two 1982 experiments at Orsay. He recalled an experiment made in 1985 by Perrie, Duncan, Beyer, and Kleinpopp⁵²¹, quite prior to the evolution due to the PDC. He made also a symptomatic statement of his thought on the issue related to the alleged violation of Bell's inequality:

These authors measured the polarization correlations between two photons produced simultaneously (i.e. not in an atomic cascade) by the metastable state of atomic deuterium $2S_{1/2}$ in a true two-photon decay.

⁵²¹ Perrie W., Duncan A.J., Beyer H.J., Kleinpopp H. (1985), "Polarization Correlation of the Two Photons Emitted by Metastable Atomic Deuterium: A Test of Bell's Inequality", *Physical Review Letters* 54 (16), pag. 1790. Erratum *Physical Review Letters* 54 (16), pag. 2647.

The emission of a single photon from this deuterium state is forbidden and the main channel for de-energization is the simultaneous emission of two photons, which may have different wavelengths from event to event, provided that it is consistent with the conservation of energy for the pair (constant frequency sums). However, the observation window was in practice limited between 185 and 355 nm due to the absorption of radiation into atmospheric oxygen. In this experiment an atomic deuterium beam with a density of 10^4 cm^{-3} was produced and used in the metastable state. The two photons resulting from the decay of the $2S_{1/2}$ state were collimated by two lenses. The polarizers were parallel plates. By measuring the probabilities of coincidence, the authors obtained [a result] in accordance with the theoretical prediction [of quantum mechanics] but in violation of the strong inequality for little more than two standard deviations. The statistical accuracy of this experiment is therefore lower than that [of the previous] experiments with the atomic cascades. In an extension of this experiment the correlation between the circular polarizations was measured by placing achromatic quarter-wave plates on the path of each photon, between the source and the linear polarizer. The results obtained did not violate the inequality and the agreement with quantum mechanic predictions was restored only by postulating a very poor way of working of the plates. However, it remains very interesting that the only two circular polarization measures in EPR-type experiments [...] have indicated that Bell's (strong) inequality is not violated and have required intellectual tricks to search, in some way, to represent the results in a consistent way with quantum mechanics⁵²³ (Selleri Paradossi, 2001, pagg. 157-158. *Italics in the text. My underline*).

In his book of 1999 he had also said that:

In a scientifically sound world these measures would already have been repeated many times until the reality of these divergences from quantum mechanics would have been definitively clarified⁵²⁴ (Selleri Fisica 900, 1999, pag. 215).

I will show how Selleri, in his *à la* Einstein perspective of local realism, introduced several results that would still leave the speech open, including a probabilistic application of the EPR reality criterion and the Bell's strong and

⁵²² Clauser J.F. (1976), "Measurement of the circular-polarization correlation in photons from an atomic cascade", *Il Nuovo Cimento B*, 33, pagg.740-746; Duncan A.J., Kleinpoppen H. (1988), "The experimental investigation of the Einstein-Podolski-Rosen question and Bell's inequality", in Selleri F. (ed) (1988), *Quantum Mechanics versus Local Realism: The Einstein-Podolsky-Rosen Paradox*, London/New York: Plenum, pagg. 175-218.

⁵²³ The Italian original text is in Addendum 3, pp. 997-998.

⁵²⁴ "In un mondo scientificamente serio queste misure sarebbero già state ripetute molte volte fino a chiarire definitivamente la realtà o meno di queste divergenze dalla meccanica quantistica".

weak inequalities that allow the preservation of the locality, a real hub for Selleri. Aspect's outcomes were for Selleri non-resolutive, being in disagreement only with the strong inequalities, which are related to some additional assumptions.

2.7.5-SELLERI'S ACTIVITY ON BELL'S INEQUALITY

What was Selleri's role, how was his research activity on EPR and Bell's inequality in specific? Obviously the two questions are closely interrelated, having Bell brought EPR into practice from the level of philosophical disquisition within the laboratories. Selleri considered both issues in this way, writing papers that received several endorsements, as well as dissemination articles taken as reference, and books as well. I would like to recall how Selleri always campaigned for possible experiments to be carried out, often proposing them theoretically out of nowhere and for the first time. Selleri experienced an inverse path to historical development, first approaching Bell's inequality, and writing on EPR in a second time, even if he subsequently ended with writing on both topics. In the following I will show papers on Bell's inequality and/or EPR seamlessly.

Selleri's first reference to Bell's inequality dated back to 1969 actually, with his lectures in Frascati (Selleri 1969b).

(Capasso, Fortunato, Selleri 1970) was another seminal paper, fully devoted to Hidden Variable theories and with several references to Bell.

Selleri's activity allowed to find other inequalities, to introduce the definition and the difference between strong and weak inequalities as well as to reach the conclusion that LR is still more general than Bell's inequality, and that the only violated inequalities are the strong ones, related to LR plus additional assumptions, still maintaining LR as an extremity natural point of view in physics. In addition, jointly with the Bari group that he created, he investigated on the three different generation of Bell's-type experiments. According to his own words, the first paper with the complete definition of weak and strong Bell's inequality was "Is Bell's Inequality Violated in

Nature?”⁵²⁵, submitted in 1987 at a conference in Gdansk (Poland)⁵²⁶, where Selleri stated that:

Some inequalities, like the original one due to Bell, have been deduced from local realism alone. They are far weaker than other inequalities, here called strong inequalities, that have been deduced with the essential help of arbitrary additional assumptions. The results of the performed experiments with atomic photon pairs violate strong inequalities, but are fully compatible with Bell’s inequality. Therefore, no evidence for a breakdown in nature of local realism still exists (Selleri Gdansk, 1988, pagg. 256. Underline in the original text).

Given $C(a,b)$ the correlation function, the average product of the results obtained by two observers which measured dichotomic observables $A(a)$ and $B(b)$, namely some property of pairs of quantum objects in an ensemble, Bell’s inequality could be deduced from LR, giving

$$|C(a,b) - C(a,b')| + |C(a',b) - C(a',b')| \leq 2$$

where $(a, a'$ and $b, b')$ are two different values of each of the two experimental parameters.

The original proof based on a deterministic formulation of LR had been already extended to a probabilistic formulation of the same idea in Selleri’s paper, “Weak Realism for Pairs of Correlated Systems”⁵²⁷.

According to Selleri:

[The above] inequality has never been tested experimentally and can be considered a “weak” inequality if compared with much stronger (i.e., more restrictive) inequalities that can be deduced from local realism if some suitable “additional assumptions” are made (Selleri Gdansk, 1988, pagg. 257).

⁵²⁵ Selleri F. (1988), “Is Bell’s Inequality Violated in Nature?”, in *Problems in Quantum Physics; Gdańsk '87. Recent and Future Experiments and Interpretations. Gdańsk, Poland 21-25 September 1987*, Kostro L., Posiewnik A., Pykacz J., Żukowski M. (eds.), Singapore: World Scientific, pp. 256-268. (Selleri Gdansk, 1988) in the following.

⁵²⁶ Selleri experienced a deep relationship with Poland and specifically with Ludwik Kostro. Selleri was a member of the Gdanskie Towarzystwo Naukowe Academy and he was also awarded the gold medal; Kostro gave rise to an international endorsement for proposing Selleri for the 1991 International Lakatos Award (even though he did not win).

⁵²⁷ Selleri F. (1988), “Weak Realism for Pairs of Correlated Systems”, in *Microphysical Reality and Quantum Formalism, Proceedings of the Conference ‘Microphysical Reality and Quantum Formalism’ Urbino, Italy, September 25th-October 3rd, 1985 Vol. 1*, Van der Merwe A., Tarozzi G., Selleri F. (eds), Dordrecht: Kluwer Academic Publishers, pp. 347-361.

Selleri also stressed how “other weak inequalities expressing the physical meaning of local realism at the empirical level have been obtained by Garuccio and Selleri and by Lepore for arbitrary linear combinations of correlation functions and of joint probabilities, respectively”⁵²⁸ (Selleri Gdansk, 1988, pag. 257).

Then Selleri showed how the additional assumptions, like the CHSH one, give birth to inequality really different by Bell’s one, yet “resolving” in strange ways the concrete problems. In addition, it was well explained how the low efficiencies of polarizers created a real problem to the effective resolution and validity of the experiments:

In 1969 Clauser, Horne, Shimony and Holt (CHSH) suggested the use of pairs of optical photons emitted in atomic cascades. For such photons the binary choice was between transmission and absorption in a polarizer.

[...] the only way to know that a photon has been transmitted is to detect its presence beyond the polarizer, but **photon detectors have efficiencies of only 10-20%**.

This means that one cannot really measure a double transmission probability⁵²⁹, but only a joint probability for double transmission and double detection of the two photons. This problem has traditionally been “solved” by means of ad hoc assumptions concerning the nature of the transmission/detection process. The additional assumption of CHSH is the following: Given that a pair of photons emerge from two regions of space where two polarizers can be located, the probability of their joint detection from two photomultipliers is independent of the presence and the orientation of the polarizers (Selleri Gdansk, 1988, pagg. 259. Selleri’s underline, my **bold**).

And finally he defined the weak and strong inequalities:

It is perhaps not useless to repeat that all the new results deduced [...] have become possible only because the CHSH assumption has been made. It is therefore not correct to confuse the the original Bell’s inequality with the much stronger inequalities which were [...] deduced. In the future we will therefore adopt the following definitions:

⁵²⁸ The two papers are Garuccio A., Selleri F. (1980), “Systematic Derivation of All the Inequalities of Einstein Locality (A Reply to a Comment by A. Garg)”, *Foundations of Physics*, 10 (3-4), pp. 209-216, and Lepore G. (1989), “New inequalities from local realism”, *Foundations of Physics Letters*, Volume 2, Issue 1, pp.15-26, respectively. The latter was a preprint at the time of Selleri’s quotation. In the early 1980s, the FQM Bari group produced a lot of interesting papers on Bell’s inequality, EPR and empty waves.

⁵²⁹ The double transmission is nearer to experimental observation since it is impossible to detect the absorption of a photon in a polarizer.

Weak inequality: An inequality exclusively deduced from local realism and violated by quantum mechanics in the case of nearly perfect instruments.

Strong inequality: An inequality deduced from local realism and from ad hoc additional assumptions, such as the CHSH hypothesis stated above, and violated by quantum mechanics in the case of real instruments (Selleri Gdansk, 1988, pagg. 261. Selleri's underline and **bold**).

[...] the inequalities used for the experimental study of the Einstein-Podolsky-Rosen paradox are of two different types:

Weak inequalities, like Bell's original inequality, deduced directly from the principles of local realism. They would disagree with the empirical predictions of quantum theory only in the case of nearly perfect instruments, but are entirely compatible with such predictions for all performed experiments, owing to the low efficiency of photon detectors.

Strong inequalities, deduced with additional assumptions [...], are typically 20-30 times stronger than the weak ones, in the sense that they restrict *the same misurable* quantity to an interval 20-30 times smaller. The strong inequalities are violated by the predictions of quantum theory in all experiments that have been published.

The additional assumptions [...] always lead to exactly the same consequences (Afriat, Selleri 1999, pag. 99. *Italics* in the original text).

Selleri also emphasized the arbitrariness of the additional assumptions:

[...] It is important to stress that these additional assumptions are completely arbitrary and do not rest on any fundamental physical principle. Furthermore, there is no experiment to test them independently of local realism (Afriat, Selleri 1999, pag. 99. My underline).

Selleri spoke very often on Einstein locality. It is known that the EPR paradox proved the existence of an incompatibility between three hypotheses:

- 1) QM is correct;
- 2) QM is complete;
- 3) the results of measurements on atomic systems are determined by “elements of reality”, which are associated with the measured system and/or the measuring apparatus, and which remain unaffected by measurements on other distant atomic systems.

The causal and local point of view as expressed in hypothesis 3 could be referred to as Einstein locality. Often the “elements of reality” of EPR are called hidden variables λ .

According to Garuccio,⁵³⁰ “this determination can be either deterministic in the true philosophical sense, or probabilistic, in the sense that only the probabilities of the different outcomes of correlated measurements are fixed by the hidden variables” (Garuccio 1988, pag. 87).

Garuccio also underscored how Selleri introduced, in (Selleri 1978), the first general method for deducing inequalities from Einstein locality. It was based on the assumptions of deterministic local hidden-variable theory, and on perfect total anticorrelation between two measurements.

EPR and Bell’s works helped Selleri to reinforce the philosophical and physical superstructure of the Local Realism (LR), a key topic which has crossed his life, becoming, as told, the cornerstone of his activity in FQM (see page 215 and following).

Selleri emphasized the importance of Bell in turning LR a concrete theory, really alternative to QM and not a merely metaphysical point of view. In addition, Selleri’s researches showed that the LR is broader and more general than Bell’s inequality itself, giving birth to many diverse inequalities:

Today [thanks to researches, among others, by Popper, Lakatos and Kuhn] it has been understood that physics theories are often very good but never perfect.

And that a truly modern theory should be an open, perfectible structure without dogmatic and irreversible postulates. If all the problems of quantum mechanics could be reduced to the paradoxes of completeness, the road that leads to a better physics would be wide open in front of us [there would be a certain and correct alternative and quantum mechanics would disappear]. Unfortunately there is the paradox of Einstein, Podolsky and Rosen, which is a much more important problem because it is based on the incompatibility between the experimental predictions of quantum mechanics and those of the very natural point of view of local realism.

⁵³⁰ Garuccio A. (1988), “All the Inequalities of Einstein locality”, in Selleri F. (ed), *Quantum Mechanics versus Local Realism: The Einstein-Podolsky-Rosen Paradox*, London/New York: Plenum. (Garuccio 1988) in the following.

[...] the turning point in the EPR paradox took place in 1965, when John Bell showed that local realism, although not a true scientific theory but only a very general point of view on the facts of nature, contained empirical predictions. These predictions were not of the type $G=g$ (i.e.: measure the physical quantity G and you will necessarily find the numerical value g) as those of quantum mechanics, but instead of the less precise type $g_1 \leq G \leq g_2$ (i.e.: measure the physical quantity G and you will surely find a numerical value between g_1 and g_2). Bell's result is very important because the well defined prediction $G=g_{mq}$ falls outside the range allowed by local realism (because $g_{mq} > g_2$). [Hence] it is impossible to interpret quantum mechanics from the point of view of local realism, which is a great lesson in epistemology: a scientific theory may be related to philosophical choices in such a profound way as to be incompatible with other philosophical choices in its own empirical predictions. In short, it can be said that we can really make philosophy on an experimental level!

[...] The new fact, which emerged in 1965 with the discovery of Bell's inequality, is that this realism is not metaphysical, given the great wealth of falsifiable empirical predictions it has generated. Bell's inequality is but one of these predictions, because literally countless other consequences of local realism have been discovered that are in no way deducible from Bell's inequality⁵³¹ (Selleri Paradossi, 2001, pagg. 131-132. My underline).

Thus, thanks to Bell, realism is no longer metaphysical, it is comparable with scientific results and generates experimental consequences that could be proven false. In the preface of (Afriat, Selleri 1999), they argued that:

Bell derived his celebrated inequality, however, from realism and locality, and showed that it is grossly violated by quantum mechanics. So it is impossible to accept realism, locality, *and* quantum mechanics.

The fact that experiments performed to decide the matter have not refuted a genuine (that is, weak) Bell inequality is well known to the experts, but there is a widespread belief that this is only due to the nonideality of the measuring devices (in particular the low efficiency of available photon counters). It is also believed that this difficulty will eventually be overcome (Afriat, Selleri 1999, pag. vii. *Italics* in the original text).

Indeed, in a letter to Emilio Santos⁵³², Bell claimed that:

⁵³¹ The Italian original text is in Addendum 3, pp. 998-999.

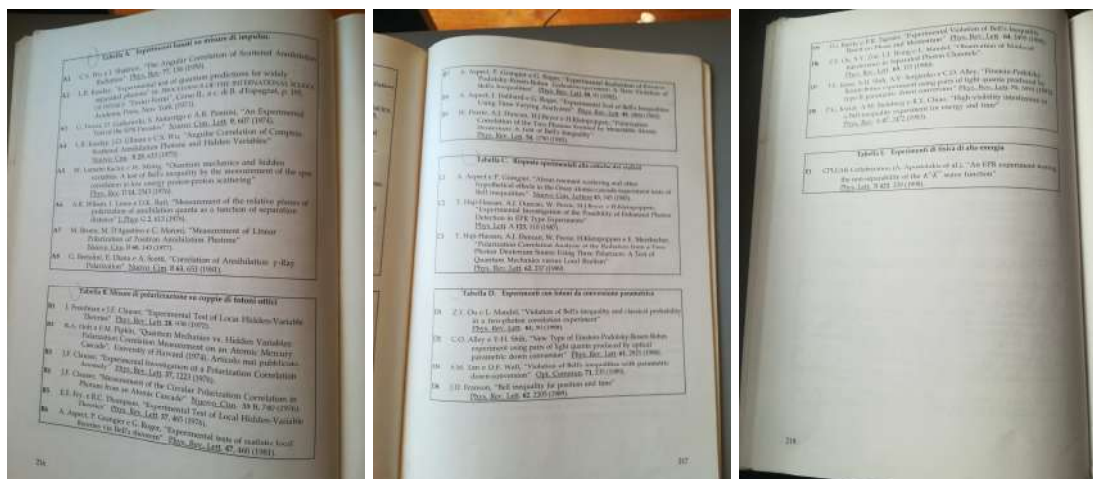
⁵³² J.S. Bell, letter to E.Santos. See Ferrero M., Santos E. (1997), *Foundations of Physics*, 27, pagg. 765-800. at pag. 787.

I always emphasize that the Aspect experiment is too far from the ideal in many ways—counter efficiency is only one of them. And I always emphasize that there is therefore a big extrapolation from practical present-day experiments to the conclusion that nonlocality holds. I myself choose to make the extrapolation, for the purpose at least of directing my own future researches. If other people choose differently, I wish them every success and I will watch for their results (cit. in Afriat, Selleri 1999, pag. vii).

To sum up, Selleri introduced diverse approaches and papers to show how the debate on Bell's inequalities is really far from being solved, and how the LR could no longer merely explain the dark paradoxes within the QM interpretation.

2.7.6-SELLERI'S RESEARCH FACED WITH BELL-TYPE EXPERIMENTS AND THEIR OUTCOMES

Selleri introduced an useful chart showing the division of some of Bell-type experiments into categories also linked to the applied technologies.



(Selleri Fisica 900, 1999, pagg. 216-218)

Concerning the three generations of experiments introduced in section 2.7.4, the common feature for Selleri was that all of them used additional assumptions. In his thought all experiments carried out until that time could not yet been decisive in favour of QM or LR because of certain additional (highly questionable) assumptions that were introduced in the reasoning to compensate for the low efficiency of the detectors.

As already mentioned, Selleri showed how this behaviour led to the violation of a peculiar kind of inequalities, that is, the “strong” inequality previous

mentioned, which had a very tight range. It is only this latter inequality which has been always violated by experiments, whereas the Bell's inequality, as all weak inequalities, based on LR with no additional assumptions, remained save and untouched. According to Selleri, "none of the [...] experiments has ever violated the weak inequality" (Selleri Paradossi, 2001, pag. 155). Selleri at the same time, criticized also the ideological approach of the QM:

[...] it is useful to clarify well an important ambiguity regarding Bell's inequality that makes unacceptable the idea, unfortunately widespread, that in certain experiments the local realism has already been falsified.

[...] While the weak inequalities are a consequence of pure and simple local realism, the strong ones are deduced using additional hypotheses that establish a comparison between different possible destinies of the same photon and are not, therefore, susceptible to experimental verification. They appear to be valid within traditional quantum theory, but they strongly limit the range of possible theories based on local realism. If you want to give a priori confidence to the existing theory, the experimental study of the EPR paradox makes no sense, the problem being solved a priori by postulate. Otherwise, it makes no sense to embark on research by making assumptions that strongly favour one of the logically available alternatives.

[...] The quantum-mechanical predictions relative to all the carried out experiments violate the strong inequalities [which restrict the allowed values to a range that, in the most favorable case, is 4% of the one allowed by the weak inequalities], but are abundantly within the range [foreseen by the weak inequalities]. Therefore, both the predictions of [the same] quantum theory and the results of the experiments satisfy the weak inequalities, and cannot be declared incompatible with local realism. This is a well-known situation, but many prefer to ignore it⁵³³ (Selleri Paradossi, 2001, pagg. 152-153. My underline and *Italics*).

Selleri introduced a categorization of the diverse classes of additional assumptions used in the generations of experiments made between 1972 and the 1982⁵³⁴, belonging to the strong inequalities for one-way and two-way polarizers.

⁵³³ The original Italian text is in Addendum 3, p. 999.

⁵³⁴ I limited myself in submitting here the Selleri's analysis of the experiments made in the period 1972-1982.

My following underlined statements of the additional assumptions are related to the original papers by the several scholars mentioned below and by the Alain Aspect thesis⁵³⁵:

In the experiments performed with one-way polarizers there is no photon reflection but only a detectable transmission or an undetectable absorption.

1. The additional assumption made [in 1969] by Clauser, Horne, Shimony and Holt (CHSH): Given that a pair of photons emerges from two regions of space where two polarizers can be located, the probability of their joint detection from two photomultipliers $D_{12}(\lambda^{536})$ is independent of the presence and orientation of the polarizers.
2. The additional assumption made [in 1974] by Clauser and horne (CH): For every photon in the state λ the probability of detection with a polarizer placed on its trajectory is less than or equal to the detection probability with the polarizer removed.
3. Aspect's [1983] additional assumption: The set of detected pairs with a given orientation of the polarizers is an undistorted representative sample of the set of pairs emitted by the source. The assumption is not very carefully formulated, because the possibility of absent polarizers should also have been considered. The main idea is, however, very clear and can be expressed thus: *Detectors introduce no distortions in the sets of pairs transmitted through regions where polarizers can be present.* Technically this means that every double-detection and -transmission probability is proportional to its corresponding double-transmission probability, and that the proportionality constant is always the same (Afriat, Selleri 1999, pagg. 104-106. *Italics* is the original text. My underline).
4. The theory of EPR experiments with two-way polarizers was developed by Garuccio and Rapisarda (GR) and adopted by Aspect *et al.* in their [third] experiment. To obtain a strong inequality, GR made [in 1981] the following assumption: The sum of the probability of transmission and detection, and of the probability of reflection and detection [respectively in the ordinary and extraordinary beams emerging from] a two-way polarizer does not depend on the orientation of the polarizer (Afriat, Selleri 1999, pag. 108. My Underline).

Selleri also analyzed the awareness of the role of the additional assumptions in the scholars community, giving some examples. He claimed that:

⁵³⁵ Aspect A., *Trois tests expérimentaux des inégalités de Bell par mesure de corrélation de polarisation de photons*, Thèse, Université de Paris-Sud, Centre d'Orsay 1983

⁵³⁶ λ is always the variable that fixes the values of dichotomic observables. i.e., λ is the hidden variable.

The awareness of the decisive role of additional hypotheses in the experimental research over the last thirty years on the EPR paradox is not very widespread. Many authors confuse strong and weak inequalities. Others produce arguments to prove the validity of the additional hypotheses. For instance, Ghirardi⁵³⁷ criticizes the idea that the observed correlations may partly be due to the action of polarizers and detectors, saying: “It seems superfluous to me to point out that everything we know about the functioning of detectors contradicts this assumption”. It takes a bit of presumption to write this, [perpetuating even today the fact that] since 1927 orthodox physics has given up understanding the things of the world to rely only on mathematical formalism.

Obviously even the realists know little, but at least they try to advance conjectures! Immediately afterwards Ghirardi adds: “But an incredible conspiracy of events and the occurrence of processes that find no explanation or reason for being in any of the known theories, would cause photons and/or counters to agree to make us believe that we live in a quantum world rather than a classical one”⁵³⁸. These are strong words, which serve to cover the weakness of objections. Obviously, if one relies on “known theories” it is easy to exclude an innovative proposal! Awareness of the true logical situation is more widespread among experimental physicists. The last experimental group which focused on local realism stated: “All experiments so far have had detection efficiencies low enough to allow the opportunity that the subset of the revealed events is in agreement with quantum mechanics although the whole set satisfies Bell’s inequalities”⁵³⁹ (Selleri Paradossi, 2001, pagg. 154-155)⁵⁴⁰.

Selleri recalled a paper⁵⁴¹, which he refers to as a bad example for an experimental comparison between QM and LR:

A good example of how the experimental comparison between existing quantum theory and local realism should NOT be done is provided by an article from the Rochester group [...]. In the abstract, the authors state: “We report on a two-photon coincidence experiment that demonstrates the violation of local realism, as defined by Einstein, Podolsky and Rosen, by about 45 standard deviations...”. This experiment deserves to be

⁵³⁷ Gian Carlo Ghirardi (1935-2018): Selleri had a bittersweet relationship with him, having great disagreement but also inviting him to several conferences, also having a critical but constructive correspondence. Selleri had an influence on Ghirardi, even if the latter moved away from Selleri for differences on the ideological meaning of physics. For this last topic, see (Del Santo 2020 preprint, page 10). For Ghirardi and for his famous GRW theory, see also <https://scholar.google.it/citations?user=kDFGYXQAAAAJ&hl=it> and <https://www.nature.com/articles/s41567-018-0249-y>. Both accessed 24 September 2020.

⁵³⁸ Ghirardi G.C. (1997), *Un’occhiata alle carte di Dio*, Milano: Il Saggiatore. Both quotations are at page 224.

⁵³⁹ Rowe M.A., Kielpinski D., Meyer V., Sackett C.A., Itano W.M., Monroe C., Wineland D.J. (2001), “Experimental Violation of a Bell’s Inequality With Efficient Detection”, *Nature* 409 (6822), pagg. 791-794.

⁵⁴⁰ The original Italian text is in Addendum 3, pp. 999-1000.

⁵⁴¹ Torgerson J.R., Branning D., Monken C.H., Mandel L. (1995), “Experimental demonstration of the violation of local realism without Bell inequalities”, *Physics Letters A*, 204 (5-6), pag. 323-328.

discussed because, if this statement was correct, the debate on local realism would finally find its conclusion. However, the research of the Rochester group immediately appears in a different light if one pays attention to the hypothesis made in the brief introduction: “As the photodetectors used in the experiment are not 100% efficient and the optical components are not completely loss-free, it is necessary to assume that photon absorptions are completely random. With that (fair sampling) assumption [...] we show that local realism is violated in two-photon coincidence experiments with linear polarizers [...]”. Actually, the hypothesis of the faithful sample⁵⁴² generates a very important mutilation of local realism: this is known at least since 1983, when that hypothesis was first proposed. Assuming that the set of revealed photon pairs provides a faithful reproduction of the physical properties of the set of pairs emitted by atoms is, in essence, the same thing as admitting that this last set is homogeneous in the sense of von Neumann, so that the observed probabilities cannot have any structure.

In this way not only the ordinary hidden variables are forbidden, but not even the much more general “propensities” can be introduced, which have been demonstrated [Afriat, Selleri 1999] to be the most natural basis for the probabilistic local realism. However, it should be remembered that the assumptions on which von Neumann’s theorem is based are known to be inadequate since the time when it was demonstrated [by Bohm and Bub in 1966] that using them strongly favours one of the two contending conceptual schemes. A first answer to Rochester’s group was given by Garuccio⁵⁴³, who was able to demonstrate that the crucial parameter η_0 (ratio of quantum-mechanical probability sums) must assume a value not lower than 0.82 in order to have a significant comparison between quantum mechanics and local realism. The actual value for the Rochester experiment was 0.10. Garuccio concluded: “This shows that the experiment is not able to distinguish between quantum mechanics and Einstein’s locality without making additional assumptions”. A second answer was given by Cabello and Santos, who built a model with local hidden variables whose predictions were in numerical agreement with the photonic coincidences observed in that experiment. Those [values] of coincidences implied a violation of local realism according to Rochester’s physicists, while Cabello and Santos rightly concluded: “We affirm that the experiment refutes only that narrow family of local hidden variable theories that contain additional hypotheses”⁵⁴⁴ (Selleri Paradossi, 2001, pagg. 158-159. My CAPS).

⁵⁴² The hypothesis is practically the one written at point 3 at page 388 in this dissertation.

⁵⁴³ Garuccio A. (1995), “Hardy’s approach, Eberhard’s inequality, and supplementary assumptions”, *Physical Review* 52 (4), pag. 2535-2537.

⁵⁴⁴ The original Italian text is in Addendum 3, pp. 1000-1001.

Selleri stressed in several sources⁵⁴⁵ the weakness of the additional assumptions with respect to LR and their incorrectness, due to their experimental nonverifiability as well as their arbitrariness:

[...] It is important to stress that these additional assumptions are completely arbitrary and do not rest on any fundamental physical principle. Furthermore, there is no experiment to test them independently of local realism (Afriat, Selleri 1999, pag. 99).

All the experiments performed with atomic photon pairs have found a very good agreement with the quantum mechanical predictions (with an isolated exception) and violations of the strong inequalities by several standard deviations. It thus seem probable that strong inequalities do not hold in nature and it must be concluded that some of the assumption used in order to derive them are not valid. **The choice is to get rid either of a fundamental physical principle (local realism) or of an arbitrary and experimentally nonverifiable additional assumption.**

The second alternative seems to be by far the most reasonable one (Selleri Gdansk, 1988, pagg. 267. Selleri underline, my **bold**).

[...] Many experiments have falsified the strong inequality and it seems that it must be concluded that among the hypotheses used to demonstrate it (local realism plus any of the additional hypotheses) there are false ones. The main way seems to be to declare false the additional hypotheses that are substantially arbitrary and have no experimental evidence in their favour. On the other hand, all the experiments carried out so far (2001) are broadly compatible with the validity of the weak inequality deduced from local realism alone. The comparison between local realism and quantum mechanics is still open, as it has always been⁵⁴⁶ (Selleri Paradossi, 2001, pag. 159. My underline).

In another paper, “Do Performed Optical Tests Disprove Local Realism?”⁵⁴⁷ it is pointed out that:

⁵⁴⁵ Specifically, (Selleri Gdansk, 1998); (Afriat, Selleri 1999); (Selleri Paradossi, 2001).

⁵⁴⁶ [...] Molti esperimenti hanno falsificato la disuguaglianza forte e pare proprio che si debba concludere che fra le ipotesi usate per dimostrarla (realismo locale più una qualsiasi delle ipotesi addizionali) ve ne siano di false. La via maestra sembra essere quella di dichiarare false le ipotesi addizionali che sono sostanzialmente arbitrarie e non hanno alcuna prova sperimentale a loro favore. Tutti gli esperimenti fatti finora (2001) sono invece ampiamente compatibili con la validità della disuguaglianza debole dedotta dal solo realismo locale. Il confronto fra realismo locale e meccanica quantistica è ancora aperto, come lo è sempre stato (Selleri Paradossi, 2001, pag. 159).

⁵⁴⁷ Lepore V.L., Selleri F. (1990), “Do Performed Optical Tests Disprove Local Realism?”, *Foundations of Physics Letters*, 3 (3), pp. 203-220. (Lepore, Selleri 1990) in the following.

The development of local realism since the 1935 EPR paper is by far the most profound criticism of quantum mechanics, and the conflict between the two approaches cannot be resolved trusting the validity of additional assumptions, essentially arbitrary and not testable independently.

The study of different inequalities and recent developments in particle physics, in order to obtain different sources of nonlocal states, are two ways to escape the loophole of the additional assumptions and to carry out more reliable experiments (Lepore, Selleri 1990).

In Selleri's thought, the issue has never ended and it sounds wrong to say that the problem is solved in favour of QM. In the section 2.9, I will expose his research on EPR applied to Particle Physics, using neutral kaons. Another road to try defining the matter, still open today.

I am going now to introduce the topic concerning the link between Selleri and his colleagues from Bari with the Bell-type experiments, specifically with the three ones made by Aspect.

Selleri knew well the Aspect experiments because, joint with Garuccio, and Rapisarda and others in Catania, he tried to do the same.

People working in Bari with Selleri, in what will be the group of Bari on FQM, had a deep collaboration with France and Jean-Pierre Vigier, specifically. Garuccio and Cufaro Petroni experienced periods in France, still collaborating and publishing joint with Vigier.

In my interview to Cufaro Petroni, he analyzed the consequences of Aspect experiments on the trajectories undertaken by Selleri and Vigier, who also had an intense, complex relationship:

The experiments on Bell's inequality by Aspect have, in a sense, divided the paths of Jean-Pierre Vigier and Franco Selleri. Jean-Pierre has always been a pragmatist; and Selleri has always been a dogmatist, I say this in a very summary way. Selleri was convinced that one could find a way to say that Aspect's conclusions were not correct, and he worked all his life (actually up to a certain point) to criticize the experiments, redo them, etc. Jean-Pierre tried to settle down, to somehow find a way to live reasonably with this reality (Interview with Nicola Cufaro Petroni, Bari, 31 May 2019).

In the widely mentioned interview by Freire, Selleri described his reaction to Aspect's experiments.

It is a very long reference, but it thoroughly evidences the road followed by Selleri in his approach to the issue, still recalling the steps of his research on Bell's inequality. Freire asked Selleri about Aspect's experiment in the 1980s, but Selleri asked to begin from 1974 and the Fry experiment, submitted at Erice conference:

I would start from the Fry experiment. Because I was in Erice when Fry gave a talk in which it was clear that there was a confirmation of the 1972 results by Freedman and Clauser. So it seemed that Bell's inequality was really violated. That was for me very difficult to take. By the way, Garuccio was with me [...] and after we came back we were so impressed by this experiment that we spent maybe three months working hard at a different approach to Bell's theorem. We knew of course the proofs of Bell's theorem and I had even published one original too, so it was not a mathematical problem. Still one had the idea that perhaps something was wrong and that perhaps after all Bell's theorem did not exclude all possible forms of local realism.

In other words perhaps Bell's theorem was not general enough. If we had to accept those experiments, still we would try not to give up the local realism. We would rather give up Bell's theorem.

So we started building models which we never published, but we built many, many models of simple situations; [...] to see if we could invent something which was according to the philosophy of local realism but in violation of Bell's theorem. And we could not. So that failure was for me a great strengthening in the belief in the generality of Bell's theorem. We tried the hard way to produce the opposite and we could not succeed, so Bell's theorem is really very general. After that, I invented many other proofs, some probabilistic. The strongest proof is probabilistic and based on Popper's propensities. That is the most general proof I know about Bell's theorem, because you do not even need elements of reality in a single object. Local propensities for statistical ensembles, they are enough. If you don't have that form of reality you have no reality at all. It is ridiculous to abolish reality like that. So my conclusion was that Bell's theorem was general, totally general, and that we should understand then better the experiments. Then started a long struggle against the naïve acceptance of the experiments, and finally I understood what was wrong with the experiments, was the fact that Bell's theorem is something like this. It is a statement that a quantity B should be within minus 2 and 2. This B is a linear combination of correlation functions. And quantum mechanics says that B is 2.83. So we can make a linear graph and say this is minus 2, this is 2, according to Bell's theorem the quantity B must lie in this region. Instead quantum mechanics says it should be there, outside. But then we read carefully the original 1969 Physical Review Letters paper by C.H.S.H. and we discovered the existence of additional assumptions in their proof.

That *Physical Review Letters* formulation of Bell's inequality is not equivalent to the original one because it has one additional assumption, one more. And in fact the quantity B is restricted to be between plus and minus 0.015, it is restricted to be in a very, very small region. And then experimentally it is found to be here outside this small region of the stronger inequality. I call the original one "weak inequality" and I call this "strong inequality", for obvious reasons. The strong inequality is violated experimentally, the weak inequality has never been violated by any experiment. So what has been violated by experiments are probably the additional assumptions. There are four additional assumption proposed by different authors for deducing the strong inequality. They are equivalent; conceptually they have been shown to be all the same. Their implication is always the same. There is no difference. But let's say if we take one of them [...] a statement comparing different setups that could be used to study the problem. Suppose you have a region in which there is a polarizer and there is a detector. The polarizer has a certain orientation, and suppose that a photon just passed through the polarizer, so the photon is now in this region between the polarizer and the detector, the photomultiplier. [the additional assumption] states that the probability for the photon to be detected is independent on the orientation and even on the presence of the polarizer. That is, the probability to detect this photon should not depend on having crossed the polarizer. This is a very strong limitation of the hidden variables because if you have a hidden variable you expect it to be modified by the polarizer. This is very important in particular if it is the same variables that makes the photon cross or not cross the polarizer and makes the photon to be detected or not detected in the photomultiplier. So if it crosses it will probably be modified. So the situation generated by the additional assumptions is like saying: now we have to fight, but you bind my hands behind my back, and after we fight. Obviously you win.

When Aspects experiment came there was already this distinction between weak and strong inequalities available. I was not surprised anymore that Aspect found a violation of the inequality because it had already been found by Fry and by Freedman and Clauser. The answer was exactly the same. And of course I told Aspect about this, but he did not seem to be very interested and he continued to go around in Europe and to give seminars in which the point of the limited meaning of his experiment was not made. They have been carefully silent about this difficulty. And you can see what people believe by whether they talk or not talk about additional assumptions. Everybody knows. I mean if you take an expert, he really knows about this. But he does not like to talk about it because he hopes that quantum mechanics is correct and that hidden variables aren't there.

That is the amazing reaction. So with Aspect it was exactly the same. We published more careful papers, this time with Lepore [...] in which we had the analysis of the additional assumptions one by one showing the consequences one by one why the same criticism applied to all four of them.

So people know that I do not believe that experiments of Bells theorem are final and that newer experiments are needed. [...] My feeling is that [...] on the average people do not want to discover the truth about Bell's theorem. I think that is the average case. Although maybe this average does not correspond to any single human being, as it is very difficult to imagine a physicist who thinks, "Probably Bell's theorem shows that quantum mechanics is wrong, but this I will cover up". This is clearly not the right psychological way. But finally it comes out to the same (AIP 2003. My underline and **bold**).

Selleri explained very well his position and his fight to try to resolve the matter, also pointing out the difficulties inherent in the strong and wide belief in the absolute correctness of QM.

Another recollections came from the interview I had with Garuccio, concerning the unusual relationship between Aspect and his group on one hand; and the Italian collaboration between Bari and Catania (Selleri, Garuccio, Rapisarda, Falciglia, Pappalardo and so on) on the other hand. Aside from the description of the conflicts that later arose with Aspect, Garuccio, after recalling how the collaboration with Rapisarda and Catania's group began in the late 1970s, explained also some unknown key details on the matter regarding the experiments, as well as the peculiar, at times unrealistic but also intriguing conditions in which the Bari-Catania collaboration set up a laboratory to produce a Bell-type experiment. The two following excerpts are extremely long but I think it could shed a new light on the topic, being also a thrilling and technically well detailed evidence:

The collaboration with Catania and Vittorio Rapisarda began in the late 70s. We had met at some other meetings, I think we were present in Erice (also in Frascati '74) where Franco, Vigier and others were also present; the elite working on these subjects. Then it continued with a series of interactions and especially in a conference, I do not remember which one, I think in Northern Italy. I remember that Vittorio and I began to talk about the opportunity to make an experiment and together we agreed that the correct thing, the one that would have reduced as much as possible the impact of the additional hypotheses, was to use a scheme with four coincidence detectors, two for the left channel and two for the right channel.

We started writing about this topic, then we met again in Paris, I think immediately after the meeting in Geneva [organized by the Association Ferdinand Gonseth⁵⁴⁸].

He came to Paris and there we established a strong and coherent collaboration but also a solid friendship. On his return to Italy, Vittorio presented a financing proposal to the INFN to build this 4-coincidence detector device. During Rapisarda's first stay in France we met Aspect and d'Espagnat, whom we had already met in Erice. We visited him, talked about the experiment and the situation. Aspect showed us his apparatus. It was an exceptional apparatus, in an optical institute where, even then, there was a power laser in every room. So he was moving in an environment that was always working on these things on a daily basis at a cutting-edge level.

Aspect was very friendly, very precise. He told us that he was going to work on the nonretarded choice experiment at the same time and basically we asked him if he was going to work on the four coincidences experiment. He told us he would not work on it. Obviously we would not even start, otherwise, with any initiative, since he had everything he wanted, photomultipliers, etc. available, while we had to start from scratch with the laboratory.

And so we went back to Italy, we got the funding and I started collaborating with Rapisarda, I believe, in 1979, or in the early 1980s. An unfortunately short but very intense collaboration.

⁵⁴⁸ <http://afg.logma.ch/>. Accessed 24 September 2020. The Association promoted the journal *Epistemological Letters* and it was really active in the debate on hidden variables during the 1970s. see also (Laurora 2014).

The mission of the Association is the following:

The Association which bears the name of the philosopher Ferdinand Gonseth intends to inscribe its action and its reflection in the spirit of the open philosophy it defended, a philosophy which advocates:

- a rejection of all dogmatism anchored in definitive and reassuring certainties;
- an openness to experience, i.e. a welcome for novelty and the unexpected, but also a recourse to the rigour of control through experience;
- in particular, an openness of philosophy to science, to a continuous interdisciplinary dialogue with science; science and philosophy are one body, and anything that happens again in science, whether in methods or results, can have an impact on philosophy, even in its most fundamental principles;
- but also an opening on man as a whole, on his commitment in action and in society, on the problems he faces today. "We choose commitment in this time and its risks" (quote from the editorial of the first issue of *Dialectica*).

F. Gonseth, on the other hand, was opposed to any philosophy that was closed in on itself, its problems and its methods, which would remain in its ivory tower and confine itself to a purely verbal, speculative, theoretical or formal level.

(L'Association qui porte le nom du philosophe Ferdinand Gonseth entend inscrire son action et sa réflexion dans l'esprit de la philosophie ouverte qu'il a défendue, philosophie qui prône:

- un refus de tout dogmatisme ancré dans des certitudes définitives et sécurisantes;
- une ouverture à l'expérience, c'est-à-dire un accueil à la nouveauté, à l'imprévu, mais aussi un recours à la rigueur du contrôle par l'expérience;
- en particulier, une ouverture de la philosophie à la science, à un continuel dialogue interdisciplinaire avec la science; science et philosophie ne forment qu'un seul corps et tout ce qui se passe de nouveau en sciences, que ce soit dans les méthodes ou dans les résultats, peut retentir sur la philosophie jusque dans ses principes les plus fondamentaux;
- mais aussi une ouverture sur l'homme dans sa totalité, sur son engagement dans l'action et dans la société, sur les problèmes qui se posent à lui actuellement. «Nous choisissons l'engagement dans ce temps et ses risques» (citation tirée de l'éditorial du premier numéro de *Dialectica*).

F. Gonseth s'est par contre opposé à toute philosophie fermée sur elle-même, ses problèmes et ses méthodes, qui resterait dans sa tour d'ivoire et se cantonnerait sur un plan purement verbal, spéculatif, théorique ou formel).

I did nothing else in those years but go back and forth between Bari and Catania, working together with him and the other people of the group, Falciglia, Pappalardo, in building this laboratory. Which was placed, not like Aspect's, on a normal floor, in a facility for making optics (and therefore had water, light, gas, air in all the rooms), but in a basement, a basement of a building next to the old Physics Institute. We had to build everything from the walls, that separated a garage from our laboratory to a local thermal cooling system because our laser, a power laser, consumed an enormous amount of energy to work and had to be cooled. In Catania you could not use running water, for the simple reason that, at a certain point, it was taken away from the building. And this would have created dramatic problems for us and the rest of the building and we had to get 8-10 tanks of 1 m^3 of water (each one) to create a cooling lung. We worked during the day using this water in a closed circuit, which was then cooled during the night, when we did not work and the hot air produced by the exchange we threw it on a secondary road, practically at the level of people's feet. So even that system was designed and calibrated in such a way that it did not bother the people who were passing by too much. So we were moving in a complex situation, with a completely non-existent laboratory; with people present in Catania with skills in the field of vacuum, optics and electronics that had never been put together in all this type of equipment.

The years were very beautiful, very exciting, until February 1982, when Aspect, who had had problems doing his famous experiment on last minute choices of polarization, published a preprint in which he said he had done the experiment with the four coincidences. This caused us great embarrassment and tension, especially as our initial idea was burned out over time. And Vittorio decided to take a tour to explain to the top of the INFN, which gave us money for the experiment, and around other people like Gozzini of Pisa the meaning of the operation, our surprise, and also the fact that we were sure that Aspect did not do this experiment. I could not move because my wife was in the ninth month of pregnancy and had also had problems during the pregnancy itself. So he decided to go through Bari before going to Rome. However, during the journey from Catania to Bari, on February 21, 1982, near Gioia del Colle, he had an accident during an overtaking and died along with three other people. This changed the climate and the meaning of everything we were doing. But in spite of everything, we went on. Without Vittorio's guidance I moved for a few months, with the small child, to Catania, until September 1982. We carried on with the experiment and started to collect data. But basically we never really managed to activate that two-photon process of the atomic cascade that would give a signal significant enough from the point of view of coincidences. After a few years the experiment, now obsolete, closed and the members of the group decided to make other projects. Meanwhile there was a violent clash between Franco Selleri and Aspect. Instead, we had a more reasonable attitude.

But Aspect behaved at that time in a not very elegant way and I must also say that in the last conference that there was on Bell's inequality, talking about the additional hypotheses, Aspect stated that he had made an additional reasonable assumption regarding the four-coincidence experiment. But that additional hypothesis, which he considered reasonable, was contained in one of the theoretical works that Vittorio and I had done and which had been published in 1981 on the *Nuovo Cimento*⁵⁴⁹ and also sent to him in terms of *preprint* and even on that occasion had the courtesy to say, despite the fact that I was present, that these hypotheses were Garuccio-Rapisarda's. (Interview with Augusto Garuccio, Bari, 01 July 2019. My underline).

In the same interview I asked Garuccio to shed light on his road from atomic cascades to PDC, another issue in which his work, which give birth to PDC of the type II, was in danger of being ignored and also overlooked. It is another evidence really compelling on the dynamics of the scientific research, on the envy, the clashes but also on the unique atmospheres inherent in the collaborations. And also a thoroughly analysis from the inside on PDC. In addition, it is always pointed out the importance of the esteem and reputation Selleri received, as well as his presence as a supervisor and an intermediary for new contacts and partnerships, as for the meeting between Garuccio and Mandel:

The problem was always to continue a bit on the open road in the work by Vigier, Popper, Garuccio⁵⁵⁰, in order to understand how it was possible to find anomalous situations with photons in pairs having good characteristics. Proceeding in this direction, I do not remember how I came to discover that there was this interesting strand of PDC. At a conference held here in Bari, there was Mandel, and I talked to him about the opportunity to use these photon pairs to do experiments on the undulatory or corpuscular nature of de Broglie's wave, because we were already working on this subject. He was puzzled at first, then very interested and began experimenting over the PDC. He knew a lot about the PDC while I knew very little about the big picture. But I remembered Mandel's experiment with interference from two separate lasers, an experiment that was somewhat at odds with the big picture. So the interaction with Mandel was very positive. We resumed the same idea with Croca, Moreira in Portugal during my stay there.

⁵⁴⁹ Garuccio A., Rapisarda V. (1981), "Bell's inequalities and the four-coincidence experiment", *Il Nuovo Cimento A*, 65, pagg. 269-297.

⁵⁵⁰ Garuccio A., Popper K.R., Vigier J.P. (1981), "Possible direct physical detection of de Broglie waves", *Physics Letters A*, 86 (8), pagg. 397-400.

I did the accounts during a beautiful stay in Lisbon. The interaction was very positive, we wrote this work and now I cannot remember which magazine we sent it to, whether *Physical Review Letters*, or *Physics Letters*.

I remember that one of the referees, who could have been Aspect or some French, replied that we were working on the issue of de Broglie's wave, which was a topic that no one in the scientific community was interested in. Immediately after this evaluation, therefore ideological and non-scientific of the work, came Mandel's preprint who had done the experiment instead. And Mandel was an important figure. I sent the article with Croca and Moreira to a competing magazine and the magazine rushed to publish the work that I happen to think came out a few days after the experimental work. But at least it came out. Since then the collaboration with Mandel began. However, unlike Franco's one with Popper, which I was intermediary in, whether voluntary or involuntary, this one started because Franco invited Mandel to Bari for a series of conferences and this was decisive for this aspect of the collaboration. Then it went on. The precise motivation is the loss of my first competition for associate. After this experience I decided to check the ability and the interest for these things abroad. And so I sent an email, a letter to Mandel to find out if I could visit his labs, obviously at no cost to him, asking for a grant from the INFN that I received. I went to Mandel for a very short time, if I remember correctly in 1993. Mandel was very happy to receive me, we talked a lot, met the people in his group and decided that I would return in 1994 to work with them all summer. I also suggested some experiments that they did. For two years I was at Mandel's for the summer. It was a very interesting period, I learned and gave a lot there too, there were very promising young people. Monken, who is now a professor in Brazil; Branning, with whom I established a close and strong friendship and even scientific collaboration. I worked on the subject of the PDC, because no one was thinking of experimenting with atomic cascades anymore. At that time there were the first experiments on PDC as a source for related pairs. The problem with PDC was that the initial approach was to Type I, which produced photon pairs of identical polarization. So, to make entangled states, it was necessary to take these states with the same polarization, rotate the polarization of a photon, overlay them, make them indistinguishable and then obtain a Type II source. With many conceptual doubts, about the intrinsically entangled nature of the states. Speaking with one of Mandel's collaborators the first year, a Chinese, I said that the ideal would be to have from the beginning related photons with different polarization. And he said "but no, look there are these sources that are already type II, they make or emit pairs of polarized photons in different ways". We thought that they were all emitted with any angular aperture, but then with subsequent studies we realized that the cones in which these objects were emitted were not concentric, not coaxial. This potentially reduced the possibility of building these things, but then I had the idea to study the behavior of the emitted photons at the points of intersection between the two cones, at the intersection zones.

In 1994 I went to a conference in Baltimore organized by Alley and Shih and presented this idea. I also talked about it with a promising young physicist named Kwiat who said: “but no, nothing happens because they only meet in two places”. I said that precisely because they meet in two places, you can study them. The fact is that Kwiat told me that basically you could not go much further down that road. But I still presented the idea at the conference. When I got back from Mandel, I told him all about it and he decided that since they were already doing other experiments on EPR, and it did not (seemed) feasible, it was not appropriate to intervene. I discovered afterwards that there had already been a strong controversy between the Baltimore group and the Rochester group about some ideas stolen from one group to the other. Mandel was very cold at the time about the idea of dealing with this. He did not want to get into an area where others were already working. But I did not publish the work except on the conference proceedings, only to find out the following year that Kwiat, Zeilinger, Shih and others had done this experiment using the idea of PDC type II, but still introducing a modification that made the experiment feasible and that I had not foreseen. They put next to a bi-axial crystal that produced the PDC another crystal of the same nature but rotated by 90°.

Since the crystal was very short and in principle the two photons traveling along different channels, different refraction indexes, came out no longer correlated in time but out of phase in time, by placing a biassic orthogonal crystal the delayed photon was anticipated and the photon that went ahead was delayed and the correlation was regained. It is clear that this was an important part of the process. But the fundamental idea was contained in my work. But it has to be said that in the work they published there is a reference to my idea in a note. Better this than nothing! I found out later during the conference in Trieste, which was attended by Zeilinger’s group and also by Shih, who, as an experimental of the group, made a speech saying that yes, they had done the experiment but the idea of using the PDC type II was by the Vienna group. At that point a colleague of the Vienna group stood up and said that he had to acknowledge that the idea had come, at least at the same time, to Garuccio, because cited in the work. Which made Shih realize, and as a good Chinese, a very elegant person, he came to me to apologize explicitly, saying that he did not know about it, otherwise he would have quoted me explicitly. From there the collaboration with Shih was born, which obviously always took place in the context of the esteem that people had for Franco Selleri. Franco knew well the mentor of Shih, Carroll O. Alley, who had been a very important figure in the physics, in the optics of the time. He was the one who had designed the reflectors that went to the moon. There were very important people in Shih’s group, like David N. Klyshko, the Russian theorist who had discovered the PDC. And we shared the studio and especially the lunch time, which from being a few minutes, sometimes extended to whole hours in which we discussed everything (Interview with Augusto Garuccio, Bari, 01 July 2019. My underline).

The scholars of Bari and Catania group took a key part in the debate on Bell-type experiments, a matter really far from closed today.

2.7.7-SELLERI AND BELL: A SUBTLE RELATIONSHIP

According to (Freire 2004)⁵⁵¹, the ambience for the link between Selleri and Bell's inequality was the 1970 Varenna Summer School:

The time Selleri went on the stage of the research into foundations of quantum mechanics was also the time proposals for experiments on Bell's inequalities appeared. At the very Varenna's course where Selleri spoke about realism and defended the necessity of experiments to settle those controversies, Shimony discussed the proposals for experiments on Bell's inequalities he and Clauser had published the year before.

However, it seems that, when experiments went on the stage, there already was a new mood among physicists, or, at least, a certain number of people willing to consider foundations of quantum physics as a relevant field of research (Freire 2004, pag. 1756).

Anyway, it must be said that Selleri was also tied to Bell by a long acquaintance. They had a correspondence one another at the end of the 1960s, actually, and in the 1970s. And Bell was in Bari for the LXVI SIF National Congress during the period 16-20 October 1980.

Selleri continuously recognized the great breakthrough inherent in Bell's work:

Because the most important ideas came before 1970. I mean Bell's theorem and the very important demolition of von Neumann's theorem done by Bohm and by Bell. Because that theorem had convinced many people, and when I went around to give talks I always found somebody saying, "Oh, but look this is nonsense, it is impossible because there is von Neumann's theorem showing that you cannot have a causal completion of quantum mechanics." And then there was a good answer: "that is not true because it has been shown by Bell and by Bohm that the hypotheses of this theorem are not general enough." So there was very strong point in favour of the innovators (AIP 2003).

In his 1992 book, *Fondamenti della fisica moderna*⁵⁵², Selleri pointed out again the outstanding Bell's work:

⁵⁵¹ Freire Jr. O. (2004), "The historical roots of "Foundations of Quantum Physics" as a field of research (1950-1970)", *Foundations of Physics*, 34 (11), pagg. 1741-1760.

Bell's inequality: if I were the only one who knew it and if I lived on a desert island I would throw a bottle in the ocean with just a single message: *Study Bell's inequality!*⁵⁵³ (Selleri Fondamenti 1992, pag. 24. *Italics* is in the original text).

I have already revealed in Chapter 1 some correspondence between them concerning the Selleri's work with Amaldi Jr and with the angular momentum nonconservation. Selleri was really fascinated by Bell, too, although they experienced an uneasy relationship.

Indeed, as recollected by Barone:

[...] between Franco and Bell there had been a diatribe. Franco was surrounded by all these young people, even recent graduates. Bell accused Franco of being a bit of a kamikaze because by bringing these young people along [in his radical choices], they could be the target of reprisals by the establishment and see their careers crushed. There must be a very tough exchange of letters between Franco and Bell about this. There has been a strong controversy between them (Interview with Michele Barone, Bitonto (BA)- June 1st, 2019).

It could be very interesting and maybe a good breakthrough to try finding these hypothetical letters reported by Barone. But, at the moment, I did not find them at all.

Bell was always worried about the possible damages in the career of other scholars. Before Aspect's decision in order to perform its known experiments, Bell asked him if he had a stable employment position.

Selleri described the same little tidbit in a 1988 letter to Barbiellini, an Italian physicist in Trieste (quite impressive for an idea on an experiment on EPR) and in two other different letters, written in preparation to the 1991

⁵⁵² Selleri F. (1992), *Fondamenti della fisica moderna*, Milano: Jaca Book. ISBN: 88-16-43029-X7. (Selleri Fondamenti 1992) in the following.

⁵⁵³ "La diseguaglianza di Bell: se fossi il solo a conoscerla e se vivessi su un'isola deserta butterei nell'oceano un bottiglia con un solo messaggio: *studiate la diseguaglianza di Bell!*" (Selleri Fondamenti 1992, pag. 24. *Italics* in the original text).

conference held in Cesena⁵⁵⁴, after the dreary and untimely death of Bell in 1990, and sent to Francesco Barone and Silvio Bergia, respectively:

Dear Barbiellini, Gianluigi Fogli told me of your interest in possibly doing an experiment on the EPR paradox using synchrotron light. I am very curious, even if I cannot really understand how pairs with the right correlation are produced in this case. If you wanted to come and give us a seminar, it would be a wonderful thing to talk about the whole thing. I know you talked to Bell: if he had discouraged you, don't worry too much because he does it with everyone, living now for two or three decades in a state of frustration⁵⁵⁵ (Selleri to Barbiellini, 27 April 1988-FSPA-My underline).

In a letter to the Italian philosopher Francesco Barone⁵⁵⁶, there are also some substantial anecdotes on Bell as recollected by Selleri. As in his attitude, he was able to be very edgy towards someone but at the same time (not always) manage not to affect a shred of his esteem:

Bell's discovery will be remembered for centuries to come, I am sure of that, and I have tried to explain why on NCM.

I knew him well, we interacted a lot in the 70s. Then he must have thought that my enthusiasm for his inequality was excessive and once he said to me: "Leave me alone, I am an old man". I did not bother him anymore. He was living this contradiction, of having made the greatest discovery of realism, while being steeped in pragmatism and positivism, and despite being forced to suffer these ideologies at CERN every day. Another time he told me that he was staying at CERN for his wife, who was working there as an experimenter. Otherwise he would have gone to a small Irish university to work exclusively on FQM. He was a person of very deep intelligence, with him I was always on the wrong side. He would plant himself with wide legs in front of the blackboard, almost to ensure a solid balance that concentration elsewhere could put in crisis, and he would bring out important results in a few minutes. I learned years ago that the British are weak in philosophy.

⁵⁵⁴ *Bell's theorem and the foundations of modern physics*. Organized by: City of Cesena and Physics Department of Bari University, Cesena, 7-11 October 1991.

⁵⁵⁵ Caro Barbiellini, mi ha detto Gianluigi Fogli di un tuo interesse a fare eventualmente un esperimento sul paradosso di EPR usando la luce di sincrotrone. Mi incuriosisce molto, anche se non riesco a capire bene come si producono in questo caso le coppie con la giusta correlazione. Se tu volessi venire a farci un seminario sarebbe una cosa splendida per parlare di tutta la faccenda. So che hai parlato con Bell: se ti avesse scoraggiato non ti preoccupare troppo perché lo fa con tutti, vivendo egli ormai da due o tre lustri in uno stato di frustrazione (Selleri to Barbiellini, 27 April 1988-FSPA-My underline).

⁵⁵⁶ Francesco Barone (1923-2001): He was an Italian philosopher, who studied History and Philosophy of Science. See also <http://www.francescobarone.it/> and https://it.wikipedia.org/wiki/Francesco_Barone. Both accessed 24 September 2020.

He was, and despite he wanted it very much he could not get rid of the heavy positivistic conditioning that came from the English environment first, then from Geneva. The need for realism is however shouted out in some of his articles, for example where he renamed “beables” the “observables” of quantum mechanics⁵⁵⁷ (Selleri to Francesco Barone, April 13th, 1991-FSPA. My underline).

In the following letter sent to Bergia, Selleri emphasized again his last anecdote with Bell and also introduced a significant notes on the contribution of Bergia himself to Bell’s inequality, as well as a curious story on David Mermin⁵⁵⁸:

[...] I knew Bell well and dated him in the ‘70s. Then it seemed to me that he considered my interest in his inequality too excited, and a day he said to me: “Leave me alone, I am an old man”.

I did not bother him anymore. He was torn between the positivist tendencies he suffered in his university studies at CERN, and the need for realism, shouted out almost in certain works, such as when he talks about “beables” instead of “observables”.

[...] Let me tell you my opinion, which is based on admiration and interest in the many works that you have dedicated to Bell’s inequality in past years. A review of those results and a claim of their priority over subsequent works by Mermin would be in my opinion extremely useful to focus well on the Italian contributions to the research on the paradox of EPR. It is possible, perhaps probable, that there has been a change in your attitude towards those times and those works, and nothing prevents you from describing this change, but it remains a fact that there are results in your works of a more technical than philosophical nature that should be better known and better valued. I myself would benefit from such a review, even though I constantly received the works you wrote. But who has more time to reread past things? By the way, Mermin won’t be in Cesena, because I did not invite him despite American pressure. The fact is that we invited him to Urbino and then he treated us like fools (he had noticed). Better to keep at bay certain great mastermind of contemporary physics⁵⁵⁹ (Selleri to Silvio Bergia, April 15th, 1991-FSPA. My underline).

It should also be noted that Selleri organized with great passion and gratitude the 1991 conference at Cesena completely dedicated to Bell.

⁵⁵⁷ The original Italian text is in Addendum 3, p. 1001.

⁵⁵⁸ Nathaniel David Mermin (1935-); See https://en.wikipedia.org/wiki/N._David_Mermin; <http://www.lassp.cornell.edu/mermin/>; <https://scholar.google.com/citations?user=BqqUJzIAAAAJ&hl=en>. All accessed 24 September 2020.

⁵⁵⁹ The original Italian text is in Addendum 3, pp. 1001-1002.

And that, for instance, Bell send a postcard to Selleri, after receiving his book “Quantum Paradoxes and physical reality”, the English version of Selleri’s famous book “Die Debatte um den Quantentheorie”:

Dear Franco, a copy of your “Quantum Paradoxes” just came. I am very happy to have it.
Thank you. Warm regards (Bell to Selleri, 22 January 1990-FSPA).

Definitely a relationship with sweet-and-sour undertones!

2.7.8-SELLERI ON EPR

I tried to reveal Selleri’s approach to Bell-type experiments as well as his strong confidence in an issue really far from solved. He devoted himself to EPR later with respect to Bell’s inequality, coming to conceive an interesting *experimentum crucis*. Applying EPR to particle physics (still coming back to his first field of research), Selleri will try, as I will show in section 2.9, to discriminate between QM and LR, bypassing, thanks to kaons, the loophole related to the poor accuracy of the instruments. Kaons which, as we have seen, Bell had already introduced as being more promising than particles with spin $\frac{1}{2}$.

Anyway, to emphasize the importance of this topic in Selleri’s research, just think of the fact that Selleri has dedicated a whole book to EPR, (Afriat, Selleri 1999), as well as several chapters in other books and some papers.

In his introduction to (Nutricati 1998), Selleri stressed how the alleged completeness of QM involves several paradoxes, whose resolution appeared very simple if only the incompleteness of QM is accepted:

The idea that hidden variables do not exist and therefore that the current quantum theory is complete [...] leads to a number of incredible paradoxes, such as those of Schrödinger’s cat, de Broglie’s box, Wigner’s friend and Wheeler’s delayed choice. These paradoxes melt away like snow in the sun as soon as you accept the reasonable point of view that the present theory is incomplete (which of course, in itself, does not imply that it contains errors).

Their existence is merely a consequence of the arrogance inherent in the idea that a human theory can be flawless⁵⁶⁰ (Nutricati 1998, pag. 9. My underline).

He reaffirmed the issue in *La fisica tra paradossi e realtà. Le critiche dei fisici realisti alla teoria dei quanti*⁵⁶¹, probably the most complete Italian book written by Selleri in FQM:

All paradoxes [...] find an obvious solution by admitting that quantum mechanics is incomplete: is not this choice better than having to deal with retroactions of the future on the past, with unobserved states of reality radically different from those we see, with evidence of the existence of a transcendent soul and with parapsychology? But it gets worse. The refusal of serious research that starts from a frank admission of the incompleteness of the present theory prevents us from carrying out those experiments, now suggested by various authors, that could make a qualitative leap forward in our understanding of the most fundamental properties of atomic systems.

[...] Only the paradox of Einstein, Podolsky and Rosen has a distinct logical position. It cannot be solved by admitting that the existing theory is incomplete, but the same validity of the statistical predictions of quantum mechanics comes into play, which is not necessarily affected by the solution of the paradoxes we have discussed above⁵⁶² (Selleri 2001-2005, pagg. 46-47).

Then, Selleri pointed out the possible and wished resolution of EPR paradox related to kaons. While stressing his preference for LR, nevertheless he was always ready to accept every outcome, for the mere interest in science and truth:

The paradox of Einstein, Podolsky and Rosen [...] will soon be solved experimentally. This will happen thanks to a new series of experiments using high efficiency detectors, perhaps with photon pairs, or more likely in particle physics using disintegrations of the ϕ mesons in neutral K mesons pairs [...]. The result can only be either the overcoming of the existing quantum theory, or the death of what appears today as the only rational description of physical reality, local realism. It can also be said that a rational description of atomic reality will become fully possible only when it has been proved that the existing theory is wrong in some of its predictions.

⁵⁶⁰ The original Italian text is in Addendum 3, p. 1002.

⁵⁶¹ (Selleri 2001-2005) in the following.

⁵⁶² The original Italian text is in Addendum 3, p. 1002.

From a strictly logical point of view there is the opportunity that in the end we must conclude that what has been considered rational in the history of thought (the causal description in space and time) is not rational at the atomic level.

Of course this is the solution preferred by the defenders of the dominant paradigm, but it must be added that its complete scientific arbitrariness has become so evident to those who want to see it that today it may seem probable that the final outcome of this comparison is favorable to local realism⁵⁶³ (Nutricati 1998, pag. 14. My underline).

In another book, *La causalità impossibile. L'interpretazione realistica della fisica dei quanti*,⁵⁶⁴ published in 1987, Selleri recalled how is possible to reject the EPR reality criterion, still defending it, of course. And how, among the diverse attacks to EPR, Bohr made the more systemic one, being this a purely philosophical objection, though:

The most radical solution to the paradox is that which rejects the argument of Einstein, Podolsky and Rosen because it is based on the criterion of reality. There are a priori three ways of rejecting EPR's reality criterion:

1) One may declare oneself dissatisfied not with the idea that there is a reality external to us, but with the specific way contained in the reality criterion of recognizing its existence. Sometimes this objection has been made, although not with great conviction.

It can be noted that the EPR criterion is based on determinism, since it requires us to make predictions *with certainty*. But [...] the EPR paradox can be based on a much more general reality criterion, without changing anything in substance. This first hope of overcoming the paradox is therefore unfounded.

2) One can embrace an explicitly idealistic philosophy by simply denying that there is an objective reality independent of man and his observations, at least as far as the world of atoms is concerned.

3) One can embrace a formally agnostic philosophy by stating that the idea of a real atomic world independent of our observations is a *metaphysical* idea.

The position taken by many important physicists is the third, while there is practically no one who explicitly defends the second.

However, it should be noted that the difference between 2 and 3 is not so relevant, since in both cases the introduction of an atomic reality is forbidden (Selleri *causalità impossibile* 1987, pag. 112. *Italics* in the original text).

⁵⁶³ The original Italian text is in Addendum 3, pp. 1002-1003.

⁵⁶⁴ (Selleri *causalità impossibile* 1987) in the following.

[...] The most systematic attack on the work of Einstein, Podolsky and Rosen, which can still today be easily adapted to the rejection of the premises necessary to deduce Bell's inequality, was that of Bohr. The weapon used [...] was his idea of complementarity [...] [according to which it] is the same idea of objective reality that seems unacceptable. [...] Bohr never says that EPR's reasoning is wrong; on the contrary, he seems to think it is completely correct if the premises are accepted. It is therefore on the latter that real disagreement must be sought [...] [and Bohr seems] to find it precisely in the idea that one can [...] speak of an unobserved physical reality. In essence, Bohr considers only our acts of observation and nothing else real. It is precisely for this reason that the root of the disagreement must be considered philosophical in nature. This is an obvious conclusion, although the majority of contemporary physicists think that Bohr proved EPR's reasoning wrong (perhaps without having read his work!). [...] Real, [for Bohr], is only what is concretely measured by a physicist: anthropomorphism kicked out of the door at the dawn of modern physics thus falls through the window with the new quantum theory. [...] Bohr had nothing else to object to, and this highlights the fact that the difference [...] was philosophical in the deepest sense of the word. The scientific community was faced with the choice between two opposing philosophical positions, one of which guaranteed the maintenance of the newborn paradigm, and had no doubt in its choice. Interestingly, Einstein said on several occasions that he failed to understand the full content of Bohr's response and this was probably not an ironic statement (or was only partly so). In fact, it is still possible to fully reckon with Bohr's ideas only on the condition of having made some unusual readings (for physicists) on Kierkegaard's qualitative dialectic⁵⁶⁵ (Selleri causalità impossibile 1987, pagg. 113-115).

Selleri emphasized and categorized the various solutions (mainly nonlocal ones) of the EPR paradox. In (Selleri causalità impossibile 1987) he firstly reminded the so-called “simple collapse” of quantum theory, concept by which it is implied the disappearance of the wave functions generating the EPR paradox (nonfactorizable state vectors) and their replacement with not paradoxical wave functions (factorizable state vectors). Although Bohm and Aharonov came to definitive conclusions against simple collapse, others have rediscovered and re-proposed it, as Jauch, for instance:

[...] who in 1971 proposed that those wave functions that lead to the EPR paradox did not exist in nature; or de Broglie who in 1974 affirmed that paradoxical states exist only when the wave functions of the two correlated objects overlap in space; or finally

⁵⁶⁵ The Italian original text is in Addendum 3, pp. 1003-1004.

Piccioni who in 1985 wrote that the correct formulation of quantum mechanics can contain only factorized state vectors⁵⁶⁶ (Selleri causalità impossibile 1987, pag. 116).

Selleri developed the analysis of the foregoing solutions also in his paper (Selleri EPR History 1988, pp. 1-61), and in his two books, (Selleri Quantum paradoxes 1989⁵⁶⁷, pp. 235-261) and (Afriat, Selleri 1999, pp. 195-242)⁵⁶⁸. In his 1988 paper he introduced them in this way. A possible solution comes from:

Superluminal connections in Dirac's aether, a nonlocal model by Vigier and collaborators in which the aether, with suitable properties, is no longer ruled out by special relativity, especially if the probabilistic nature of quantum phenomena is taken into account.

[...] In Vigier's model this aether-like physical vacuum is made of extended rigid particles which can support, within their interiors, signals with superluminal velocity. The statistical properties of quantum objects reflect, then, nothing but the real random fluctuations of the ether. [...] In Vigier's opinion the existence of superluminal propagations does not necessarily imply a breakdown of causality, if "causality" is defined as follows: 1) the possibility of solving the two-particle problem in the forward (or backward) time direction as a Cauchy problem; 2) the time-like nature of all particle trajectories; 3) the invariance of the formalism under the Poincaré group of transformations (Selleri EPR History 1988, pagg. 49-50).

Another solution comes by Henry Stapp's work⁵⁶⁹:

Nonlocal weak realism. Henry Stapp believes that the quantum-mechanical predictions for the situation dealt with in the EPR paradox have been accurately confirmed "under experimental conditions essentially equivalent to those needed for the EPR argument".

⁵⁶⁶ "[...] Jauch che nel 1971 propose che non esistessero in natura quelle funzioni d'onda che portano al paradosso di EPR; oppure da De Broglie che nel 1974 affermò che gli stati paradossali esistono solo quando le funzioni d'onda dei due oggetti correlati si sovrappongono nello spazio; o infine da Piccioni che nel 1985 ha scritto che la giusta formulazione della meccanica quantistica può contenere solo vettori di stato fattorizzabili" (Selleri causalità impossibile 1987, pag. 116).

⁵⁶⁷ Selleri F. (1989), *Quantum Paradoxes and Physical Reality*, Van der Merwe A. (ed), Dordrecht: Kluwer Academic Publishers. This is the English version of his *Die Debatte um den Quantentheorie*, maybe the most famous book written by Selleri.

⁵⁶⁸ I also quoted other statements by (Selleri causalità impossibile 1987) in the following.

⁵⁶⁹ Henry Stapp (1928-): he is an American mathematical physicist, known for his work in quantum mechanics, particularly the development of axiomatic S-matrix theory, the proofs of strong nonlocality properties, and the place of free will in the "orthodox" quantum mechanics of John von Neumann. See for further information: https://en.wikipedia.org/wiki/Henry_Stapp. See also https://www.csus.edu/cpns/fellows/h_stapp.html. Both accessed 24 September 2020.

Hence he concludes that the world we live in is nonlocal. However, he does not believe that the results obtained by Bell (1965) and by Clauser and Horne (1974) are sufficient for establishing the need for nonlocality since, in his opinion, these authors made very strong assumptions about microscopic reality that are not compatible with orthodox quantum thinking. The refutation of these “strong” assumptions of realism does not imply, however, any retreat to idealism or subjectivism. It is, in fact, possible to substitute them with an “informal” Copenhagen interpretation of quantum phenomena.

In this way Stapp distinguishes a *strict* Copenhagen interpretation, in which nothing at all is said about any reality other than our observations, from an *informal* interpretation, in which one accepts the common sense idea of a *macroscopic* reality that exists independently of our observations and can be described, at least approximately, with the concepts of classical physics. This ‘informal’ interpretation is partly related, at least by Stapp, to Heisenberg’s idea of a transition from the ‘possible’ to the ‘actual’ taking place during the act of measurement. [...] A model theory proposed by Stapp [in 1987] contains certain ‘hidden variables’ λ which represent all the deterministic and stochastic quantities that characterize the unified organic world and which are *not* used to provide the basis for a Clauser-Horne factorization structure of probabilities. They do not reflect ideas of separation, localization, or microscopic structure. [...] Furthermore, in this theory it is assumed that every act of measurement involves a *choice*.

[...] The choices [...] are treated as independent free variables.

[...] Stapp could easily prove that the local choice contradicts the empirical predictions of quantum theory and concluded: “...neither indeterminism, nor counterfactual definiteness, nor any idea of reality incompatible with orthodox quantum thinking need be assumed in order to prove the incompatibility of the empirical predictions of quantum theory with the EPR idea that no influence can propagate faster than light”. The remark about the absence of a ‘counterfactual definiteness’ is, of course, justified by the important fact that, in Stapp’s theory, the fixing of [choices and hidden variables] fixes the value *only of the observable that is actually measured*. The values of the other [...] observables remain, instead, completely indefinite (Selleri EPR History 1988, pagg. 51-53. *Italics* in the original text).

[...] [Stapp] thinks he lives in a nonlocal world. [...] By giving up the locality, he tries to save some form of realism. [...] In the microscopic world is worth [...] only an indefinite and nonlocal form of realism⁵⁷⁰ (Selleri causalità impossibile 1987, pag. 119).

⁵⁷⁰ “[...] [Stapp] crede di vivere in un mondo nonlocale. [...] Rinunciando alla località, cerca di salvare una qualche forma di realismo.

[...] Nel mondo microscopico vale [...] solo una forma di realismo indefinito e nonlocale” (Selleri causalità impossibile 1987, pag. 119).

Then Selleri introduces one of the most supported solution, the retarded choice, conjugated in various ways by several scientists:

Actions of the future on the past. A solution of the EPR paradox based on the idea that it is possible to modify past events by means of retroactions from the future was first proposed by Costa de Beauregard [in 1977]. [His] idea is that a careful examination of the world in which we live is bound to lead to the conclusion that retroactions in time do play a role and should not be discarded in the formulations of our theories.

[...] the roots of Costa de Beauregard's conceptions go deep into classical physics, [but] is in the quantum theory that he thinks the most important effects of retroaction can be seen.

[...] the theory is completely time-symmetrical [he stresses], but only until the idea of the collapse of the wave function is introduced. At this point quantum theory commits itself to the philosophy of retarded waves. In Costa de Beauregard's opinion this happens because "the Copenhagen school has forgotten the hidden face of Aristotle's information". It is precisely in the situations envisaged by the EPR paradox that this "hidden face" shows up again. In order to understand the essence of EPR paradox, Costa de Beauregard considers the mathematical apparatus of quantum theory and concludes that the problem, today, is only that of *tailoring the wording of the EPR situation after the mathematics*. In his opinion there has in fact been, in our century, an irreversible victory of formalism over modelism. From this starting point he deduces that when an EPR pair [...] is measured by two observers in two regions separated by a space-like distance, then it is precisely the act of observation that produces *in the past* of the measurement process, the right physical properties of the photon pair. Each observer is thus considered capable of *telediction* plus *teleaction*, by taking, so to say, a relay in the past, or more precisely, in the source that emitted the two photons. The conclusion that one can draw from this theory is that the element of reality introduced in the formulation of the EPR paradox can be accepted as real, but that it is viewed as created by *actually performed* acts of observation, and as propagating backward in time with one of the two correlated quantum objects, from the region of measurement to the source. In particular, there can be no question of associating elements of reality with observables that are not concretely measured, as was done originally by Einstein, Podolsky and Rosen, and later by Bell and other authors.

In this sense the solution of the EPR paradox proposed by Costa de Beauregard is similar to that of Bohr.

Several other authors have proposed propagations toward the past as a solution of the EPR paradox. In chronological order one can list: Stapp (1975), Davidon (1976), Rayski (1979), Rietdijk (1980), Cramer (1980), Sutherland (1983) (Selleri EPR History 1988, pagg. 53-55. *Italics in the original text*).

[...] a similar proposal for time retroaction was developed by Wheeler, but he never applied it to the EPR paradox⁵⁷¹ (Selleri causalità impossibile 1987, pag. 117).

Incidentally, it should be noted that in a letter to Costa de Beauregard, dated 24 May 1999, Selleri stated that “If I understand well Wheeler took the idea of delayed choice from you: if it is so I am not surprised. I remember the horrible style with which Wheeler stopped Trevor Marshall’s career” (Selleri to Costa de Beauregard, Bari 24 May 1999. FSPA).

Selleri then recalled how others scholars submitted the idea of a delayed choice:

A third author who introduces this idea is the well-known Polish physicist Rayski who [...] concludes that the wave function of quantum mechanics “does not refer directly to physical reality but rather represents our knowledge of this reality”. The action at a distance exhibited by the EPR paradox can be considered as pure appearance, while the information obtained with a measuring act on one of the two objects forming a pair of EPR type actually propagates backwards in time up to the moment of the common emission by the source and - passing from the first to the second object - propagates with it forward in time.

A similar point of view is maintained by Rietdijk according to whom the most natural idea of reality of the theory of Special Relativity implies that events that are future for one observer may be present - and therefore already real - for a second observer in motion with respect to the first. Since the description of the reality of the second observer is as valid as that of the first, we are pushed to accept the four-dimensional reality of the universe, in which past, present and future are on the same level - given, real and in all cases “already existing”. Therefore there may be retroactive actions of the “already existing” future on the present, and it is possible to imagine a solution of the EPR paradox according to this line of thought. The previous ideas [...] are certainly very natural for a theoretical physicist necessarily accustomed to dealing with invariant formal structures for past-future exchange.

It is difficult for a “normal” person to avoid the feeling of having a little bit to do with science fiction, also because irreversibility is part of everyone's daily life⁵⁷² (Selleri causalità impossibile 1987, pag. 118).

⁵⁷¹ “[...] una simile proposta di retroazione temporale è stata sviluppata da Wheeler, che però non l’ha mai applicata al paradosso di EPR” (Selleri causalità impossibile 1987, pag. 117).

⁵⁷² The Italian original text is in Addendum 3, p. 1004.

Another class of “solutions” of the EPR paradox postulates the absence of separability of quantum systems and tries to save their reality by introducing mechanisms for instantaneous communication at a distance of two objects.

The first proposal of this type was probably that of Bohm according to which the nonlocality is the most essential and inescapable aspect of quantum theory: a system cannot be analyzed in parts whose properties do not depend on the state of the whole system, no matter how well separated in space these parts may appear⁵⁷³ (Selleri *causalità impossibile* 1987, pag. 118).

The following solution introduces the interaction between large separated objects and the measuring apparatus:

The nonergodic interpretation. [It] assumes that a sequence of quantum objects, even if separated by large time intervals from one another, do not behave independently in their interaction with the measuring apparatus. The basic idea is that these objects may essentially interact with each other, by means of memory effects in an hypothetical medium filling the space crossed by them, on their way toward the measuring instruments. [...] This nonergodic interpretation of quantum mechanics can, in principle, solve the EPR paradox, because it can explain the apparent violations of local realism as due to nonergodic effects within a strictly local theory (Selleri *EPR History* 1988, pagg. 55-56).

The following solution search for a reconciliation between QM and locality:

Negative probabilities. The idea of negative probabilities has been entertained in different times by physicists such as Dirac and Feynman. [In 1982] a “negative-probability solution” of the EPR paradox has been proposed by Mückenheim⁵⁷⁴ [...] [who built] a negative-probability *local* hidden-variable model that reproduces all the predictions of quantum theory for the “singlet” state of two spin- $\frac{1}{2}$ particles. [...] A local model is thus able to reproduce the quantum-mechanical violations of Bell’s inequality, if negative probabilities are introduced.

⁵⁷³ The Italian original text is in Addendum 3, pp. 1004-1005.

⁵⁷⁴ Wolfgang Mückenheim (1949-): See https://de.wikipedia.org/wiki/Wolfgang_M%C3%BCckenheim. Disposable only in German. See also https://www.researchgate.net/profile/Wolfgang_Mueckenheim. Both accessed 24 September 2020. Selleri also published a paper answering to Mückenheim, who questioned on the Selleri’s thoughts on a Stimulated Emission in Laser Media induced by empty waves (see section 2.8.4 in the following).

It has also been shown [Ivanonic, 1978] that the introduction of complex probabilities into the EPR paradox can reconcile locality with the quantum-mechanical predictions (Selleri EPR History 1988, pagg. 57-58. *Italics in the original text*).

Variable probabilities. The idea of “variable probabilities” as a solution of the EPR paradox starts from the evidence provided by the experiments performed with atomic photon pairs and assumes that the inequalities of the strong type (deduced from local realism *and* from additional assumptions) are violated. [...] The point of view adopted with this line of research is that not local realism but the additional assumptions should be blamed for the failure of the strong inequalities. One must then study local models of reality in which the logical negation of the additional assumptions is explicitly taken as true. The interesting models should thus imply the simultaneous validity of the following three statements [i.e., CHSH, CH, and GR property]. (Selleri EPR History 1988, pagg. 58-59. *Italics in the original text*).

Then Selleri introduces his comments, still proposing in 1988 how a possible solution between QM and LR rests in some experiments different by the pairs of atomic photons but in particle physics (neutral kaons, which Selleri studied at least since 1983) as well as in other paths or proposed experiments:

[...] From a general point of view, one can maintain that local realism cannot be proved wrong by experiments designed for testing the strong inequalities. Only if weak inequalities could be tested, could a crucial confrontation between quantum theory and local realism finally take place. This appears unlikely in the foreseeable future as far as experiments with pairs of atomic photons are concerned. The situation is, however, better for some proposed particle-physics experiments and for experiments with pair of atoms, since detectors operate, in these cases, nearer to the ideal behavior. Even in the case of low-efficiency detectors there are interesting investigations to be carried out, for example, by replacing the usual additional assumptions (the CHSH, CH, and GR assumptions) with more physical restrictions. After all, it is unlikely that the considerable disagreement between quantum theory and local realism for high-efficiency detectors becomes perfect agreement for low-efficiency detectors! For instance, it would be interesting to study the use of symmetrical functions for describing the detection processes of the two photons, since it has been shown by Caser (1984) that the quantum-theoretical predictions cannot, in such a case, agree with the factorizable probabilities of Clauser and Horne.

It is interesting to recall that the idea of variable probabilities presents itself as a natural consequence of probabilistic local realism.

[...] Also for this reason, it would be very interesting to carry out the [following] experiments: 1) Insertion of atomic source of photon pairs in a magnetic field of about 200 or 300 gauss. 2) Use of $\lambda/4$ plates for systematic measurements of circular polarizations. 3) Search for small terms proportional to $\cos 4(a - b)$ in the correlation function $P(a,b)$ (Selleri EPR History 1988, pagg. 59-60. My underline).

In an email written to Tobias Schwaibold, at Springer, concerning his books and their presence in a Springer list of books to be saved, Selleri dealt with his paper, “History of the Einstein-Podolsky-Rosen paradox” (present in the book *Quantum Mechanics versus Local Realism: The Einstein-Podolsky-Rosen Paradox*), emphasizing how it “is one of my best papers of all times”.

In (Afriat, Selleri 1999), eleven year later, he also recalled the diverse abovementioned solution, dealing in addition with:

- the enhanced particle detection, undertaken by R. Risco Delgado, and applied at the cases of one detector, a detector and a polarizer, a detector and two polarizers
- a correlated detections of two photons, applied at the cases of two detectors, two detectors and a polarizer, two detectors and two polarizers, two detectors and two nonideal polarizers
- the experiments with three polarizers.

And with other realist proposals, as, for instance:

- Chaotic ball model by Caroline Thompson⁵⁷⁵. Selleri said that Thompson “concluded that the very large violations of Bell-type inequalities are unlikely to be explained by the detection loophole alone. There may be another major factor, an ‘emission loophole’ coming into play” (Afriat, Selleri 1999, pag. 232). In another part of their book, Afriat and Selleri describe Thompson model, pointing out the importance of low-efficiency detectors for the violation of inequalities. Bell and EPR are always strictly connected:

⁵⁷⁵ Caroline H. Thompson (1943-2006): See https://en.wikipedia.org/wiki/User:Caroline_Thompson. Accessed 24 September 2020.

Thompson's "chaotic ball" model approaches the detection loophole problem from another angle. It presents an intuitive picture, with hidden variables assumed to be distributed over the surface of a sphere. She dismisses certain strong inequalities as invalid: those that estimate probabilities as a ratio with the observed sum of coincidences as the denominator [...]. The "fair sampling" assumption necessary for their validity seem reasonable under quantum mechanics but is totally unreasonable for a realist. The chaotic ball model makes it clear that there are circumstances in which the less efficient the detector the greater the violation of these inequalities.

This latter fact is of great importance. Experimenters openly assume the validity of the quantum-mechanical inference that imperfections in the apparatus can only decrease the violations. No one so far appears to have fully appreciated that imperfections in the apparatus can increase violations with realist models. Because of this assumption, experimenters have (practically) felt free to choose the instrumental settings that gave the strongest violations. It is very unfortunate that Nature appears to encourage bias here, providing all sorts of temptations in the way! As the usual purpose of EPR experiments is to produce an interference pattern (which is what a coincidence pattern is wrongly perceived to be) and to make it as clear as possible so as to study the positions and spacings of the peaks, how can experimenters be expected to resist opportunities to "improve" it?

[...] Thompson believes that a modest investment in a series of experiments designed to look into the possible realist explanations of published EPR experiments would tell physicists more than the most elaborate "loophole-free" experiment. [...] Low-efficiency detectors should give the most important information, and she would expect to find clear evidence that the quantum-mechanical approach is inadequate (Afriat, Selleri 1999, pagg. 233-234. *Italics in the original text. My underline.*)

- Restricted Quantum Mechanics (RQM), investigated, among others, by M. Ferrero, T. Marshall, E. Santos⁵⁷⁶. Selleri referred to a 1997 paper by Ferrero and Santos⁵⁷⁷ and he deep analyzed their work⁵⁷⁸:

⁵⁷⁶ Miguel Ferrero Melgar: See: <http://www.worldcat.org/identities/lccn-n85221654/>. Accessed 24 September 2020. It is very hard to find some information about him.

Trevor W. Marshall: See: <https://www.researchgate.net/scientific-contributions/7549484-Trevor-W-Marshall>. Accessed 24 September 2020. As I already told at page 403, in a letter to Costa de Beauregard dated 24 May 1999 Selleri "remember the horrible style with which Wheeler stopped Trevor Marshall's career". It is very hard to find some information about him.

Emilio Santos Corchero (1935-): See https://en.wikipedia.org/wiki/Emilio_Santos_Corchero. For his publication see https://www.researchgate.net/publication/225634645_Publications_of_Emilio_Santos Both accessed 24 September 2020.

⁵⁷⁷ Ferrero M., Santos E. (1997), "Empirical consequences of the scientific construction. The program of local hidden-variables theories in quantum-mechanics, *Foundations of Physics*, 27 (6), pp. 765-800.

⁵⁷⁸ Selleri collaborated with them and with Marshall, writing several papers.

They state what realism means to them: it is the ontological principle according to which *there is an external world, independent of the subject and his mind, that already existed before it and that will continue to exist when the subject perishes*. Furthermore, this independent external world *can be known*. [...] The realist postulate [...] is not an arbitrary assumption because the active relation between the subject and the world in which he operates is what proves *a posteriori* the existence of an objective reality.

[...] Ferrero and Santos propose the adoption of a “restricted quantum mechanics” (RQM) instead of the usual theory. The main differences with respect to standard quantum theory are that in RQM there are no measurement postulates, and it is neither claimed that all state vectors represent physical states, nor that all Hermitian operators represent observables. The contradiction of such a RQM with local realism cannot be shown as a theorem, e.g., because the violation of Bell’s inequality cannot be deduced. From a conceptual point of view the two authors argue that noise has been underestimated in quantum theory: it does not appear in quantum mechanics, but it becomes essential in quantum field theory. They conjecture that noise will prevent the violation of Bell’s inequality and, more generally, will eliminate all incompatibility with local realism. Bell’s “conjecture” of an irreducible incompatibility between local realism and quantum mechanics, according to them, is false and hence not a theorem. This implies that there are mistakes in the many published proofs of Bell’s conjecture. [...] The conclusions of this important line of thinking [by Ferrero and Santos] are the following:

1. The principles of realism and locality are fundamental for the construction of physics and therefore ought to be accepted.
2. Local realism leads necessarily to an interpretation of quantum mechanics in terms of hidden variables.
3. It is not true that Bell’s inequality has been empirically violated.
4. If the measurement postulates are removed from quantum theory and the vectors-states and operators-observables relations are weakened, then it is not possible to prove “Bell’s theorem”.
5. Quantum noise will probably prevent an empirical refutation of local hidden variables (Afriat, Selleri 1999, pagg. 234-238. *italics in the original text. My underline*).

Selleri, still recalling how the first three foregoing conclusions are very similar to his, never misses the opportunity to emphasize once again his thought and his line of research:

[...] the incompatibility between quantum mechanics and local realism simply arises from the existence of nonfactorizable state vectors, e.g., for two neutral kaons, which do

not depend on the parts of quantum axiomatics that Ferrero and Santos consider superfluous. The conclusion proposed here is that quantum theory shall undergo more drastic modifications than the ones envisaged by Ferrero and Santos before it can become compatible with local realism (Afriat, Selleri 1999, pag. 238. My underline)

Also in the foregoing pages Selleri showed his strong skill in submitting a deep state-of-the-art connected to a specific topic, quoting the scholars who worked at it, and still proposing some experiments.

Pointing out the importance of causality in Selleri's research, he devoted a whole chapter to causality in his book *La fisica tra paradossi e realtà. Le critiche dei fisici realisti alla teoria dei quanti*. Talking about EPR, he dealt with the differences between causality and determinism, leaning towards the former. Preferring causality to determinism, as Selleri choose to do, could be useful to better counteract the von Neumann theorem. He preferred to give up to determinism, still defending a causal picture of the atomic processes:

Thankfully, there are philosophers of science who still defend causality. [...] An important distinction to be made when considering the natural processes that science deals with, is between causality and determinism. On this ground it must be said that much has been done in the twentieth century by philosophers of science and little by physicists, who have overcome the determinism of classical physics with quantum probabilistic laws, but then concluded that no causal description is possible.

[...] the anti-deterministic conclusions based on von Neumann's theorem were unfounded. Similar conclusions have been drawn for Heisenberg's relations and for Bohr's complementarity [...], so much so that today it can be said that a deterministic description of atomic processes has become possible. However, the fact that it is possible does not mean that it is desirable, because random processes are very common in the atomic domain and it seems a stretch to look for a strictly deterministic description. Moreover, it can be a "tactical" mistake to rely on a somewhat extreme description like determinism, when a convenient causal description is widely possible. This conclusion is reinforced by thinking of the recent history of causality and the strong repression generated by von Neumann's theorem on realistic trends.

If it was immediately understood that the von Neumann's theorem addressed the problem of determinism but nothing could say against causality, much space would be diverted from the rampant influence of the Copenhagen school, renouncing determinism but continuing to defend a causal view of atomic processes (Selleri 2001-2005, pag. 23. My underline).

[One can] interpret many results of quantum mechanics without renouncing causality, though not all of them. [...] in the case of the paradox of Einstein, Podolsky and Rosen the principle of causality in any reasonable formulation leads to empirical predictions conflicting with existing theory⁵⁷⁹ (Selleri 2001-2005, pag. 25. My underline).

Afriat and Selleri emphasized the ontological violation with respect to QM that came from the application of EPR reality criterion:

The very possibility of applying the reality criterion of Einstein *et al.* and deducing Bell's inequality represents a kind of *ontological* violation, not only of complementarity but also of Heisenberg's uncertainty relations. It is a violation accepted only by realists, however [...] (Afriat, Selleri 1999, pag. vii. *Italics* in the original text).

They used a statement by Feynman, Leighton, and Sands book⁵⁸⁰:

The uncertainty principle "protects" quantum mechanics. Heisenberg recognized that if it were possible to measure momentum and position simultaneously with a greater accuracy, quantum mechanics would collapse. So he proposed that it must be impossible. Then people sat down and tried to figure out ways of doing it, and nobody could figure out a way to measure the position of anything - a screen, an electron, a billiard ball, anything - with any greater accuracy. Quantum mechanics maintains its perilous but still accurate existence (cit. in Afriat, Selleri 1999, pag. vii).

Selleri devoted some books to the debate between the greatest scholars on the LR's three postulates as well as their approach to them, again underscoring the importance of reality, locality and time arrow. The main one is *Die debatte um die Quantentheorie*, translated from the above original German text into French, Spanish, Greek, Japanese and Italian. And in English with a different title: *Quantum Paradoxes and Physical Reality*. A book which received a lot of endorsements as well as the application for the Lakatos Award in 1991.

⁵⁷⁹ The original Italian text is in Addendum 3, p. 1005.

⁵⁸⁰ Feynman R.P., Leighton R.B., Sands M. (1965), *The Feynman Lectures on Physics*, Reading: Addison Wesley, vol. III, pagg. 1-11.

Once again he showed himself to stand against the entanglement (which Selleri called “entangled wave functions”), of which he declared still in 2005, despite he was disavowed by the experiments:

[...] research on teleportation makes sense if non-factoring state vectors properly represent the properties of nature, which [...] is a very, very open question. Several experiments have been carried out on teleportation [...] and the results have been much emphasized in the media, but there is still no evidence that teleportation is possible. Just like the EPR paradox, the possibility of teleportation stems from entangled wave functions. From this point of view there is full logical equivalence between violations of weak inequalities and teleportation: both would be evidence of the existence of entangled wave functions. But, in my opinion, it is unlikely that such evidence will ever be found⁵⁸¹ (Selleri Paradossi, 2001, pag. 160My underline).

Then Selleri, in order to make the reality criterion and the whole LR even more general, expanded its research by introducing the Probabilistic Local Realism (further referred to as PLR).

In place of the EPR reality criterion one has to introduce a probabilistic reality criterion that attributes real physical properties to statistical ensembles for which probabilities can be predicted. These “real physical properties” are similar to the propensities of Popper’s realism.

When the locality assumption is added, one obtains a very general formulation of local realism that leads, for example, to a new proof of Bell’s theorem (Afriat, Selleri 1999, pag. 86).

For a realist, the EPR reality criterion has a rather solid conceptual basis. However, it can be criticized in two not entirely independent ways. First, it can be observed that it has a rather narrow applicability, since it recognizes as real in the atomic domain, in practice, only what quantum mechanics describes with eigenstates of the measured observable. Why cannot we [...] introduce elements of reality for all those situations that theory describes in a more general way, through superpositions?

⁵⁸¹ [...] le ricerche sul teletrasporto hanno senso se i vettori di stato non fattorizzabili rappresentano correttamente le proprietà della natura, il che [...] è una questione molto, molto aperta. Diversi esperimenti sono stati compiuti sul teletrasporto [...] e i risultati sono stati molto enfatizzati sui mezzi di comunicazione di massa, ma non esiste ancora alcuna prova che il teletrasporto sia possibile. Proprio come il paradosso di EPR, la possibilità del teletrasporto nasce dalle funzioni d’onda ingarbugliate. Da questo punto di vista c’è piena equivalenza logica fra violazioni delle disuguaglianze deboli e teletrasporto: entrambe le cose sarebbero prova dell’esistenza delle funzioni d’onda ingarbugliate. Ma, a mio parere, è difficile che una tale prova sia mai trovata (Selleri Paradossi, 2001, pag. 160. My underline).

Einstein, Podolsky and Rosen were well aware of this limitation, so much so that they wrote that their criterion “far from exhausting all possible ways of recognizing a physical reality, at least it provides us with one of these ways” (EPR 1935, pag. 778). The second weakness [...] lies in the predictability with certainty of the considered physical quantity: when can one ever be absolutely certain of something in a concrete physical situation? The certain predictability of 1935 seemed to be a consequence of quantum theory for certain types of state vector (eigenfunctions). However, the situation is different today [...] after the theorem of Wigner, Araki and Yanase, which demonstrated the need for imperfect measures, even in principle, if the quantum theory must be consistent with the well-known laws of conservation [...] even in its description of measurement processes. These difficulties can both be overcome by means of a probabilistic generalization of the reality criterion, in which the prediction of a probability is used to infer the existence of real physical properties of appropriate statistical sets.

Local probabilistic realism can be formulated in such general terms that many should find it easy to accept. Indeed: (a) if there were not some form of reality behind a stable and predictable probability, then there would not really remain any kind of material reality; (b) even the same probabilities of quantum mechanics are local in the sense [...] of probabilistic local realism⁵⁸² (Selleri Paradossi, 2001, pagg. 142-143. My underline).

In his paper “Even local probabilities lead to the paradox”⁵⁸³, Selleri, just referring to Einstein locality and to LR as synonymous, stated the advantages inherent to PLR, as well as the incompatibility between the PLR and the QM:

The probabilistic Einstein locality [a generalization of Einstein locality] has two very important advantages: First, it is not limited conceptually to those rare (or even nonexistent) cases in which one is able to make *totally certain* predictions. Second, it can be applied to all types of quantum-mechanical descriptions (i.e., eigenstates, superpositions, and mixtures) and not only to eigenstates of the predicted observables. *Thus whenever one can predict values of probabilities our probabilistic approach can be applied, no matter what the source of information leading to the prediction* (e.g, trusted theory, previous experiment). This present form of Einstein locality is so general, and rests on such a weak type of realism, that one wonders if even the physicists of the Copenhagen and Göttingen schools could not have accepted it. It will however be shown that it nevertheless suffices for the derivation of Bell-type inequalities.

⁵⁸² The original Italian text is in Addendum 3, pp. 1005-1006.

⁵⁸³ Selleri F. (1988), “Even Local Probabilities Lead to the Paradox”, in *Quantum Mechanics versus Local Realism: The Einstein, Podolsky, and Rosen Paradox*, Selleri F. (ed), pp. 149-174, London/New York: Plenum. (Selleri Even 1988) in the following.

Since these latter inequalities are violated by quantum-mechanical predictions, it follows that not even the present weak form of local realism is compatible with existing quantum theory (Selleri QM vs LR 1988, pag. 162. *Italics in the original text. My underline*).

Inside his research on PLR, Selleri pointed out the role played by Karl Popper's propensity:

A new description of physical reality in probabilistic terms based on the concept of propensity was proposed by Popper, but unrelated to the EPR paradox. The proposed aim was to reformulate the calculation of quantum probabilities in objective and realistic terms, without reference to the usual idealistic concepts of the Copenhagen school.

The writer has tried to develop this program and believes to have demonstrated that any objectivistic and local formulation of probability leads to the validity of Bell's inequality and therefore to the impossibility of a full agreement with the existing quantum mechanics⁵⁸⁴ (Selleri Paradossi, 2001, pag. 143).

In his 1989 book, *Fisica senza dogma. La conoscenza scientifica tra sviluppo e regressione*⁵⁸⁵, Selleri pointed out that:

[...] the concept of propensity expresses a tendency inherent in the studied object to produce certain outcomes when it is allowed to evolve in a certain environment. The tendency should be understood with its numerical "intensity" and not simply as a mere qualitative possibility to produce a certain evolution.

The frequency with which that evolution will manifest itself among the many possible ones will be a quantitative measure of the intensity of the tendency, that is, of the propensity. Thus posed the problem, [...] propensity is a term quite similar to probability. And in fact Popper says that: "the idea of propensity is the best interpretation I know of the application of probability calculation to a certain kind of 'repeatable experiments', especially in physics [...]"⁵⁸⁶ (Selleri Dogma 1989, pag. 181).

The application of the propensity can lead to introduce what Selleri called the probabilistic reality criterion:

A probability cannot be deduced from a single measuring act and individual propensities remain not measurable.

⁵⁸⁴ The original Italian text is in Addendum 3, p. 1006.

⁵⁸⁵ Selleri F. (1989), *Fisica senza dogma. La conoscenza scientifica tra sviluppo e regressione*, Bari: Edizioni Dedalo. (Selleri Dogma, 1989) in the following.

⁵⁸⁶ The original Italian text is in Addendum 3, p. 1006.

For a set of atomic systems, however, one can introduce the propensities to give different results at different frequencies. These latter propensities, on the other hand, are easily measurable and can be considered a kind of physical average of the individual non-observable propensities. Thanks to the concept of propensities, the probabilistic reality criterion can be formulated. Let a set S of physical objects of the same type (e.g. electrons) be given, an experimental apparatus A capable of measuring the physical magnitude F of electrons, and let f_1 and f_2 be the only two possible results of F [...]. The probabilistic reality criterion can be formulated as follows:

if it is possible to predict the existence of a subset S' of S without disturbing in any way the S' and S' component objects; if it is also possible to correctly predict that future measurements of F on S' will give the results f_1 and f_2 with respective frequencies p_1 and p_2 then there is a real physical property λ' (propensity) belonging to S' which determines the values of the probabilities and we have $p_1=p_1(\lambda')$ $p_2=p_2(\lambda')$ ⁵⁸⁷ (Selleri Paradossi, 2001, pag. 144. My italics).

[...] There is no area of phenomena to which this new criterion of reality cannot be applied. Its generality is such that it may perhaps seem completely harmless. In reality, coupled with the ideas of locality and the arrow of time, it constitutes a powerful weapon and allows, in particular, to demonstrate the validity of those inequalities of Bell that the existing quantum theory instead violates⁵⁸⁸ (Selleri Paradossi, 2001, pag. 145).

Even if Selleri arrived later to EPR, having started his research with Bell's inequality, nevertheless he recognized its deeply importance in the opportunity to save causality as well as to create a local and realistic alternative theory to QM. And he also recognized the intrinsic difficulty inherent in the EPR paradox and its Bell's form.

In every phase of his research, he was really attentive to introduce a current state-of-the-art of the diverse scholars who worked in a specific field, in order to provide the maximum possible information and knowledge of the different views.

2.7.9-SHOWING SOME SELLERI'S PAPERS TO EMPHASIZED HIS RESEARCH. STRESSING QUOTATIONS TO HIS WORK

Now I am going to introduce several papers that Selleri wrote alone or jointly with other scholars and linked to the previous topics, as to show more his

⁵⁸⁷ The original Italian text is in Addendum 3, pp. 1006-1007.

⁵⁸⁸ The original Italian text is in Addendum 3, p. 1007.

approach to research in FQM and the various paths he experienced, included the unsuccessful and the abandoned ones.

In his paper “A Stronger Form of Bell’s Inequality”⁵⁸⁹, (Selleri 1972 in the following), Selleri started giving a simple way to prove the stronger inequality. He argued that:

It has been proven by Bell and by Clauser, Holt, Horne and Shimony [J. F. CLAUSER, A. HORNE, A. SHIMONY and R. A. HOLT: *Phys. Rev. Lett.*, 23, 880 (1969) ref. (2)] that the correlation functions calculated according to local hidden variable theories satisfy the inequality (1) $|P(a, b) - P(a, b')| + |P(a', b) + P(a', b')| \leq 2$.

Here $P(a, b)$ is a suitably defined correlation function, related to the measurements in two separated regions of space of the dicotomie observables $A(a)$ and $B(b)$. The parameters a, b, a' and b' refer to the settings of the experimental apparatuses and the possible values of A and B are $A = \pm 1, B = \pm 1$

It is the purpose of the present note to show that the stronger inequality

(2) $|P(a, b) - P(a, b')| + |P(a', b) + P(a', b')| \leq 2$ can be proven in a very simple way.

Using the notation of ref (2), the correlation function $P(a, b)$ is defined by

(3) $P(a, b) = \int d\lambda \rho(\lambda) A(a, \lambda) B(b, \lambda)$, where λ is the local hidden variable (or a set of these variables). Strictly, similar definitions hold for the other correlation functions entering in (1) and (2).

Following the initial part of (1) given in ref (2) one has

(4) $|P(a, b) - P(a, b')| \leq \int d\lambda \rho(\lambda) |A(a, \lambda) B(b, \lambda) - A(a, \lambda) B(b', \lambda)| = 1 - \int d\lambda \rho(\lambda) B(b, \lambda) B(b', \lambda)$.

Notice now that the last side of (4) does not depend on a and furthermore that all minus signs in (4) can be replaced by plus signs. Therefore

(5) $|P(a', b) + P(a', b')| \leq 1 + \int d\lambda \rho(\lambda) B(b, \lambda) B(b', \lambda)$.

Summing (4) and (5) one gets (2).

Bell showed that the “singlet” state of the observables A and B violates (1). It is, in fact, possible to prove⁵⁹⁰ the stronger conclusion that in the absence of superselection rules *all* state vectors of the second type (of which the “singlet” state is just an example) violate (1) and (2) for suitable choices of the parameters a, b, a' and b' (Selleri 1972, pagg. 581-582. *Italics* in the original text).

Selleri also wrote several papers on the state-vectors of the first and second type known as factorizable and nonfactorizable state, respectively⁵⁹¹.

⁵⁸⁹ Selleri F. (1972), “A Stronger Form of Bell’s Inequality”, *Lettere al Nuovo Cimento*, 3 (14), pp. 581-582. (Selleri 1972 in the following).

⁵⁹⁰ Selleri cited his future article, Capasso V., Fortunato D., Selleri F. (1973), “Sensitive Observables of Quantum Mechanics”, *International Journal of Theoretical Physics*, 7 (5), pp. 319-326, with a different title, “Second-type state vectors in quantum mechanics”, which was modified, underlining how it was only submitted to the same journal, without being published yet.

⁵⁹¹ First and second-type state vectors revealed several representation: pure states and mixtures; mixture of the first type and of the second type. The second type state vector are the current entangled states.

In his book (Selleri causalità impossibile 1987), Selleri stated how the state-vectors of the second type are responsible for the occurrence of the diverse paradoxes related to QM:

It is interesting to note that the violations of local realism contained in quantum mechanics do not depend on any difficulties related to the representation of measurable (“observable”) quantities by operators. [...] It is instead from the algebraic properties of the wave function describing two related systems that the incompatibility with local realism arises. But not even all wave functions generate violation of Bell’s inequality: it is in fact possible to introduce two types of wave function. Of the first type are those factorizable [...], while of the second type are those non factorizable. Well, only the states of the second type generate the EPR paradox, while those of the first type are fully compatible with Bell’s inequality! Everything happens as if the principle of overlapping, which implies the existence of nonfactorizable states, was responsible for the paradox⁵⁹² (Selleri causalità impossibile 1987, pag. 107. *Italics* in the original text. My underline).

Therefore, for two related systems, all nonfactorizable state vectors lead to a violation of Bell’s inequality and thus to incompatibility with local realism.

In 1936, Furry was probably the first to reveal the use of these different descriptions for two separate system in QM.

Their importance lied in the fact that, according to a note that I found in Selleri’s archive:

All the unresolved difficulties of quantum mechanics (EPR paradox, Bell’s inequality, measurement theory, etc.) arise from the presence in the theory of nonfactorizable state vectors.

This situation led many physicists to believe that quantum mechanics could be reformulated with only factorizable state vectors. In (Fortunato, Garuccio, Selleri 1977)⁵⁹³ an observable has been deduced whose value differs considerably depending on the type (factorizable or not) of state vector associated to the studied physical system.

The concrete measurement of this observable was carried out by Clauser. Subsequently, however, it has been demonstrated in (Garuccio, Scalera, Selleri 1977)⁵⁹⁴ that a formulation of quantum mechanics without nonfactorizable state vectors led to a theory with new and very serious paradoxical aspects⁵⁹⁵ (FSPA-My underline).

⁵⁹² The original Italian text is in Addendum 3, p. 1007.

⁵⁹³ Fortunato D., Garuccio A., Selleri F. (1977), “Observable Consequences from Second-Type State Vectors of Quantum Mechanics”, *International Journal of Theoretical Physics*, 16 (1), pp. 1-6.

⁵⁹⁴ Garuccio A., Scalera G., Selleri F. (1977), “On Local Causality and the Quantum-Mechanical State Vector”, *Lettere al Nuovo Cimento*, 18 (1), pp. 26-28.

⁵⁹⁵ The original Italian text is in Addendum 3, p. 1007.

According to Gino Tarozzi⁵⁹⁶, in a book he edited with Alwin van der Merwe⁵⁹⁷:

[...] It was, however, Bohm and Aharonov, followed in this approach by Jauch, who propose the elimination of the second-type state vector as the cause of the EPR paradoxes, attempting to refound quantum formalism without this ‘inconvenient’ mathematical entity.

[...] [Fortunato, Garuccio, Selleri 1977] [...] showed the failure of the Furry (or Bohm-Aharonov) hypothesis to be a purely formal solution to the Einstein-Bell problem (Tarozzi, van der Merwe eds 1988, pag. 39).

Another paper, which was also cited by Jammer in his book was “Sensitive Observables of Quantum Mechanics”⁵⁹⁸, written by Selleri with Capasso and Fortunato. They stated:

Second type state-vectors are responsible for many paradoxes of QM (ERP, Schrödinger’s cat, theory of measurement) and, in general, for the inconsistency of a realistic philosophy with the quantum mechanical axioms (For a clear discussion of this point see the quoted book by d’Espagnat) (Capasso, Fortunato, Selleri 1973, pag. 326).

The authors claimed in the abstract how the observables in QM could be sensitive to the state-vectors of first and second type:

We prove that there are quantum mechanical observables which are sensitive to the type of state-vector (first type or second type) describing two correlated physical systems, in the sense that the expectation value of these ‘sensitive observables’ is measurably different in the two cases. The proof centers around Bell’s inequality since we show that in quantum mechanics for *all* state-vectors of the second type (and only for them) sensitive observables exit in the absence of super-selection rules. Experimental verification of the existence of sensitive observables rules out local hidden variables (Capasso, Fortunato, Selleri 1973, pag. 319).

⁵⁹⁶ Gino Tarozzi is an Italian philosopher of science who experienced a long relationship with Selleri, co-organizing some conferences, co-writing interesting articles, and edited important books in which he analyzed also Selleri’s work. A remarkable source that I cited in the following.

⁵⁹⁷ Tarozzi G., van der Merwe A. Eds. (1988), *The Nature of Quantum Paradoxes. Italian Studies in the Foundations and Philosophy of Modern Physics*, Kluwer Academic Publishers. (Tarozzi, van der Merwe eds 1988) in the following.

⁵⁹⁸ Capasso V., Fortunato D., Selleri F. (1973), “Sensitive Observables of Quantum Mechanics”, *International Journal of Theoretical Physics*, 7 (5), pp. 319-326. (Capasso, Fortunato, Selleri 1973) in the following.

So, they introduced three theorems, showing firstly sensitive and indifferent observables. Concerning sensitive observable, Tarozzi stated it is:

[...] a term used to define those physical magnitudes whose expectation value on a state of the second type [...] is observably different from the one for any mixture of first-type state vectors (Tarozzi, van der Merwe eds 1988, pag. 39).

Then, with the second theorem, Capasso, Fortunato and Selleri revealed how Bell's inequality was always satisfied for a mixture of the first type and, above all, with the Theorem III, how, "for a mixture of the second type the observables [...] can be chosen in such a way that the [Bell's] inequality is not satisfied if no super-selection rules are present" (Capasso, Fortunato, Selleri 1973, pag. 323). They gave the proof of the Theorem in a two-dimensional Hilbert space for simplicity, underlining that this proof was useful in real simple cases. Among them, they spoke about neutral kaons K_0 and $K_{0\text{bar}}$ ⁵⁹⁹.

Anyway, their conclusion are always focused on the opportunity to perform further experiments quite similar to the 1972 Clauser-Freedman one. It is worth pointing out how Selleri in the following decades tried to propose new inequalities and new experiments, always standing up for the validity of Bell's inequalities (in the weak form) as well of LR:

It should be stressed that our main result (Theorem III) was proved by implicitly assuming that all projection operators are observable.

This is not true when super-selection rules are present and therefore mixtures of the second type may not be observably different from local hidden variables theories (in the sense of Bell's inequality) when there are super-selection rules. This turns out in fact to be the case for isotropic-spin eigenstates.

It is very interesting to notice that Jauch's definition of state of a physical system led him to the conclusion that mixtures of the second type do not exist. Therefore, in view of Theorem III, the experiment performed by Clauser & Freedman (1973) can be considered as the first direct experimental check of the existence of state-vectors of the second type.

⁵⁹⁹ $\overline{K_0}$. Nevertheless, using this character breaks up all the text, so I preferred to indicate it as "k crossed out" using the abbreviation *bar*, that is, $K_{0\text{bar}}$, K_{bar} , and so on.

In view of the delicacy of this experiment and of the great conceptual value of its implications it seems very important to perform similar experiments in other cases before ruling out definitely local hidden variables theories (Capasso, Fortunato, Selleri 1973, pag. 326).

In “Sensitive Observables On Infinite-Dimensional Hilbert Spaces”⁶⁰⁰, written with Fortunato, Selleri made a generalization of the outcomes found in (Capasso, Fortunato, Selleri 1973) and so far limited to a two dimensional Hilbert space.

Let Σ be a physical system consisting of two subsystems, S and T: we prove that there are, in the absence of superselection rules, quantum mechanical observables (called “sensitive”), whose expectation value depends on the type of state vector (first type or second type) describing Σ . This result generalizes a previous one obtained under the restriction that the Hilbert spaces of S and T are two dimensional (Fortunato, Selleri 1976, pag. 333).

With the paper, “Observable Consequences from Second-Type State Vectors of Quantum Mechanics”⁶⁰¹, coauthored with Fortunato and Garuccio, Selleri started finding possible theoretical differences researching for experimental opportunities to reveal the differences between first and second type state vectors, soon thinking to experimental opportunities to test the role of the latter. They stated:

Starting from a “singlet” state vector for two correlated systems we find an observable whose expectation value is 3 according to QM, while it has a maximum value of 1 if only state vector of the first type are considered. This allows a much easier experimental check of the hitherto unobserved state vectors of the second type than suggested by Bell’s inequality (Fortunato, Garuccio, Selleri 1977, pag. 1).

[...] The distinction between these two types of state vectors has received increasing attention in recent years (d’Espagnat 1965, Jammer 1974, Jauch 1971) when it has been realized, on the one hand, that most of the unresolved fundamental difficulties of QM (EPR-type paradoxes, theory of measurement and “reduction of the wave-packet”,

⁶⁰⁰ Fortunato D., Selleri F. (1976), “Sensitive Observables in Infinite-Dimensional Hilbert Spaces”, *International Journal of Theoretical Physics*, 15 (5), pp. 333-338. (Fortunato, Selleri 1976) in the following.

⁶⁰¹ Fortunato D., Garuccio A., Selleri F. (1977), “Observable Consequences from Second-Type State Vectors of Quantum Mechanics”, *International Journal of Theoretical Physics*, 16 (1), pp. 1-6. (Fortunato, Garuccio, Selleri 1977) in the following.

unreconcilability with local causality) can be traced back to the existence of state vectors of the *second* type. And, on the other hand, that no *direct* experimental verification of the existence in nature of such vectors exists.

A concept useful to the experimental distinction between the two types of vectors is that of sensitive observables, which are defined as those observables whose expectation value over a statistical ensemble of N identical pairs of systems $S+T$ is different according to whether the N pairs are described by a mixture of state vectors of the first type, or by a state vector of the second type (Capasso, Fortunato, Selleri 1973).

[...] It has recently been found that a general theorem exists (and has been proved in *Sensitive Observables On Infinite-Dimensional Hilbert Spaces*) that allows a much easier experimental test of the existence of state vectors of the second type than the one deduced from Bell's theorem.

In (Fortunato, Selleri 1976) the following theorem has been proved: *Theorem F*: If $|\eta\rangle$ is a state vector of the *second* type for two correlated systems S and T the projection operator $\Gamma_\eta = |\eta\rangle\langle\eta|$ represents a sensitive observable for the system $S+T$.

[...] a straightforward application of the previous theorem allows one to suggest an experiment that should provide a new and meaningful test of the quantum mechanical description of two correlated photons. (Fortunato, Garuccio, Selleri 1977, pagg. 1-2).

The sensitive observable changed if calculated over a single state or a mixture of state vectors of first type, namely a second type state vector. So, they argued, “this large difference should allow a rather easy experimental discrimination between state vectors of the first and of the second type” (Fortunato, Garuccio, Selleri 1977, pag. 4).

Then they consider a mixture of N identical pairs of photons, still finding differences, very similar to the one obtained from Bell's inequality, “which is always satisfied by state vectors of the first type but it is violated by those of the second type” (Fortunato, Garuccio, Selleri 1977, pag. 5).

They tried to compare their result with Bell's inequality. They proved to deduce their result from local causality, but they failed (“we did not succeed in so doing”). Anyway, they

[...] consider the present paper a modest contribution along the line of research going from the EPR paradox to Bell's inequality. The time seems to approach when choices about quantum mechanical paradoxes will be made by experiments (Fortunato, Garuccio, Selleri 1977, pag. 6. All *italics* are in the original text).

Finally, in the paper “On Local Causality and the Quantum-Mechanical State Vector”⁶⁰², as told before with Tarozzi, the authors found the impossibility to drop the second type state vectors, otherwise a lot of paradoxes were to rise. Nevertheless, they distinguished between the second type state vectors and the local causality. I recalled again how Selleri always upheld locality and causality.

Bell’s inequality deals with pairs of correlated systems: such systems are described by QM in a two-fold manner, sometime with factorizable state vectors (also called state vectors of the first type, whose set will be denoted by S_I in the following), some other time with nonfactorizable state vectors (called of the second type, set denoted by S_{II}). This distinction of S_I from S_{II} is important because the elements of S_I always satisfy (while those of S_{II} sometimes violate) Bell’s inequality.

This is not the only unpleasant feature absolute S_{II} because the famous paradox by EINSTEIN, ROSEN and PODOLSKY is due to the presence of S_{II} . Furthermore, all the unresolved difficulties met in trying to understand the description of the measurement given by QM arise from the need to get rid of nonfactorizable states from the final state of object plus measuring apparatus. Such a situation has led many physicists to believe that, perhaps, QM should be reformulated *without* S_{II} . Such an opinion has been expressed, for instance, by Jauch: “We may thus say that the essence of our new notion of state is contained in the statement: Mixtures of the 2nd kind do not exist”. Other authors have linked strictly nonlocality and S_{II} : Baracca writes: “...the states of the 2nd kind...give the most precise physical meaning which may be actually attached to the concepts of ‘locality’ or ‘nonlocality’ ”.

We shared for a long time the first of the two previous point of view, but we will show in the present paper that it is actually untenable, because the concrete formulation of such a naïve point of view (drop S_{II} and keep only S_I) would lead to a theory with new types of paradoxical aspects.

We will furthermore show that local causality and states of the second kind are two quite different things because there are locally causal physical systems which cannot be described by state vectors of the first type (Garuccio, Scalera, Selleri 1977, pag. 26. *Italics in the original text*).

[...] QM with vectors belonging to S_I is not the most general formulation of local causality (Garuccio, Scalera, Selleri 1977, pag. 27).

⁶⁰² Garuccio A., Scalera G., Selleri F. (1977), “On Local Causality and the Quantum-Mechanical State Vector”, *Lettere al Nuovo Cimento*, 18 (1), pp. 26-28. (Garuccio, Scalera, Selleri 1977) in the following.

[...] The conclusion is thus that local causality and a theory with only state vectors of the first type are two rather different things (Garuccio, Scalera, Selleri 1977, pag. 28).

In the paper “Nonlocal Interactions and Bell’s Inequality”⁶⁰³, Selleri and Garuccio expressed some reservations about the fact that local causality be the most essential ingredient of Bell’s inequality, still preferring the wave-particle dualism. In this paper the Bell-type experiments performed up to that moment were quoted:

We show that natural extensions of the local hidden-variable theories to include nonlocal effects still lead to a full validity of Bell’s inequality. It is conjectured that the essential point expressed by this inequality is not locality, but the wave-particle dualism.

Although the experimental situation cannot as yet be considered conclusive (Freedman-Clauser, 1972; Clauser 1976; Fry 1976; Holt-Pipkin, preprint), considerable evidence is starting to emerge for a violation of Bell’s inequality, at least for the space-time distances of the two correlated systems studied in the experiments performed up to now.

Therefore, it is interesting to analyse in detail the meaning and the physical implications of Bell’s inequality. It is often stated that the essential ingredient is the locality assumption which is made in the causal hidden-variable models used in deducing the inequality.

In fact the “quantum potential” for two correlated quantum systems is nonlocal, as shown for instance by Bohm and Hiley, a fact which seems to imply that a hidden-variable generalization of quantum mechanics is necessarily nonlocal. Nonlocal effects have been considered by some authors.

RAPISARDA has shown that local causality is only a necessary condition for the validity of Bell’s inequality and has conjectured that stronger inequalities may be found by imposing local causality also as a sufficient condition.

EDWARDS has studied a nonlocal hidden-variable model which never contradicts Bell’s inequality.

SCALERA has discussed a causal model, whose mathematical formalism satisfies Bell’s inequality and has the property of remaining unchanged when the correlations between the two subsystems are attributed to nonlocal effects.

Finally FLATO, PIRON, GREY, STERNHEIMER and VIGIER have conjectured that Bell’s inequality may lose validity in a substantial way as soon as even very weak nonlocal interactions are switched on.

⁶⁰³ Garuccio A., Selleri F. (1976), “Nonlocal Interactions and Bell’s Inequality”, *Il Nuovo Cimento*, 36 B (2), pp. 176-185. (Garuccio, Selleri 1976) in the following.

We show, however, that the conclusions of the latter authors seem unwarranted, since a slightly different calculation based on their approach leads to opposite conclusions.

[...] We discuss a class of nonlocal hidden-variable models which can be obtained by introducing nonlocal corrections (not necessarily small) to a local HV theory, and show that for the most natural formulations of such models Bell's inequality remains valid. [Then] we discuss the peculiar mathematical way used by quantum mechanics to violate Bell's inequality and show that no explicit nonlocal effects are contained in it. Violations are instead due to interference terms which are typical of the wave-particle dualism. The conjecture is made that the absence of dualism is the essential feature which distinguishes the HV models studied until now from quantum mechanics (Garuccio, Selleri 1976, pagg. 176-177. My underline).

[...] We have shown that here there are at least three classes of nonlocal effects [...] which do not lead to any violation of Bell's inequality. This does not mean, of course, that it is impossible to adjust the nonlocal effects in such a way as to violate Bell's inequality.

[...] We think that it is, nevertheless, remarkable that the most reasonable physical approaches to nonlocal effects lead to a full validity of Bell's inequality. This seems to imply that the most essential point expressed by Bell's inequality is not locality, but something else.

Essentially, we have shown that the amount of nonlocal effects compatible with Bell's inequality is very large. One then starts to wonder whether locality is really a sufficient condition for the validity of Bell's inequality (Garuccio, Selleri 1976, pag. 183).

[...] It seems, therefore, conceivable that a HV approach which keeps explicitly into account the wave properties of matter may lead to results similar to those of QM without any violations of Bell's inequality (Garuccio, Selleri 1976, pag. 184).

In (Selleri 1978), also cited by Clauser and Shimony, Selleri tried to weaken the additional assumptions normally adopted for create Bell-type experiments, most of which lead to a violation of Bell's inequality. In the abstract he stated that:

After some considerations about the equivalence of the objective local theories to the deterministic theories of Bell's type, a simple and systematic way to deduce inequalities from Einstein locality is introduced: All the inequalities deduced by Bell and by other authors, as well as several new ones, are so obtained. Some theorems are proven which show how striking the difference is at small angles between a correlation function satisfying Einstein locality and the quantum mechanical one.

Experiments at small angles involve weaker additional assumptions than those used up to now in experimental research on Bell's inequality (Selleri 1978, pag. 103).

Selleri wrote several articles centered on the hypothesis of Einstein locality, as, for instance, "On the Equivalence of Deterministic and Probabilistic Local Theories"⁶⁰⁴, written with A. Garuccio, and "Further Consequences of Einstein Locality"⁶⁰⁵, written with L. Schiavulli. In the former the authors claimed that:

Local hidden-variable theories have received an increasing attention in recent times because of their connections with the EPR paradox and because of the possibility to deduce from them inequalities for linear combinations of certain observables, called correlation functions, which are often violated by QM.

[...] The local hidden-variable theories have been formulated in two opposite ways.

The deterministic approach (*La Mécanique Quantique et la Réalité; On the Consequences of Einstein Locality; A Stronger Form of Bell's Inequality* et al: Bell 1965, d'Espagnat 1975 ecc.) starts from the assumption that, given the hidden-variable λ , the result of every single act of measurement is completely fixed.

[...] The second hidden variable approach (Bell and d'Espagnat at Varenna) is based on the assumption that λ determines only probabilities for the different outcomes of measurements on the two correlated particles.

[...] In a certain sense the opposition between deterministic and probabilistic local theories (DLT and PLT) looks too sharp to be true.

If one looks to the history of science one finds indeed that it was precisely the most deterministically oriented thinkers who introduced probabilistic considerations into physics: Laplace, Boltzmann and Einstein.

In the present paper we will essentially show that the class of all possible DLT's and the class of all possible PLT's are completely equivalent from an empirical point of view as far as inequalities for linear combinations of correlation functions are concerned (Garuccio, Selleri 1978, pag. 555-556).

⁶⁰⁴ Garuccio A., Selleri F. (1978), "On the Equivalence of Deterministic and Probabilistic Local Theories", *Lettere al Nuovo Cimento*, 23 (15), pp. 555-558. (Garuccio, Selleri 1978) in the following.

⁶⁰⁵ Schiavulli L., Selleri F. (1979), "Further Consequences of Einstein Locality", *Foundations of Physics*, 9 (5-6), pp. 339-352.

Another paper was the joint one written with Garuccio, entitled “Systematic Derivation of All the Inequalities of Einstein Locality”⁶⁰⁶, in which they explained the meaning of Einstein locality and showed an equivalence between deterministic and probabilistic local theories, still looking at many Bell’s inequalities:

We show that a method exists for deducing from Einstein locality all the possible inequalities for linear combinations of correlations functions. This allows us to show also that there is a complete observational equivalence of deterministic local theories and probabilistic local theories.

Einstein locality is fundamentally the hypothesis that the result of measurements on atomic systems are determined by “elements of reality” (sometimes called hidden variables), associated to the measured systems and/or to the measuring apparatus, which remain unaffected by measurements on other distant atomic systems. This determination can be either deterministic in the true philosophical sense, or probabilistic, in the sense that only the probabilities of the different outcomes of correlated measurements are fixed by the hidden variables. In either case, Einstein locality leads to several interesting empirical consequences, the best known of which is Bell’s inequality. Several authors have in recent years deduced new inequalities from Einstein locality (d’Espagnat 1975, Roy-Singh 1978, *On the Consequences of Einstein Locality*). Most of them turn out to be violated by quantum mechanics. Attempts at understanding such violations in physical terms have consistently failed.

(Particularly promising seems to the line of research of Vigier, which has, however, not yet provided a complete physical basis for the quantum mechanical spin predictions: Vigier 1978, Cufaro-Vigier 1978).

Even well-known nonlocal theories, such as Newtonian dynamics or the de Broglie-Bohm hidden-variable models, are unable to violate the inequalities deduced from Einstein locality (*Nonlocal Theories Satisfying Bell’s Inequality*).

[...] In all cases it is interesting to deduce systematically from Einstein locality all the possible inequalities which it can generate, particularly because they could be tested in the new experiments of quantum correlations (*On the Equivalence of Deterministic and Probabilistic Local Theories*).

[...] In order to do so it is useful to refer to the probabilistic formulation of Einstein locality, since it is more general than the deterministic formulation (Garuccio, Selleri 1980, pagg. 209-210. My underline).

⁶⁰⁶ Garuccio A., Selleri F. (1980), “Systematic Derivation of All the Inequalities of Einstein Locality (A Reply to a Comment by A. Garg)”, *Foundations of Physics*, 10 (3-4), pp. 209-216. (Garuccio, Selleri 1980) in the following.

So, underscoring the limitation of the deterministic local realism, Selleri reached an approach centered on the more general meaning of a probabilistic one, in order to deduce all the possible inequalities, as well as to test them. Selleri wrote with Gino Tarozzi also: “Nonlocal Theories Satisfying Bell’s Inequality” and “Extension of the Domain of Validity of Bell’s Inequality”. And a review paper, “Quantum Mechanics Reality and Separability”⁶⁰⁷. They introduced almost all features of Selleri’s research in FQM: EPR, HV, Einstein locality, Bell’s inequality as well as the current situation at that time concerning the Bell-type experiments. Basically, the abstract was an index:

Introduction / de Broglie’s paradox/quantum theory of distant particles / the EPR paradox / Einstein locality and Bell’s inequality / recent research on Bell’s inequality / general consequences of Einstein locality / nonlocality and relativity / time-symmetric theories / the Bohm-Aharonov hypothesis / experiments on Einstein locality / reduction of wave packet / measurements, reality and consciousness / conclusions (Selleri, Tarozzi 1981, pag. 1).

Among the various description in this paper, I choose the possible solutions to the EPR paradox and his consequent incompatibility between QM and LR:

[...] The most important developments have started from the EPR paradox and have led to the conclusion that there is a deep-rooted incompatibility between quantum mechanics and the principle of local causality and, furthermore, that this incompatibility can be resolved experimentally in favour or against one of the two opposed points of view.

These developments have probably gone too far to be forgotten in the future. If this “unorthodox” research keeps going on, there seem to be only a few ways out of the crisis, barring spiritualistic and mystic solutions:

- *QM has to be modified*. If this is the solution, it will not require minor modifications of the theory. It is probably the superposition principle or the very description of physical states with state vectors that require modification. Present experimental evidence seems to be against this possibility.
- *SR [Special Relativity] has to be modified*. Acceptance of nonlocal interactions over macroscopic distances requires the possibility to send faster-than-light influence, an acceptance of effects that relativity considered impossible. It will be shown [...] that the

⁶⁰⁷ Selleri F., Tarozzi G. (1981), “Quantum Mechanics Reality and Separability”, *Rivista del Nuovo Cimento*, 4 (2), pp. 1-53. (Selleri, Tarozzi 1981) in the following.

basic notion of relativistic causality (propagation of all signals within light-cones) leads to contradictions with some consequences of quantum theory.

- Microscopic objects do not exist and/or space-time is an illusion of our sense. No problem seems to exist, in fact, if one maintains that electrons, photons, atoms and the like are not endowed of objective existence in space and time, but are merely human concepts created to put order in an undifferentiated “physical reality”.

Other proposed solutions are in our opinion variants of the previous ones: models with nonlocal interactions or with propagation of signal toward the past have been proposed and will be discussed, together with the idea of an absolute determinism regulating even the choices of human beings and of generators of random numbers (Selleri, Tarozzi 1981, pag. 3. *Italics* in the original text).

The paper, “Quantum Correlations and Separability”⁶⁰⁸ revealed how Selleri often came back again and again on the same issues, still underscoring his conviction and defense of LR, Einstein locality or whatever could be defined:

The Einstein-Podolsky-Rosen paradox has been formulated at different and progressively deeper levels since its original proposal:

1. as a contradiction between validity of quantum mechanics, completeness of quantum mechanics and existence of separable elements of reality using space-momentum correlations (EPR 1935);
2. as a contradiction between the same three ideas but with an extension to spin correlations (Bohm 1951);
3. as a contradiction between validity of quantum mechanics and existence of separable elements of reality (that is, without the assumption of completeness) both for space-momentum and for spin correlations (d’Espagnat 1965, Augelli, Garuccio, Selleri 1976).

In the present note we will extend further the validity of the EPR paradox by showing that, in the absence of superselection rules, every state vector of the second-type (nonfactorizable state) leads to a contradiction between quantum mechanics and the existence of separable elements of reality. In this way we obtain then an extension of 3. essentially to correlations of arbitrary observables. The proof will be given for discrete eigenvalues but can be extended without major difficulties to the continuous case (Selleri 1980, pag. 1).

⁶⁰⁸ Selleri F. (1980), “Quantum Correlations and Separability”, *Lettere al Nuovo Cimento*, 27 (1), pp. 1-3. (Selleri 1980) in the following.

[...] We conclude therefore that all the nonfactorizable state vectors of quantum mechanics lead to the EPR paradox. The paradox is furthermore at the experimental level [...].

The appearance of the paradox is a manifestation of a deep-rooted incompatibility between quantum mechanics and Einstein locality (elements of reality plus separability). The incompatibility is tied to the mathematical structure of the theory since it originates from the state vector. Other manifestations of the same incompatibility are the violation of Bell's inequality (Selleri 1980, pag. 3).

In another paper written with Tarozzi, "Is Clauser and Horne's Factorability a Necessary Requirement for a Probabilistic Local Theory?"⁶⁰⁹, Selleri dealt with the deterministic and probabilistic local HV model, rejecting the Clauser and Horne factorizability hypothesis, and relating the validity of their probabilistic local HV model to Bell's inequality:

We show with a specific counterexample that a factorizability hypothesis, put forward by Clauser and Horne as the formal basis for the probabilistic local theories, is not generally valid (Selleri, Tarozzi 1980, pag. 533).

[...] Starting from a deterministic local hidden-variable model and averaging over one of the hidden variables, we have so obtained a local probabilistic hidden-variable model for which the Clauser-Horne factorability hypothesis is not true.

This shows that the same hypothesis cannot be considered as general enough also for probabilistic local models for which a deterministic foundation may be absent or unknown. Therefore all the considerations developed from the factorability hypothesis are generally valid only for deterministic local theories.

[...] A comparison between our local hidden variable model, in which an arbitrarily large number of observables can be defined, and Bell's inequality is perfectly possible.

The existence of such a model shows that still today the only sure field of validity of Bell's inequality is the one of the *deterministic* hidden variable theories. This is very unpleasant because determinism is a very narrow philosophical frame, hard to accept as true even by people with realistic and rationalistic inclinations (Selleri, Tarozzi 1980, pag. 535. Italics in the original text. My underline).

⁶⁰⁹ Selleri F., Tarozzi G. (1980), "Is Clauser and Horne's Factorability a Necessary Requirement for a Probabilistic Local Theory?", *Lettere al Nuovo Cimento*, 29 (16), pp. 533-536. (Selleri, Tarozzi 1980) in the following. I found in FSPA also a hand written preliminary version.

There seem to be two possible ways-out of the present situation. If one found that local hidden variable models of probabilistic nature (therefore not of deterministic nature) exist which do not satisfy Bell's inequality, the latter would lose the general empirical and epistemological significance which is commonly attributed to it and the door for a reconciliation between quantum mechanics and local causality would be open. In this perspective only the narrow hypothesis of determinism would bring to a direct conflict with quantum theory. If, on the contrary - and this appears to us as a far more likely possibility - one found that local probabilistic hidden-variable models satisfy Bell's inequality, even when they are not of the factorable type discussed by Clauser and Horne, one could be presumably find a firm theoretical and philosophical basis to the contradiction between local causality and quantum mechanics (Selleri, Tarozzi 1980, pagg. 535-536).

Selleri seems more inclined to find a contradiction between QM and LR, more than to accept theories aimed at reconciling them. The central role of LR in Selleri's approach arose in a very later paper, from the early 90's, namely, "The weakest LR is still incompatible with quantum theory"⁶¹⁰, in which he emphasizes how every kind of realism, even the most general one (that is, the probabilistic LR, developed to answer to some criticisms to EPR reality criterion), shows an innate incompatibility with QM. Displaying once again how, while not being a full theory, it can be considered an alternative approach to study with suitable experiments.

In the abstract he stated:

The existence of predictable probabilities in the microworld requires a reality tied to such a probability. This is a very general form of realism, probably the weakest form that can be conceived. If we add the idea that single-particle probabilities are local (which is also true in quantum theory), then we can deduce Bell's type inequalities.

In spite of its generality, our formulation of local realism is thus still incompatible with quantum theory (Selleri 1991, pag. 17. My underline)

⁶¹⁰ Selleri F. (1991), "The Weakest Local Realism is Still Incompatible with Quantum Theory", *Philosophia Naturalis*, 28, pp. 17-34. (Selleri 1991) in the following.

He introduced some experiments with three polarizers as a way to discriminate between QM and LR. In a paper written with A. Garuccio, “Enhanced photon detection in EPR type experiments”⁶¹¹, they stated that:

Local realistic models of enhanced photon detection reproducing quantum mechanical predictions for single photons and approximating closely the quantum mechanical predictions for pairs of correlated photons are discussed. It is shown that Einstein-Podolsky-Rosen-type experiments with *three* polarizers can easily discriminate such models from ordinary quantum theory (Garuccio, Selleri 1984, pag. 99. *Italics in the original text*)

These experiments have been another proof of the failure to solve the problem, but also another example of all the paths that Selleri has decided to explore in order to find an answer to the shortcomings of the QM.

The importance of Selleri’s work can be found in some quotations made by various scholars.

Firstly, according to Freire, I would take into account that Selleri became, with respect to the applications of Bell’s inequalities, “the most vocal critic of loopholes in these experiments”.

Then, Max Jammer (1915-2010), the great physicist and science historian cited Selleri on his remarkable (Jammer 1974) as regards to Bell’s inequalities. Jammer put the (Selleri 1972) and (Capasso, Fortunato, Selleri 1973) papers at the center of his speech, using the Selleri’s proof of the stronger form of Bell’s inequality.

Let a, b, c be three unit vectors; $P_{q.m.}(a,b)$ and $P_{h.v.}(a,b)$ the expectation values for the product of the spin observable, for QM and HV respectively. Hence, you get:

$|P(a,b) - P(a,c)| \leq 1 + P(b,c)$, the so-called “Bell’s inequality”. According to Jammer, the generalized inequality is: $|P(a,b) - P(a,c)| + P(b',b) + P(b',c) \leq 2$.

⁶¹¹ Garuccio A., Selleri F. (1984), “Enhanced Photon Detection in EPR Type Experiments”, *Physics Letters*, 103A (3), pp. 99-103. (Garuccio, Selleri 1984) in the following.

Then Jammer talked about Selleri and his preprint “A stronger form of Bell’s inequality (Selleri 1972)⁶¹²:

Recently Franco Selleri of the University of Bari, Italy, gave a simple proof of a stronger form of Bell’s inequality, that for any four unit vectors a, b, c, d the following relations holds: $|P(a, b) - P(a, c)| + |P(d, b) + P(d, c)| \leq 2$.

Jammer continued quoting Selleri also for his joint paper (Capasso, Fortunato, Selleri 1973):

He also studied, in collaboration with Vincenzo Capasso and Donato Fortunato, the following problem: for what state functions (of which the singlet states considered by Bell are particular cases) can the inequalities $[|P(a, b) - P(a, c)| \leq 1 + P(b, c)]$ and $[|P(a, b) - P(a, c)| + |P(d, b) + P(d, c)| \leq 2]$ be violated for suitable choice of vectors? [...] was the derivation of Bell’s inequality, or of its stronger form, which led to the conflict with quantum mechanics, really based assumptions general enough to warrant such far-reaching conclusions? (Jammer 1974, pagg. 311-312).

And with a letter, sent to Selleri in 1987 and regarding the paper “History of the EPR Paradox”:

[...] your History of the EPR Paradox, which I greatly enjoyed reading, deserves in my opinion to be published as a separately available brochure (Max Jammer to Selleri, 01 November 1987-FSPA)

Also David Kaiser⁶¹³ underscored the predominant role played by Selleri in FQM:

Selleri and his group in Bari, Italy, had been among the earliest and most active researchers on Bell’s theorem anywhere in the world (Kaiser 2011, pag. 208 on Kindle Book).

According to Laurora, Selleri was:

⁶¹² In (Jammer 1974), footnote n. 129 at page 311, Jammer cited (Selleri 1972) paper and still summarized his derivation

⁶¹³ Kaiser D. (2011), *How the hippies saved Physics. Science, Counterculture and the quantum revival*, New York: W.W. Norton & Company.

[...] another protagonist in the history of research into the EPR paradox, who since the late '60s had developed a critical attitude towards the interpretation of quantum theory and who has made important contributions both to the clarification of the ideas underlying the foundations of quantum mechanics and to the development of the experimental investigation on the verification of the Bell Theorem⁶¹⁴ (Laurora 2014, pag. 61).

A probative and profound contribution comes from Gino Tarozzi⁶¹⁵, given his long-standing collaboration with Selleri. In the conference for the 75 years of Selleri, whose proceedings were collected in (Tassani 2013), he summarized the activity of Selleri this way⁶¹⁶:

Franco Selleri's (henceforth Franco) criticism of the orthodox interpretation of quantum mechanics has constantly focused on its anti-realistic nature, taking shape as the philosophical presupposition of a research programme on the foundations of physics that has highlighted the existence of further paradoxes and contradictions of the standard formulation compared to those already known, and tried to identify new ways towards alternative interpretations.

Franco first highlighted with extreme clarity through rigorous arguments, both on the formal and physical level, how the orthodox interpretation of the Copenhagen school (or rather of Copenhagen and Göttingen) [...] was in open conflict with a philosophical perspective of a realistic kind from two different points of view: first of all with regard to the fundamental theoretical concept of wave function, deprived of physical meaning in Born's interpretation, and secondly with regard to the more general problem of so-called local realism.

According to Franco, the latter represented the simultaneous assumption of the validity of the criterion of physical reality of EPR and of the Einsteinian locality principle, which appeared incompatible both from a logical and empirical point of view with the quantum description through non-factorable state vectors, which in those years were defined as states of EPR or second species, to distinguish them from the ordinary factorable states of the first species, while now in literature the denomination of entangled, or entanglement states has prevailed (Tarozzi 2013⁶¹⁷, pagg. 223-224).

⁶¹⁴ the Italian original text is in Addendum 3, p. 1008.

⁶¹⁵ Gino Tarozzi (1954-): full professor of Logic and Philosophy of Science at Urbino University. He collaborated a lot with Selleri, still publishing remarkable papers of FQM. See: <https://www.uniurb.it/trasparenza/amministrazione-trasparente/organizzazione/gino-tarozzi>, <https://www.uniurb.it/persona/gino-tarozzi>. Both accessed 24 September 2020.

⁶¹⁶ Being a long excerpt, the Italian original text is in Addendum 3, p. 1008.

⁶¹⁷ Tarozzi G. (2013), "Oltre la fisica normale. Realtà della funzione d'onda e delle proprietà fisiche prevedibili nell'interpretazione di Selleri della meccanica quantistica", in Tassani I. (ed), *Oltre la fisica normale: interpretazioni alternative e teorie non standard nella fisica moderna per il 75-esimo compleanno di Franco Selleri*, Isonomia Epistemologica, 3, pp. 223-250.

[...] To the research on the problem of both logical and empirical incompatibility between quantum mechanics and local realism, and to the double problem of the hypotheses at stake in the demonstration of Bell's theorem and its validity, Franco has made a contribution that can hardly be overestimated in 20th century physics. In the most authoritative reconstruction of the debate on the foundations and interpretations of quantum theory from its origins until the early 1970s, [i.e., Jammer 1974], the author highlighted the importance of this contribution, showing how, in addition to having demonstrated the strongest form [and more structured, paradoxically said weak] of Bell's inequality, Franco had identified the logical roots of the problem in the conflict between entangled and non-entangled states, as well as the existence of other observable states other than Bell's able to discriminate with respect to the descriptions that used one or the other of these states. [Jammer] dwelt again on the role of contributions to the study of the problem made by Franco and his collaborators in his opening report to the symposium in memory of John Bell in 1991 (Tarozzi 2013, pag. 226. My underline).

Starting from another citation by Freire (a further confirmation of the awareness in the scientists community of the role played by Selleri), I am introducing another paper, specifically, “Local realism has not been refuted by atomic cascade experiments”⁶¹⁸, in which the three authors, Marshall, Santos, and Selleri managed to reduce their model to a single angular Hidden Variable.

The quotation expressed by Freire stated this way:

[...] Other physicists did trust that detectors with higher efficiencies would lead to violations of quantum mechanics.

Indeed, one year after the publication of Aspect's experiments, Marshall et al. [Santos and Selleri] (1983), published a paper entitled “Local realism has not been refuted by atomic cascade experiments,” in which they built a hidden-variable model able to mimic quantum-mechanical predictions. Marshall, Santos, and Selleri had resumed a line of approach first suggested by Clauser and Horne (1974), by relaxing the fair sampling assumption adopted in the CHSH (1969) paper (Freire 2014, pag. 279).

Concerning the foregoing paper, Marshall, Santos, and Selleri stated in the abstract their choice to involve only one HV:

⁶¹⁸ Marshall T.W., Santos E., Selleri F. (1983), “Local Realism Has Not Been Refuted by Atomic Cascade Experiments”, *Physics Letters*, 98A (1-2), pp. 5-9. (Marshall, Santos, Selleri 1983) in the following.

We obtain a local realistic model, involving just one angular hidden variable, giving predictions, for the coincidence counts in atomic cascade experiments, which come extremely close to those of the quantum-mechanical model, and which fit the experimental data as closely as do the predictions of that model (Marshall, Santos, Selleri 1983, pag. 5).

They found how their model fit with the first Aspect experiment “to within two standard deviations” (Marshall, Santos, Selleri 1983, pag. 117), and that:

A further feature to be stressed is that *in most cases only the angles $22\frac{1}{2}$ and $67\frac{1}{2}$ have been considered for checking Bell's inequality: our predictions are identical to the quantum-mechanical ones for these values* owing to the vanishing of $\cos 4(b - a)$ at these two angles. The maximum deviation between our model and quantum mechanics corresponds to the extreme values of $\cos 4(a - b)$, namely at 0° , 45° and 90° . With the exception of the second experiment by Aspect et al.⁶¹⁹ [...] our predictions lie almost always within the error bars of the experimental data (Marshall, Santos, Selleri 1983, pag. 7. *Italics in the original text. My underline*).

Marshall, Santos, and Selleri pointed out how Aspect et. al. did give no numerical data to permit a control: “certain criticisms may be made of [Aspect] data analysis”. “In another paper⁶²⁰ we show that rescattering plays an important role in most atomic-cascade experiments performed up to the present time” (Marshall, Santos, Selleri 1983, pag. 7).

Finally, the authors submitted something about the physical significance of their model, still enhancing Selleri's view of the validity of weak inequalities and LR against the additional assumptions:

[...] it is possible to view it simply as a counterexample which shows that, because of the low detection rate, atomic cascade experiments have not refuted local realism.
[...] It is clear to us that the ban on “enhancement” proposed by Clauser and Horne is an entirely artificial restriction on hidden variable theories. It is small wonder that, by imposing such a ban, one comes to the conclusion that hidden variable theories cannot explain the experimental data

⁶¹⁹ Selleri et al. referred to Aspect A., Grangier P., Roger G. (1982), “ Experimental Realization of Einstein-Podolsky-Rosen-Bohm Gedankenexperiment: A New Violation of Bell's Inequalities”, *Physical Review Letters* 49, pp. 91-94.

⁶²⁰ They referred to Marshall T.W., Santos E., Selleri F. (1983), “Collective Effects in the Atomic Cascade Experimental Tests of Bell Inequalities”, *Lettere al Nuovo Cimento*, 38, pp. 417-420.

[...] Only for very high-efficiency counters would it be possible to ignore the dependence of the detection rate on [the hidden variable]. For such counters Bell's inequality would, naturally, recover its full significance (Marshall, Santos, Selleri 1983, pag. 8. My underline).

[...] while the evidence from atomic cascade experiments is, so far, inconclusive in deciding the issue for which they were designed, such experiments can, with suitable improvements, continue to provide valuable material.

In view of our results we think that tests should also be made in completely different areas, and one of us has already indicates such a test⁶²¹ (Marshall, Santos, Selleri 1983, pag. 9. My underline).

Here the authors emphasized how atomic cascade experiments are not conclusive and they quoted the paper written by Selleri on kaons in 1983, maybe the first publishing on this issue, on which Selleri will work during the 1990s and the 2000s and that I will introduce in section 2.9:

The very general conclusion they took from their results (and with which I am going to finish this paragraph, too) stated that, even recognizing the importance of the experiments done, it will be necessary to challenge the quantum assumptions:

Experiments to test quantum theory against realistic theories have been of immense help in sharpening our understanding of the difference between these two classes of theories.

But, because there is not, as yet, any single realistic theory whose formulation has the preciseness of quantum theory, we must expect to find ourselves making assumptions, often unwittingly, about such theories, based on three generations of immersion in and acculturation by quantum concepts. Any serious examination of the realist alternative must involve a challenging of such assumptions (Marshall, Santos, Selleri 1983, pag. 9. My underline).

⁶²¹ Selleri F. (1983), "Einstein Locality and the $K^0\bar{K}^0$ System", *Lettere al Nuovo Cimento*, 36 (16), pp. 521-526.

2.7.10-SELLERI'S FURTHER GENERAL THOUGHT ON THE PREVIOUS TOPICS

I reported here several documents (letters, interviews, unpublished and unprecedented documents) useful for a better comprehension of Selleri's approach to FQM.

I showed how LR has always been the center of Selleri's thought and he researched a lot in order to preserve it, counteracting the idea that Aspect's experiments had put an end to the difference between QM and LR in favour of the former.

In his interview with Freire, Selleri summarized his activity on FQM:

You see my research in the foundations of quantum mechanics has the following strong points, the way I see it. First, the proposal of experiments to detect the objective reality of the probability waves. Second, showing that the inequalities violated by experiments are very different from the original Bell's inequalities in many ways, so that they do not constitute a violation of local realism. And third, proposing a set of new experiments with K mesons which could really allow one to discriminate between the Copenhagen approach and local realism. That is what I know. But this is not a new theory. It is only a set of ideas (AIP 2003).

In addition, I found in Selleri's archive two handwritten notebooks in which he remarked, with short sentences and notes, some basic ideas on the issues addressed so far in the section, often written during his travel for meeting or conferences or missions. I think this is a really compelling and vivid documentary evidence, with some strong conclusion on the need to preserve LR. So I decided to report here several excerpts in the form he wrote them, without any grammar correction or completion, just highlighting the sketchy structure typical of undeveloped notes.

The first notebook was written in the mid 1980s; whereas the second covers a period full of journeys, between April 1989 and September 1990. I submitted here the former, whereas the latter will be introduced in section 2.8.5.5.

Lastly, I will write here directly the English translation. The original notes are all in the Addendum 3, pp. 1009-1010.

NOTEBOOKS AND NOTES FOLDER

Several von Neumann theorems

- von Neumann
- Jauch-Pironi
- Gleason
- Kochen-Specker
- Salomon
- Destouches-Fevrier
- Gudder

It is not possible that they did not understand the limits of their theorems.

Bohm and Bell models do the impossible. Paradigma is now unprotected. This role for Bell's inequality is wanted by the conservatives.

But innovators see different things, they discover beautiful things:

- 1) Probabilistic formulation
- 2) same inequalities as deterministic
- 3) propensities
- 4) infinite inequalities

Local Realism is trivially true in all the examined situations.

The Bell's inequality was discovered long after the birth of MQ (1927-1965). It is strange for a fundamental phenomenon such as non-locality.

What do you mean [the hidden variables] are hidden? They can't be more obvious than that! Hidden variables can be local, nonlocal, mixed. It doesn't matter.

The following sounds as very impressive notes, also in view of Selleri's research:

If it were true that Bell's inequality is violated, it would be the end of local realism.

All realistic and local models satisfy the LR.

If the violation is systematic, an LR violated lies to all local models violated. Better to think before accepting violations.

An instance open to different contributions is or should be in everyone's interest.

The grave closing of the EPR+Bell's inequality paradox would stifle all tendencies towards renewal

Then he continued describing the environment linked to Bell's inequality:

Cultural climate

- Bell was uncomfortable
- Experiments were done by young strangers. Then, having found the desired result, an international *passaparola* made them well known
- Results often taken for granted a priori by the authors
- Stress and clashes in conferences
- The defenders of the locality had a hard life. They were few because it took courage to assume the defense of the locality. Even that doesn't make scientific sense
- Young careers crushed (es. Lepore, De Caro)
- Proposed experiments, never done

There were experiments that violated the quantum mechanics

- Holt-Pipkin (never published)
- Clauser LNC
- Catania LNC

Forced not to publish their results not because they had found errors in their experiment, but purely with threats of repression.

The dialectic of different experiments that deepen an aspect of quantum physics has never started. Theme nailed to a minimal form.

"it is not a coincidence that all those who look sympathetically at the magical world of 'paranormal' phenomena accept without difficulty the violation of the Bell's inequality" (Selleri Fondamenti 1992, pagg. 33-34)

when quantum mechanics was born there was no discussion/debate on what has become the deepest thing in quantum mechanics. In fact the non-locality is not even mentioned by the great physicists who then divided into two opposing fronts.

In his reconstruction of Selleri's work, Pompilio Nutricati, recalling some affirmations expressed by Selleri, too, highlighted that:

All the physicists who have dealt with Bell's inequality (pragmatists, positivists, idealists, spiritualists, realists) agree that Bell's Theorem does not concern a specific model of local reality but that it is a theorem of the highest generality, [...] decisive for the development of physics.

[...] a possible confirmation of the theory of quantum mechanics would lead to the nullification of the validity of local realism and therefore, to an irreversible defeat of realism.

Rationalism, in fact, is a philosophy that is at the same time extremely strong and extremely weak. It is extremely strong in its ability to penetrate reality, to guide towards the understanding of natural phenomena. It is extremely weak in the sense that, by breaking down just one of its principles, everything collapses. It is in fact a philosophy that tends towards totalization and therefore also towards the exclusion of any kind of “miracle”. This is why [Selleri] is convinced that the battle on the validity of quantum mechanics or on the validity of local realism, and therefore of Bell’s inequality, is the decisive battle in one sense or another.

[...] If rationalism loses a single battle, it also loses the war⁶²² (Nutricati 1998, pagg. 104-105. My underline).

2.8-SELLERI UNFLAGGING ACTIVITIES DURING THE 1980s: THE STRUGGLE TO DETECT EMPTY WAVES. WITHOUT OVERLOOKING EPR AND BELL’S INEQUALITIES ONCE AGAIN, THE FULL PROFESSORSHIP, THE BARI’S GROUP, THE GREAT NUMBER OF ORGANIZED AND ATTENDED CONFERENCES, THE MANY WELL REVIEWED POPULARIZATION AND PUBLISHING ACTIVITIES

2.8.1-FOREWORD

In this section, after a foreword on the historical context, I will show how Selleri abandoned political activity to immerse himself in a very intense research, also linked to the winning of the competition and the coveted position of full professor. Then, I will introduce the activities of the Bari group on FQM and how it was perceived within the Department. In those years, in different parts of the world, the FQM were not well accepted within the Institutes and Departments of Physics, as can be seen from some letters related to the perception of the FQM group of Bari and other letters sent to various departments in the USA. I will go on to analyze the researches on empty waves and how they have been received by the community of scientists.

⁶²² The original Italian text is in Addendum 3, p. 1011.

This remarkable issue in Selleri's research played a key role for his passage to FRT, since Selleri searches for a medium in which the EW had to propagate, finding an opportunity in the Lorentzian ether.

Within his activity, Selleri wrote on EPR and Bell's inequality also during 1980s, because he never separated completely his field of research: science issues are related each other! Selleri has brought forward all the topics in a contemporary way, as it is rarely used to see in physics today, with the huge specializations present⁶²³. Many papers dealt with the probabilistic local realism as well as the violation of the only strong Bell's inequalities, still preserving the weak ones, saving Local Realism and rejecting the additional assumptions. In 1991, he made another deep review of the situation concerning Bell's theorem and EPR paradox⁶²⁴. Again in 2002 he wrote on weak and strong Bell type inequalities related to the experiments on neutral kaons⁶²⁵.

I will show how Selleri also organized and attended numerous international and national conferences, in order to share the scholars diverse views, to spread his ideas and the approaches in which he believed, as well as to weave numerous relationships with different scholars, as Popper, for instance. And he published several books which received a warm welcome and good reviews.

Nevertheless, his vibrant activity never merged into a real school, as some interviews confirmed.

2.8.2- A GLANCE AT THE EIGHTIES AND AT SELLERI'S SOCIAL COMMITMENT

Historically speaking, the 1980s established another intense decade. Among the numerous events, there was a counter-revolutionary reflux which followed the intense socio-political activity in the 1970s, and the outbreak of

⁶²³ See the chronological development of Selleri's papers, in which you can find papers on EW and EPR alternatively during the same years, as well as in FQM and FRT, too.

⁶²⁴ Home D., Selleri F. (1991), "Bell's Theorem and the EPR Paradox", *Rivista del Nuovo Cimento*, 14 (9), pp. 1-95.

⁶²⁵ Selleri F. (2002), "Weak and Strong Bell Type Inequalities", in *Física Quántica y Realidad*, Mataix C., Rivadulla A. (eds), Madrid: Ed. Complutense, pp. 231-247.

several low intensity conflicts, other than real wars, more marked by invasions and guerrilla actions, in areas under the sphere of influence of the two superpowers, USA and USSR. There was also the beginning of an unavoidable path that led to the fall of the Berlin Wall and the end of the Soviet model that began in 1917. Moreover, Italy experienced a return to private life and the desire for a few leisures, in reaction to the very heavy years experienced in the previous decade, and the advent of heroin that destroyed many lives and movements⁶²⁶.

The so-called “Riflusso nel privato” (the return to the private sphere), a term used in the journalistic field, characterized an attitude linked to political and social disengagement and withdrawal into the private sphere in a climate of disillusionment and return to values of the past. In Italy the period is dated between 1978 and 1982.

The first signs of a return to private life were seen between the end of 1978 and the beginning of 1979, with the revival of consumption during the Christmas holidays, with the rejection of ideologies and parties.

[...] The turning point of the reflux and the return to private life was often identified in the autumn of 1980 with the march of the forty thousand in Turin, when the existence of a “silent majority” came back into the limelight in contrast to the clamour of the social clashes of the previous decade.

On 14 October, numerous middle managers of FIAT, tired of the continuous protests of the trade unions opposing the redundancy fund and preventing them from entering the factory to work, gave birth to a “silent” procession through the city. Their demonstration silenced the strikes and occupations.

[...] The period was characterized by a widespread return of people from the squares to the private sphere, from the beginning of the era of commercial television combined with a take-off of advertising and an increase in consumption.

⁶²⁶ For this introduction, I referred to the Wikipedia entry on the comeback to the private, https://it.wikipedia.org/wiki/Riflusso_nel_privato#:~:text=In%20Italia%20il%20periodo%20del,sociale%20e%20politica%2C%20e%20al; and to a very interesting essay, *L'Italia degli anni 80*, by Carlo de Maria e Tito Menzani, published on https://ladigacivile.eu/1_italia_degli_anni_80. Both accessed 24 September 2020. On this last site it reads as “the *Ladigacivile* didactic portal is conceived, built, managed and developed by the Istituto per la storia della resistenza e dell’età contemporanea della provincia di Forlì-Cesena (Institute for the History of Resistance and Contemporary Age of the Province of Forlì-Cesena) and by the Sezione forlivese del Centro di documentazione Archivio Flamigni (Forlì Section of the Flamigni Archive Documentation Centre) (belonging to the Rete degli archivi per non dimenticare (Network of Archives So As Not To Forget)), thanks to the contribution of the Istituto per i Beni Artistici, Culturali e Naturali della Regione Emilia-Romagna (Institute for Artistic, Cultural and Natural Heritage of the Emilia-Romagna Region)”. There are also interesting didactic paths and readings and a rich bibliography on the subject.

[...] On a political level, personalization began to prevail over ideological belonging; there was a decline in the power of trade unions and the Italian Communist Party [Partito Comunista Italiano, PCI], in parallel with the rise of Bettino Craxi among the ranks of the Italian Socialist Party [Partito Socialista Italiano, PSI], called in 1976 to raise the fate of the party, which was then at an all-time low, tightened in the pincer of the attempt at historical compromise between the DC and the PCI.

[...] Disengagement in political action also affected young people, replaced by the birth of youth subcultures⁶²⁷ (Wikipedia, see footnote 630).

In order to generalize to the Italian situation in the Eighties, always without claiming to give an exhaustive view, I am quoting several excerpts by L'Italia degli anni '80, di Carlo de Maria e Tito Menzani (see footnote n. 613).

They begin analyzing the international framework, characterized by the Cold War and the neoliberal advance. In order not to further burden the thesis with other notes, all the Italian original text of the excerpts of this essay are given in the Addendum 3, pp. 1012-1016:

The 1980s are a relatively recent historiographic category. They represent the decade that incubated the decline of communism in the Western world, and consequently also the end of the Cold War and the rise of neoliberal instances. At this stage the conflict between the American dream and the Soviet myth, which had long been substantiated on two separate levels, was resolved. On the one hand, there was the military confrontation, made of a weapon race and a long-distance duel to support friendly governments in emerging countries. On the other hand, there was a cultural clash, which aimed at offering to the international public opinion its own model of development as the best one ever.

In 1985, Michail Gorbachev was appointed to the leadership of the Soviet Union, the first secretary of the Soviet Union Communist Party not to have been directly involved by Stalinism and decided to introduce various reforms in the country.

The basic intention was a correction of centralized planning through elements of market economy and a loosening of the monocratic overlap between state and communist party structures, in order to legitimize even some moderate expressions of dissent. In a short time, Gorbachev gained a broad international consensus, and was seen with favor and confidence especially by Western public opinion.

In 1985, [Gorbachev and Reagan] met in Geneva, a year later again in Reykjavik, and a year later again in Washington.

⁶²⁷ The original Italian text is in Addendum 3, p. 1011.

Although in practice the agreements between the USA and the USSR were limited to a reduction in armaments and some specific agreement of non-interference at international level - the USSR undertook to stop the military occupation of Afghanistan - the media impact of these meetings was huge and it was agreed that the Cold War had reached a new season.

At the same time, Western Europe continued the process of economic and, subordinately, political integration which it had timidly begun after the Second World War. The Eighties marked an acceleration of this process, thanks to the long and laborious (but above all fruitful) diplomatic work that led, in 1985, to the Schengen agreements (effective only since 1990). These established the free movement of people between the European signatory countries, and were in a sense the prelude to the Maastricht Treaty, signed on 7 February 1992 by the twelve member countries of the European Economic Community (EEC) and entered into force on 1 November of the following year, which established the birth of the European Union, replacing the previous EEC.

These scenarios of progressive detente were accompanied by a liberalist turn in the political life of the main countries with mature economies.

The seventies had been almost everywhere the climax of a season of struggles and political-union claims started in the second post-war period and implemented by the movements born in the '68. In the following decade these instances returned, also on the wave of a generational turnover that gave space to young people who had grown up in the civilization of well-being and consumption. The mindset of these new generations and the interests of important economic lobbies acted as a substratum for the rise of neoliberalism, a political-economic conception that aimed at deregulation and at reducing the weight of the State in the public economy. British Prime Minister Margaret Thatcher was among the world's leading leaders to ride out this doctrine, and she identified the public enterprise as the main focus of controversy.

Economists such as Friedrich von Hayek and Milton Friedman - the main theorists of neoliberalism - had a good game in denouncing the inefficiency of public enterprises, but the radical solution they proposed, i.e. privatization, ended up in several cases with serious social costs, because it was often applied indiscriminately, even to companies operating in natural monopolies or managing essential services.

The other face of neoliberalism was the attack on trade union forces, accused of having a conservative or too ideological attitude, in any case hostile to the ownership of companies, and therefore guilty of holding back development (L'Italia degli anni '80).

Subsequently, the authors focus on the distinctive Italian situation, specifically on the turning point symbolized by the "March of the Forty Thousand":

In Italy it was very clear that the transition from the Seventies to the Eighties had involved a break in the history of the union. After the upheavals of 1968 there had been a general improvement in wage conditions and the progressive reforms launched by Parliament had contributed to a general advance of the left throughout the country. The internal debate within the PCI and other left-wing movements and parties ended up [instead] becoming largely self-referential, and neglecting that part of the citizenship that was recognised in a political proposal of the opposite sign. It was a sort of ideological hangover, with an abrupt awakening on 14 October 1980. On that day, in the heart of the working class Turin, the so-called “March of the Forty Thousand” was held, a demonstration by Fiat executives and employees against the union, which had been in agitation for over a month and prevented many of them from entering the workplace through pickets at the gates. [The march was] a harsh stand of the middle class of the factory against the workers’ rhetoric and the vindictive practices adopted by the union. Beyond the immediate effects - i.e. the rapid closure of the dispute that had triggered the strikes - the march had a huge emotional impact (L’Italia degli anni ’80).

Another serious defeat for the union occurred during the years of the Craxi government, when he decided to resize the so-called “scala mobile”, which adjusted wages to inflation:

Among the protagonists [...] of Italian politics is certainly Bettino Craxi, who became secretary of the Italian Socialist Party (PSI) in 1976, after the party’s electoral consensus had fallen below the 10% threshold. Strongly critical of communism, Craxi presented himself as a decision maker attentive to society and civil rights, able to dialogue with the movements and broad swathes of the middle class.

[...] In charge of forming the new government, Craxi was the first socialist to become president of the council. [...] He confirmed the alliance of the so-called “pentaparty”, between Christian Democrats (DC), Socialists (PSI), Social Democrats (PSDI), Republicans (PRI) and Liberals (PLI), and launched an executive destined to remain in office for almost three years, a record in the history of the Italian Republic.

It was followed by a second, shorter Craxi government, which meant that he remained *de facto* chairman of the council from 1983 to 1987, thus marking the decade of the 1980s.

[...] In domestic politics, in 1984, Craxi passed a decree that reduced the “scala mobile”, a mechanism that adjusted wages to inflation. Although this instrument was important to protect purchasing power, it contributed to aggravate the problem of the cost of living. The PCI and the CGIL made a harsh opposition to the reform and also collected signatures for a referendum to repeal the measure, which - surprisingly - saw the yes vote at 45.7%, against 54.3% in favour of the Craxian reform. This was a second serious defeat for the main Italian trade union, after the march of the forty thousand.

[...] Moreover, Craxi reformed the Italian television system, which at the time was based on the dichotomy between Rai's national public service and small private broadcasters, allowing the creation of private national television channels. Basically, the position of the sector's main entrepreneur, Silvio Berlusconi, owner of Rete 4, Canale 5 and Italia 1, who already broadcast throughout the national territory via interconnected stations, was healed. The measure was much discussed, because not only were Craxi and Berlusconi very close friends - even the former witnessed the wedding of the latter and Barbara Berlusconi's baptismal godfather - but because the birth of the first great pole of Italian commercial TV implied a debt of gratitude of Berlusconi to the government. On the other hand, from a cultural point of view, the success of the new commercial television meant, among other things, a much more aggressive and effective model of advertising promotion than the old and discreet Carosello⁶²⁸. Some commercials of the eighties became a cult, entering fully into the "television memory" of Italians.

[...] The new governments [subsequent to Craxi's] did not have the strength to be long-lasting, let alone incisive. More than anything else, there was a clear detachment between civil society and political commitment, which took away any residual space for subversive proposals, but which also ended by accentuating the distrust of the parties. In particular, in the face of growing criticism of the government, the oppositions did not seem able to gain ground, anchored to ideological schemes that at the dawn of the nineties did not seem to be current anymore (L'Italia degli anni '80. *My Italics*).

In 1987, there was a revocatory referendum in Italy on nuclear power, proposed in the aftermath of the Chernobyl nuclear disaster on 26 April 1986. The query asked to repeal some articles allowing the state intervention in the event of municipal refusal to grant a land for the construction of a nuclear power plant. In the wake of Chernobyl, not wanting any municipality to have a nuclear power plant in its territory, and failing in the event of a 'yes' vote in the referendum, any opportunity for the state to impose its construction, the option of using nuclear energy on Italian soil was in fact lost. The 'yeses' won in the referendum with more than 80%!

⁶²⁸ Carosello was an Italian television programme aired on Rai's National Programme (later to become Rete 1) from 3 February 1957 to 1 January 1977. It consisted of a series of films (often comic sketches in the style of light theatre or musical interludes) followed by advertisements.

The realization of Carosello was attended by famous directors such as Luciano Emmer (who is considered the inventor), Age&Scarpelli, Luigi Magni, Gillo Pontecorvo, Corrado Farina, Ermanno Olmi, Sergio Leone, Ugo Gregoretti, Valerio Zurlini, Pupi Avati, Pier Paolo Pasolini, Federico Fellini and Richard Lester. See also <https://www.italiamobilesrl.it/blog/carosello-storia-origini-curiosita-e-fine-del-predecessore-del-viral-marketing/>. Accessed 24 September 2020.

From a scientific point of view, this outcome led Italy to renounce an energy source that could also give it a certain autonomy, disconnecting it from the dependencies of the OPEC⁶²⁹ oil-producing countries. In (Bonolis, Melchionni 2003) and (Bonolis 2008) there are significant testimonies of oral history on this subject by the great Italian physicists who lived in that period. The debate on nuclear energy revived in the period 2008-2011, in which the Italian government attempted to return to nuclear electronic production in the country. In 2011 was submitted another abrogative referendum. Another dramatic event such as the accident at the Fukushima nuclear power plant in Japan, certainly contributed to influence the vote once again. With a majority of over 94% against a quorum of around 54% of those entitled to vote, the rules pertaining to the return to nuclear power were therefore repealed, leading to the closure of the new nuclear programme⁶³⁰.

Selleri was always in favour of nuclear energy for civil use, writing books and papers and also participating at Italian conferences.

For elementary particle physics, the decade of the 1980s was a flourishing period, which saw it at the centre of the intense development of so-called *big science*, characterized by massive investments and the creation of international centres for research in PP with the participation of hundreds of scientists involved in long-term projects.

Going back to the foregoing essay on the Italian situation in the 1980s, Carlo de Maria e Tito Menzani also introduce a paragraph linked to the end of Communism, which goes as far as the 1990s. I am reporting here as it concerns Selleri's political position, which I introduced at the end of Chapter 1 and in Chapter 2.

The policy of detente with the United States inaugurated by Reagan and Gorbacev helped to increase the popularity of both.

⁶²⁹ OPEC is the acronym for the Organization of the Petroleum Exporting Countries. See also https://www.opec.org/opec_web/en/. Accessed 24 September 2020.

⁶³⁰ For a description of the details see, https://it.wikipedia.org/wiki/Energia_nucleare_in_Italia, from which I got the ideas. Accessed 24 September 2020.

However [...] it did not escape the most that the Soviet model had plunged into a serious crisis, which manifested itself through a growing gap between the quality of Soviet life and that of Western countries. The reforms promised by Gorbachev had precisely the objective of revising the economic and institutional mechanisms of real socialism, to revitalise this model in terms of competitiveness and international credibility. The watchwords launched by Gorbachev were three: *perestrojka* (reform), *glasnost* (transparency) and *uskorenie* (acceleration of development), although it was above all the first, sometimes in conjunction with the second, to have greater media success and to be remembered by public opinion, including the international one. [...] By the late 1980s, the scope of these renewal policies was very tangible, but the consequences were far removed from what Gorbachev had imagined. From his point of view, the legalization of dissent should have led to constructive criticism aimed at improving the Soviet socioeconomic system, but instead, discordant voices and opinions were catalyzed, which however had their own polemical objective in Gorbachev. On the one hand, a part of the bureaucracy and the ruling class criticized the openness to the West, on the other hand an increasing number of dissidents not only asked to reform the system, but identified the overthrow of communism as the real political aim.

The increased freedom of information suddenly allowed Soviet citizens to perceive a reality very different from the one they knew through direct experience and regime propaganda. The echoes of scandals relating to the corruption of the political class, the comparisons between the standard of living in the east and the west of the Iron Curtain, the waste caused by the inefficiency of the state machine were gradually bounced from one newspaper to another, taking advantage of the relaxation of censorship, and contributed to a considerable increase in discontent in the country. The most immediate consequences [had however] in the satellite countries of Eastern Europe. In many of these, communism had increasingly shown a tyrannical and unpopular face, so much so that it was difficult to imagine a prolongation of these regimes without the support of Moscow. In Poland, for example, an independent trade union called Solidarnosc, of Catholic inspiration and openly hostile to communism had been operating for some time now. After the rise of Gorbachev, Solidarnosc had more chance of movement, and obtained the first free elections in one of the two chambers of parliament.

And so, in June 1989, the Polish people were called to the polls, choosing Solidarnosc's candidates in mass. It was a clear and definitive proof of the unpopularity of communism. The new Pope, the Polish Karol Wojtyła, who rose to the papal throne in 1978 under the name of John Paul II, played an eminent role in this delicate phase of his country's politics. He made nine apostolic trips to Poland, five of them between 1979 and 1991, and was one of the main political and spiritual reference points of the Solidarnosc movement.

The events in Poland triggered a chain reaction. In 1989, Hungary decided to open the border with neighbouring Austria and allow its citizens to expatriate. The very long barbed-wire barrier running along the border was broken down, interspersed with surveillance turrets. In reality, it was a much more far-reaching measure because it officially sanctioned a breach in the Iron Curtain. Thus, from practically all the countries of the Soviet bloc one could reach Hungary and from there enter Austria and then move freely in Western Europe. The opportunity was seized mainly by the East Germans, as they were given the opportunity to get around the Berlin Wall and the border with the West. Within a few weeks 13,000 German citizens crossed the border, revealing to the world the failure of the communist model. The East German government resigned and on 9 November 1989, on the orders of the new authorities, restrictions on movement between the sectors of Berlin were lifted.

It was a historic date, later remembered as “the Fall of the Wall”, as thousands and thousands of Berliners worked hard to materially break down the symbol of the regime and the separation between East and West. The event was experienced with great participation all over the world, while television showed images of Germans in tears embracing their friends and relatives after decades of forced distance.

It was a great emotional shock to the whole system of “popular democracies”. In Czechoslovakia, a series of street demonstrations saw the return on the scene of the protagonists of the Prague Spring.

Thus began the “velvet revolution”, i.e. the overthrow of the communist regime in a non-violent manner and without the latter trying to suppress the revolt. On the contrary, in Romania, the end of the dictatorship was marked by serious violence, mainly perpetrated by the army under the orders of President Nicolae Ceausescu, who did not want to abandon power after more than twenty years of leadership. On 25 December 1989, Ceausescu was captured, tried and executed with Kalashnikov by a firing squad.

These events ideally opened the 1990s, during which there would be the end of real socialism in Europe, the dissolution of the USSR, the reunification of Germany, and above all the introduction of representative democracy and market economy in all Eastern countries.

These events also had repercussions on the political assets of Western countries, giving new breath to neoliberal demands and accelerating the identity crises of parties that defined themselves as communist, including the PCI. On November 12, 1989, the secretary Achille Occhetto announced the will to proceed with the dissolution of the party and to build a new political structure committed to the struggles for work and placed on the left, but without an umbilical cord with the USSR, which was in disintegration, and not flattened by Marxist ideology. In what went down in history as the “turning point of the Bolognina”, because the speech was delivered in the homonymous working-class district of the Emilian capital, the premises were laid for a reflection - at times very tormented - that in 1991, in Rimini, would lead the 20th Congress of the PCI to

sanction the end of the communist experience and the birth of the Democratic Party of the Left [Partito Democratico della Sinistra-PDS], with Occhetto himself as secretary.

A not negligible slice of the irreducible did not accept the turn and operated a split to the left, going to form the Communist Refoundation Party [Partito della Rifondazione Comunista-PRC] [...]. Within a year, the entire Italian political world would have been upset by a judicial scandal, namely the Mani Pulite investigation, known mainly as Tangentopoli, which ideally closed the first Republic, opening a new season of confrontations and debates (L'Italia degli anni '80. *Italics in the original text*).

In the 1970s, Selleri had shown a political activity, mainly based on the dissemination of science in order to make it understandable to anyone. In the 1980s he decided to devote himself to science full-time, always maintaining his beliefs and thoughts and underlining the deep intertwining between science and society. Concerning the reasons behind his choice, also describing his approaches, hopes, and failures, he stated to Freire that (see page 265):

The time at which I started being interested in the foundations of quantum mechanics were also times of great political unrest in the universities - you know, the famous years 1968 and 1969.

So in all universities, in particular in Bari, there were students occupying the institutes and organizing political debates. I was then a young teacher, and for me it was very natural to side with the students. I thought they were right and they should be helped to improve our society. From that started for me a political activity that later for some time was also as a member of a party. On one side for me it was a very important activity because I felt I made a cultural jump of quality.

Before I understood nothing about man, and after I had the feeling that I started understanding how we work, how we reason, how we react — not always rationally, of course. But then it was also very frustrating because I was too naive in my approach. My method was to declare what I wanted to achieve openly, and then my behavior was as much linear as possible to achieve openly those aims.

So it was very easy to shoot me down, a thing that happened several times, yes, because some people did not like what I wanted to do and could easily find all the information they needed to contrast my actions.

So from that point of view it was frustrating, but (I think, I hope) it was not really because I was too stupid. I thought that this was the right methodological approach, that you should be completely honest and transparent every time you try to do something.

It does not work, I tell you. Anyway, for sure I tried to carry out this political activity. This lasted a few years, it was three years including those spent as director of the Bari INFN section, until I understood that I was going nowhere. I was losing my time and so I gave up. **So I gave up politics very soon and I started doing my job, physics, full time teaching and research. I thought that there was a strong connection, because it seemed to me that the ideas of renewal of society and of physics had to go together.** There was (and is) a need for a completely different approach in physics and also for a different approach in society (AIP 2003. My underline and **bold**).

Selleri, however, was always involved in his action of denunciation and social criticism as well as in his political commitment against USA, capitalistic power and its aberrations. In the following years, at least until 2001, albeit not continuously, he wrote several very interesting books and papers⁶³¹ on the crisis of the natural resources, against the hazard linked to the militarization of Italian territory, and the escalation of the Cold War with the related risks connected to it.

The first book⁶³² published in 1980 by Selleri, together with Augusto Garuccio and Giuseppe Guarino, was *Nucleare e solare come alternative al petrolio*. Starting by a criticism to a MIT landmark report on the future projections of some major parameters concerning mankind (natural resources, food, population, industrial production, pollution), asked to the US institution by the “Club di Roma”⁶³³, the three authors built a deep analysis of the worldwide situation, pointing out the responsibilities, according to their vision. They dealt with the Italian energy development, introducing the matter of nuclear and solar energy, and analyzing the proposed laws to increase the use of the latter.

⁶³¹ See the Addendum 1.

⁶³² Garuccio A., Guarino G., Selleri F. (1980), *Nucleare e solare come alternative al petrolio*, Bari: Edizioni Dedalo. (Garuccio, Guarino, Selleri 1980) in the following.

⁶³³ See: <https://clubofrome.org/>. Accessed 24 September 2020.

They talked about methane, finally explaining the concept of energy in modern science, a theme that will be the basis of a Selleri's subsequent bestseller book, *Che cos'è l'energia. Movimento luce calore. L'antimateria*⁶³⁴. At the beginning of (Garuccio, Guarino, Selleri 1980) they stressed the problems related to the advancement of capitalist development, showing their political activism and militancy:

The mass awareness of the very serious problems that capitalist development generates as it advances (exploitation of human labour, the creation of enormous underdeveloped areas alongside the rich urban areas, the progressive worsening of ecological imbalances, the poor quality of life of large masses, even though extremely rich compared to the peoples of the Third World...) has given rise to apocalyptic visions of a future humanity forced to live in an increasingly restricted space and in a seriously polluted environment. Correspondingly, there has been a tendency for certain intellectual groups to move towards an idolization of the pre-capitalist or pre-technological past and to accept an attitude of defiance towards any form of development or towards the struggle to open up prospects for genuine civil and social progress. There is a clear interest of the dominant groups, responsible for the current serious problems, in sowing disorientation and mistrust in the opportunity to change the existing situation: it is not by chance, therefore, that proposals are being made based fundamentally on the idea of keeping the current situation unchanged with all its privileges for small minorities⁶³⁵ (Garuccio, Guarino, Selleri 1980, pagg. 5-6).

In another section, their militancy emerges really strong, perhaps in a too romantic and imaginative, almost unrealistic way, but which emphasizes the reason behind the Selleri's approach of a continuous dissemination, as simple and wide as possible, really addressed to everyone, against an increasingly sectarian and excluding language by insiders:

The working class, once in power, and using the knowledge created by the scientific and technological revolution, will be able to build a different world, peaceful, fair, and industrious.

⁶³⁴ Selleri F. (1982), *Che cos'è l'energia. Movimento luce calore. L'antimateria*, Roma: Editori Riuniti.

⁶³⁵ The original Italian text is in Addendum 3, pp. 1016-1017.

[...] It is all too evident that science itself has been modified and distorted by its use and that the [...] future of humanity also passes through a mass reaffirmation of the cultural heritage of the modern science⁶³⁶ (Garuccio, Guarino, Selleri 1980, pagg. 25-26).

Keeping on, they explained the criticisms to the MIT report:

[...] A fundamental peculiarity of the whole relationship, which must be immediately reported, is the disregard for the internal contradictions of humanity, which is studied as something fundamentally unitary.

[...] The underlying vice of the whole relationship also emerges in the setting of mathematical models for the projection of the various world parameters into the future.

[...] In simple terms, the M.I.T. report has taken into consideration an “American” world from which revolutions are banned: no wonder then that the projections are not very reliable. Having said all this, the report is equally interesting because in essence it shows us what would happen if the dominant forces were able to impose their way of life on the whole world and repress every force of transformation.

[...] The answer is dramatic: rapid depletion of raw materials, huge increase in pollution, inadequate increase in food and therefore widespread hunger in the world, increase in industrialization only until 2010 followed by a rapid decline, etc. In short, in all likelihood, the end of the civilized world around the year 2000 with probable annihilation of the human race due to hunger, terrible pollution, etc.⁶³⁷ (Garuccio, Guarino, Selleri 1980, pagg. 6-8).

Obviously, in hindsight, some of these predictions have not come true and were exaggerated. But historically the book remains an example of the fear of those years as well as the most varied activities followed by Selleri. On the other hand, it also showed several damages really occurred and related to an untamed use of the natural resources and to the disproportion of expenditure. Concerning the nuclear energy, Selleri was favourable to it for civil use, also because in this way Italy could have become independent from oil-producing countries and also develop its own technologies and know-how.

⁶³⁶ “I lavoratori al potere, usando le conoscenze create dalla rivoluzione scientifica e tecnologica, potranno edificare un mondo diverso, pacifico, giusto ed operoso. [...] È fin troppo evidente che la stessa scienza è stata modificata e distorta dall’uso che se ne è fatto e che il [...] futuro dell’umanità passa anche attraverso una riaffermazione di massa del patrimonio culturale della scienza moderna” (Garuccio, Guarino, Selleri 1980, pagg. 25-26).

⁶³⁷ The original Italian text is in Addendum 3, p. 1017.

In the dedicated chapter of the above book, together with Guarino and Garuccio, he stated:

The hegemonic position of the US industry in the reactor field is well known⁶³⁸ [...] and how this position rests not only on the control of plant technology but also of the raw material, the enriched uranium. Of the nuclear power installed worldwide, the 88% is represented by enriched uranium reactors (LWR) and only 6% by Candu (HWR). The consequences of this situation are easily foreseeable in the absence of a timely planning intervention aimed at diversifying the supply chains to be introduced into the Italian market; namely, the establishment of a relationship of subordination towards US corporations accentuated by the weakness of industrial and research structures in our country⁶³⁹ (Garuccio, Guarino, Selleri 1980, pagg. 85-86).

They also developed a deep analysis of the problem related to the disposal of radioactive waste and the accident at the Three Mile Islands (Pennsylvania, USA) nuclear power plant on 28 March 1979.

It should be noted that the book was written exactly 40 years ago, and well before the Chernobyl disaster in 1986.

In the wake of the Cold War, many pacifist movements developed. In Bari, in the Physics Department, there was a continuous presence of the Italian Union of Scientists for Disarmament. Selleri himself wrote many popular articles on issues related to the arms race and the risks of an escalation of conflicts, which were published in Italian newspapers.

Among them I remember here:

⁶³⁸ Always according to (Garuccio, Guarino, Selleri 1980): “(a) enriched uranium and natural water reactors, the so-called ‘American proven’, BWR (Boiling Water Reactor) produced under Westinghouse license and PWR (Pressurized Water Reactor) under General Electric license, both classified as LWR (Light Water Reactor); (b) Candu type natural uranium and heavy water reactors, the so-called ‘advanced converters’, HWR (Heavy Water Reactor) produced under Canadian license”

((a) reattori ad uranio arricchito ed acqua naturale, i cosiddetti ‘provati americani’, BWR (Boiling Water Reactor) prodotti su licenza Westinghouse e PWR (Pressurized Water Reactor) su licenza General Electric, entrambi classificati come LWR (Light Water Reactor); b) reattori ad uranio naturale ed acqua pesante di tipo Candu, i cosiddetti ‘convertitori avanzati’, HWR (Heavy Water Reactor) prodotti su licenza canadese”) (Garuccio, Guarino, Selleri 1980, pagg. 81-82).

⁶³⁹ “È nota la posizione egemonica dell’industria statunitense sul mercato dei reattori [...] e come questa posizione poggi non solo sul controllo della tecnologia degli impianti ma anche della materia prima, l’uranio arricchito. Del potenziale elettronucleare installato nel mondo l’88% è rappresentato da reattori ad uranio arricchito (LWR) e solo il 6% da Candu (HWR). Le conseguenze di questa situazione sono facilmente prevedibili in assenza di un tempestivo intervento pianificatore che punti ad una diversificazione delle filiere da introdurre sul mercato italiano; e cioè lo stabilirsi di un rapporto di subordinazione verso le multinazionali statunitensi accentuato dalla debolezza in cui versano le strutture industriali e di ricerca di settore nel nostro paese” (Garuccio, Guarino, Selleri 1980, pagg. 85-86).

- “Nel ‘raggio’ della bomba N” and “La bomba del terrore”, against the neutron bomb and its horrible consequences only on the human beings, without any damage to buildings and other stuff.
- “La paura nucleare come scelta strategica”, and the review to the book of D. Collingridge, *The Social Control of Technology*, on the social and political use of the nuclear fear;
- “La forza strategica dei sommergibili nucleari e la base de La Maddalena”, and “I sommergibili nucleari, la corsa agli armamenti e i rischi di guerra totale”, with a deep description of the features of the nuclear submarines as well as on the militarization and the occupation of the Italian soil, in clear violation of the Article n. 80 of the Italian Constitution and despite the lack of any treaty ratified by the Parliament and the President of the Republic granting the island of La Maddalena (in the Sardinia Italian county) to the USA to plant nuclear submarines.
- “Uranio impoverito e paesi impoveriti”, against the horrible situation experienced by a lot of soldiers during the war in Yugoslavia.

In the work “L’energia nucleare tra pericoli di guerra e uso pacifico” as well as in the “La paura nucleare come scelta strategica”, he talked about USPID (Unione Scienziati per il Disarmo)⁶⁴⁰, also established in Bari.

According to Cufaro Petroni, USPID is:

an activity of civil commitment and was not born in Bari. USPID was born from the scientific need of a group of physicists in the early 80’s to be able to say something. Actually the model is a bit similar to Pugwash’s, but they are different realities, even if there were some physicists in both. The idea is not to make propaganda but to make information. To be, as scientists, technicians, able to understand what the problem is, what it is and to make the solutions available. Obviously, if it is possible to find a position to support, do so. But not for propaganda. Each of us can do the propaganda we want but not with the name USPID. The idea is to have a credibility, impartiality and a credibility so that even those who are in government and don’t understand anything can listen to us and try to understand. And therefore have an influence at this level.

⁶⁴⁰ USPID is the Italian acronym for the Italian Union of Scientists for Disarmament. See also <https://www.uspid.org/> and https://en.wikipedia.org/wiki/Italian_Union_of_Scientists_for_Disarmament. Both accessed 24 September 2020.

This was a bit the original idea. This then grafted on to the fact that in the mid-1980s there was the Euromissile crisis. Which is the thing that many of us approached the group with. USPID was born with Bernardini, Amaldi, the Romans, Roberto Fieschi, Carlo Schaerf (Interview with Nicola Cufaro Petroni, Bari, 31 May 2019).

Selleri was considered as one of its promoters, but, according to the recollections of some active members of the USPID, he did not have a real business inside.

Actually, Selleri made a conference in 1983 in Ortanova (see Addendum 2, S50.), a village in the province of Foggia, at the local Peace Committee, lecturing on USPID slides. And, on 23 March 1985, he made a lecture, *“Sottomarini con armamento nucleare”*⁶⁴¹ at the Department of Physics-University of Bari during a workshop organized by USPID (with the presence of Fieschi, Nardulli, De Marzo, Cufaro, Maestro, Cotta Ramusino, see S70. in Addendum 2).

Both the foregoing conferences seem to lead to his adherence to USPID itself. Moreover, it is true that Selleri, together with Garuccio, Carlo De Marzo and Giuseppe Nardulli, appeared in 1990 and 1993 in the research project on topics pertaining to the USPID action, namely on the Bari project on *“Non-nuclear defence in Europe and Italy”*⁶⁴², managed at national level by Roberto Fieschi and at local level by Nicola Cufaro Petroni. And Selleri also was in the team, whose responsible was Giuseppe Nardulli, which proposed another research within the Italian plan on *“Nuclear weapons, technological developments and armament control”* (*“Armi nucleari, sviluppi tecnologici e controllo degli armamenti”*) specifically related to the risk of a nuclear war by accident⁶⁴³.

⁶⁴¹ *“Submarines with nuclear weapons”*

⁶⁴² *“Difesa non nucleare dell’Europa e in Italia”* in the original language. It was a project within the National plan on *“Nuclear weapons, technological developments and armament control”* (*“Armi nucleari, sviluppi tecnologici e controllo degli armamenti”*). In the research description they wanted to create a reorganization of the conventional forces, to review the military strategies with a perspective of deterrence related to conventional and non-nuclear weapons only. Moreover, to put the Mediterranean naval forces in a purely defensive key and to introduce a treaty making the latter a *“nuclear weapons free zone”*.

⁶⁴³ The danger was great also for *“the recent installation of intermediate-range missiles in Europe and the presence of nuclear-armed submarines off the US coast, [factors that have] significantly reduced the flight time of [strategic] missiles. The possible adoption of the ‘launch-on-warning’ system, the presence of about six thousand tactical nuclear warheads in Western Europe and the prospect of an acute political crisis between the*

But it is also true that the day-to-day reliability of these research programmes⁶⁴⁴, submitted with a view to obtaining funds, is all to be demonstrated. And furthermore, according to Nicola Cufaro Petroni, who was really active in the USPID, the link between USPID and Bari was constituted by Beppe Nardulli and not by Franco Selleri.

Vincenzo Paticchio stated the role of Selleri in USPID. He claimed that:

[In the USPID project in Bari] with Franco we did not do much. I do not see Franco honestly inside, I have never seen him involved, he had a limited presence. On the other hand, USPID is honestly a way like any other to make a contribution, but quite virtual. It is an extremely restricted assembly of intellectuals who cannot even get out of that assembly. Franco was very pragmatic from this point of view, he liked to make debates but quite elementary, so that everybody could participate. I remember when I was still a graduate, or young graduate, and the Left Party was still close to these issues, there were debates also in bookshops, where Franco went to bring people closer to concepts that are not simple if you do not have the right tools. And concerning USPID, I think Franco was more detached, more skeptical (Interview with Vincenzo Paticchio, Bari, 29 May 2019).

As often happens in HoS, the truth on the link between Selleri and USPID is only partially established and is always subject to further review. Anyway, Selleri made a lot of seminars into schools against the nuclear weapons (See Addendum 2).

Obviously, the possible lack of a real relationship with USPID does not affect at all the Selleri's political activity against wars. An evidence in this sense comes from the following letter, in which Selleri, as a left-wing man, harshly criticized the government presided over by Massimo D'Alema (former communist and secretary of the PDS from 1996 to 1998), which granted in 1999 the bases to NATO to bomb Serbia, in the Kosovo affair, and let the Italian planes also bombing Serbia, practically putting Italy at war, against

two major powers make the chance of a nuclear war by mistake not negligible. The aim is to investigate this hypothesis by carrying out an analysis of the decision-making mechanisms in the event of a crisis and a computer analysis, also using Monte Carlo techniques, of the average resolution time of fake alarms and the eventuality that this time exceeds the flight time of strategic missiles”

(AGAB-Progetti di ricerca-Richieste finanziamenti ricerca scientifica dip di fisica armadio D settore 22 palchetto 4 busta 1). The original text in Italian language is in the Addendum 3, pp. 1017-1018.

⁶⁴⁴ I am going to introduce several research programmes in the next section.

the Article 11 of the Italian Constitution⁶⁴⁵. After two years from the bombing, Selleri still wrote to support, even economically, people in Serbia:

Dear Darko, I will continue to send my little aides to you and your family [in Kragujevac]. They are not great things, but they are meant to express human solidarity and also a deep sense of shame for what our government has helped to do wrong against your country and your city. The first government headed by a left-wing man in the history of Italy helped the Pentagon generals to bomb a small country once again. How disgusting, how shameful⁶⁴⁶ (Selleri to Darko Nedovic, 16 March 2001-FSPA).

In 1984, during the PCI's campaign for the European elections, Selleri took part in many public rallies always speaking against nuclear weapons.

Finally, several years later, in a 1991 letter to Blandino, (a Jesuit who teaches Philosophy of Knowledge and Science at the "Pontificia Università Lateranense", and who reviewed Selleri's book *La causalità impossibile*), Selleri disputes the recipient's view on the war in Iraq and reiterates his view on the USA and the use of weapons:

[...] I think we do not get along very well with Bush. It is far from me to feel sympathy for Saddam, who made 8 years of war against IRAN and then made another bloody one against 30 countries, including the USA. But here, if we have to talk about Hitler, there are two of them. The American planes massacred the Iraqi population, beyond the propaganda chatter about the surgical precision of their weapons: out of 30,000 tons of bombs a small part was equipped with sensors, on-board computers, etc., the bulk was made of gravity bombs like those of World War II and Vietnam. And **they hit population centres wanting to hit them. And they made a massacre of innocent people wanting to do it. Is this western civilization? Thank you very much. Ever since I got to know them there the Americans have always frightened me.**

⁶⁴⁵ The Article 11 of the Costituzione Italiana stated that: "Italy repudiates war as a means of offending the freedom of other peoples and as a means of resolving international disputes; it allows, on an equal footing with other States, the limitations of sovereignty necessary for an order that ensures peace and justice between nations; it promotes and favours international organizations to this end" ("L'Italia ripudia la guerra come strumento di offesa alla libertà degli altri popoli e come mezzo di risoluzione delle controversie internazionali; consente, in condizioni di parità con gli altri Stati, alle limitazioni di sovranità necessarie ad un ordinamento che assicuri la pace e la giustizia fra le Nazioni; promuove e favorisce le organizzazioni internazionali rivolte a tale scopo").

⁶⁴⁶ "Caro Darko, io continuerò a mandare i miei piccoli aiuti a te e alla tua famiglia [a Kragujevac]. Non sono grandi cose, ma vogliono esprimere una solidarietà umana e anche un profondo senso di vergogna per quello che il nostro governo ha contribuito a fare di male contro il tuo paese e la tua città. Il primo governo con a capo un uomo di sinistra della storia d'Italia ha aiutato i generali del Pentagono a bombardare ancora una volta un piccolo paese. Che schifo, che vergogna" (Selleri to Darko Nedovic, 16 March 2001).

Here we have pious illusions about them, the truth is that they are a violent nation at mass and institutional level⁶⁴⁷ (Selleri to Blandino, 3 April 1991-FSPA. Selleri underline, my **bold**).

Selleri was always related to a social commitment, even though he decided, as revealed above, to quit the direct political activity and to devote himself to research, also thanks to the achievement of the full professorship.

2.8.3-REACHING THE FULL PROFESSORSHIP. THE GROUP ON FOUNDATIONS OF QUANTUM MECHANICS IN BARI AND HIS ROLE. SOME DIFFICULTIES IN PERFORMING FOUNDATIONS OF QUANTUM MECHANICS IN THE PHYSICS INSTITUTES AND DEPARTMENTS IN THE WORLD DURING THE 1970s AND 1980s.

In the Introductory Notes I told how Selleri reached the full professorship in 1980 (see pages 30-35). With his university accommodation and the greater peace of mind of a permanent lecturer, Selleri took new energies, publishing several books, living lectures and course well recollected by many students during the following years, organizing a lot of national and international big conferences, being a lecturer in others, having a lot of journeys for research, publishing many papers, and so on.

He managed to establish and enhance the FQM group⁶⁴⁸ in the University of Bari, at the INFN Bari Section (further referred to as The Group IV or simply, *The Group*), which was really active in his members, who were all intertwined with the activities Selleri had begun as well as the promoters of new ones. He began to create such a group in the 1970s, but during the 1980s, strengthened by his acquired full professorship, he developed *The Group* with collaborations and interchanges with other European Universities, several fields of research and assignments, too.

In the AGAB archives, I found a lot of official reports on *The Group*: research programmes, with abstracts, participants, objectives, research descriptions and publications by the members of the group.

⁶⁴⁷ The Italian original text is in Addendum 3, p. 1018.

⁶⁴⁸ In the scientific area number 02.

Indeed, the first report dates back to 1972, when Selleri, although being certainly not yet a full professor, was the responsible for several projects already related to “Fondamenti storici e filosofici della meccanica quantistica”⁶⁴⁹, with a group of six members, a request for financing and a proposal of 10-days assignments in France, Denmark, and USSR to research sources in the archives and to try speaking with Omeljanowsky and Fock⁶⁵⁰. The research goal dealt with:

[...] an interweaving of historical and epistemological problems and also of avant-garde scientific research (problem of causality, reality of the wave function, etc.).

The historical aspect is, however, the primary one, in the sense that only having fully clarified certain important issues such as the links between Bohr and Høffding (fully aligned with Kierkegaard in epistemology), the specific form of social pressure on quantum physicists in the 1920s, the role and interpretation of the QM in the Soviet Union (with particular reference to Fock’s work) we can hope to have an unambiguous basis for further developments⁶⁵¹ (AGAB-Progetti di ricerca-Richieste finanziamenti ricerca scientifica dip di fisica armadio D settore 22 palchetto 4 busta 1).

Concerning the research methodology, Selleri proposed a contact with those Italian scholars who have already produced relevant contributions to the clarification of the historical and philosophical foundations of the QM (Geymonat, Tagliagambe, Giorello, Baracca, Scotti) and to make the most of their criticism and advice.

Then Selleri recalled his seminal researches and studies on historical and philosophical FQM during his stay at Gothenburg in 1966 and in Lincoln-Nebraska in 1970/71, as I showed in Chapter 1. During his second stay in Sweden, he held a class on the history of QM.

⁶⁴⁹ “Historical and philosophical foundations of quantum mechanics”.

⁶⁵⁰ I have already described Garuccio and Selleri’s trip in USSR at pages 244-248.

⁶⁵¹ “[...] un intreccio di problematiche storiche, epistemologiche e anche di ricerca scientifica di avanguardia (problema della causalità, realtà della funzione d’onda ecc). L’aspetto storico è tuttavia quello primario, nel senso che solo avendo chiarito fino in fondo certe importanti questioni quali i legami tra Bohr e Høffding (del tutto allineato con Kierkegaard nell’epistemologia), la forma specifica dell’esplicarsi di una pressione sociale sui fisici quantistici negli anni venti, il ruolo e l’interpretazione della MQ nell’Unione Sovietica (con particolare riferimento ai lavori di Fock) si può sperare di avere una base di partenza non ambigua per gli ulteriori sviluppi” (AGAB-Progetti di ricerca-Richieste finanziamenti ricerca scientifica dip di fisica armadio D settore 22 palchetto 4 busta 1).

Of course I am fully aware of the relative historical value of these documents, being mostly official projects which did not always correspond to the detailed and diversified activity which was then carried out and further developed in everyday life. This was confirmed to me in every interview I had with members of this group, which were free to have their own further research autonomy. In their interviews, Cufaro Petroni seen the above documents as a formal act; and Paticchio spoke about a deliberately extensive detail to justify the research. Garuccio stated:

[Franco] had imposed this rule on the group: everyone had to find their own way for a while.

[...] Franco said that we had to diversify with respect to different types of research. We no longer had to work in groups but on different approaches (Interview with Augusto Garuccio, Bari 27 May 2019).

Nevertheless, these sources can show the evolution of the research conceived by *The Group* and very similar to the ideas of Selleri, the scientific director of research projects. In addition, they form a good summary of the issues developed by Selleri, a good abstract, so to speak.

Among the various scholars who participated at *The Group* in those years, starting the 1972 to the early 2000s, (as a member or also as the expression of international partnerships), I am recalling, without being exhaustive, Augusto Garuccio, Ramon Risco Delgado, Vito Luigi Lepore, Roshan Foadi, José Ramalho Croca, Dino Fortunato, Saverio Pascazio, Nicola Cufaro Petroni, Liberato De Caro, Diego Gutkowski, Michael Schmidt.

Representing the collaboration Bari-Catania on Bell's inequality and empty waves experiments, stand the names of Luigi Fornari, Vittorio Antonino Rapisarda, Giuseppe Iaci, Franco Falciglia, Lorenzo Pappalardo and Adriano Gozzini at Pisa deserve to be reminded.

Concerning the project about some experiments on dualism and empty waves with France and Fondation Louis de Broglie, there were: Georges Lochak, Simon Diner, Jean Pierre Vigier, Gino Tarozzi.

Another project by Nicola Cufaro Petroni, which included Selleri, Jean Pierre Vigier, and Zvonko Marić and D. J. Živanović by Belgrade University, was focused on stochastic processes and quantum mechanics.

Finally, with respect to the 1993-1995 project on nonlinear optics and integrated optics⁶⁵² it is worth mentioning Leonard Mandel, Davis Andrew Branning, Justin Roald Torgerson, Carlos Henrique Monken⁶⁵³. In order to stress the importance of Selleri in these plans, I recall here how in 1996, jointly with Garuccio, the above authors wrote a paper whose title is: “Experimental test of Selleri’s variable photodetection-probability model”⁶⁵⁴. The diversity of the above names is also linked to the circumstance that several members of *The Group* requested access to funds for their research projects, in accordance with the independence always ventilated and encouraged by Selleri, thus widening the field of research and collaboration within *The Group* itself.

In this regard, Cufaro Petroni pointed out how Selleri taught from his beginning at University of Bari that publishing works and creating projects was an activity to be developed, as it was almost absent in the Institute:

Franco was one who published a lot. For a long time in this Institute most theorists published very little. The idea that one had to publish in order to make a career was not here. Nobody had explained it to me.

The first one who made me do some work was Franco, who actually told me that maybe I should write and publish (Interview with Nicola Cufaro Petroni, 31 May 2019).

In this way Selleri trained, helped and made numerous scholars grow, a consistent behaviour always maintained even sending letters to the various military districts asking for the presence of his students at conferences, thus allowing them to have licences and continue to do science even during the

⁶⁵² A field belonging to nanophotonics.

⁶⁵³ This last project dealt with some tests of Einstein locality with optical sources, measurement of the violation of the locality without the use of inequalities, new sources of photons correlated in momentum and polarization, anomalies introduced by quarter-wave plate, alternative models for correlated pairs.

⁶⁵⁴ Garuccio A., Torgerson J.R., Monken C.H., Branning D.A., Mandel L. (1996), “Experimental test of Selleri’s variable photodetection-probability model”, *Physical Review A*, 53 (5), pagg. 2944-2947.

year dedicated to military service (as Michele Barone mentioned in his interview).

Anyway, the situation related to the participants in the projects of *The Group* was not so straight, as often happens. For instance, concerning his real belonging to *The Group*, Cufaro Petroni stated that:

The working group on FQM in Bari was a formal event. There was certainly no contrast, but it is not that I followed the group's activities very much. At a certain point the streets became disjointed because I went to Paris and started working with Jean-Pierre Vigier. This fact of being linked to ideas, to Jean-Pierre's road put aside Franco's road. So this is why I was never a direct collaborator of Franco. (Interview with Nicola Cufaro Petroni, Bari 31 May 2019).

The plans that *The Group* proposed in the years between 1972 and 2010, as submitted in AGAB official reports on research programmes, included, in any order:

- The study of EPR paradox applied in particle physics, with neutral kaons pairs or mesons B^0 ; and in quantum optics, with the PDC;
- Theoretical and experimental study of entangled states;
- Quantum states and interferometry using phase conjugating mirrors;
- Production and detection of related photons;
- Research on the historical and philosophical foundations of quantum mechanics;
- Studies on the long-range behaviour of related microscopic systems;
- Foca II, i.e., the experiment on photon pairs emitted in an atomic cascade using the four-coincidence, made within the Bari-Catania collaboration⁶⁵⁵;
- Principle of correspondence and concept of state in quantum mechanics;
- Wave-particle dualism, locality and separability;
- Wave-particle dualism and stochastic interpretation of quantum mechanics;
- Stochastic processes and quantum mechanics;

⁶⁵⁵ See, for instance, Falciglia, F., Garuccio A., Pappalardo L. (1982), "Rapisarda's experiment: on the four-coincidence equipment 'FOCA- 2', a test for nonlocality propagation", *Lettere al Nuovo Cimento*, 34, pp. 1-4.

- Foundations of quantum mechanics;
- Causal description of quantum objects;
- EPR paradox and theories equivalent to relativity;
- Mathematical problems in quantum mechanics;
- Research on de Broglie's waves and causal interpretation of quantum mechanics;
- Theoretical-experimental problems of quantum mechanics.

Regarding these last two projects, *The Group* proposed to study in 1986 for the former; and to build in 1988 the proof of the impossibility of transmitting superluminal signals, for the latter. Selleri also dealt with this latter issue, extending this interest also in his activity in the FRT. According to some sources (Baracca, Bergia, Del Santo 2017) Selleri experienced at the end of 1970s the issue on superluminal signals, but I found several papers belonging to the end of 1990s and the early 2000s in which Selleri further expands the subject.

For the sake of thoroughness, in order to point out Selleri's key role in the research at Bari University, his relationship with several scholars there, and his versatile and manifold interests related to thoroughly analyze the great field of physics, in the last years of Selleri's activity in the Department of Physics in Bari, and during and after his retirement, he took part to other projects programmes presented in the University by Garuccio.

The Group produced a broad and valid number of papers, ever released in the main journals. Obviously, the publications are so numerous that they cannot be mentioned here, being our research centered on Selleri's work. It may be useful to search for publications of members belonging to the group and mentioned in the previous pages.

The Group experienced some animosity within the Institute and the Department of Physics. I found three letters, quite detrimental, really explanatory of a certain degree of disagreement and dissatisfaction for the activities of *The Group*, maybe seen as competitive in the partition and allocation of the available funds.

The first letter is sent by Selleri to Aldo Cossu⁶⁵⁶, just appointed as Dean to the Faculty of Sciences at the University of Bari. The letter could sound detrimental, but I introduced Selleri's peculiar temper and he was trying, in his thoughts, to uphold *The Group* in the Physics Department (at that time still defined as Institute of Physics). Of course I omitted some people's names directly interested in the issue:

Dear Cossu, to help you carry out the delicate task entrusted to you by the Faculty of 30 u.s., I think you may need the following information of which, I am sure, you will be able to make the best use so that a new climate might be generated in the Institute of Physics.

1. When Prof. [Giuliano] Preparata⁶⁵⁷ came to Bari to take up the chair of theoretical physics, he soon expressed his wish to close the meaningless (to him) research activities in FQM that I and my small group had been conducting for several years. The atmosphere became particularly harsh at the end of 1977 when rumours circulated, spread artfully, that Preparata was in agreement with the INFN management to close these activities, even blocking all forms of financing. Normal scientific methods, such as one or two internal seminars on our activities, did not seem to give results. So I resorted to what seemed to me at the time the only hope to save our activity by writing directly to Zichichi⁶⁵⁸.

His response was very encouraging and served for some time to stop the repressive offensive. Indicative is the way in which Preparata welcomed this news.

The following episode was reported to me [...]. In a room on the second floor of the Physics Institute, Preparata was speaking against the research of our group when someone told him that we had received a letter of support from Zichichi. At which Preparata stopped suddenly and exclaimed: "No, Nino [Zichichi] should not have done this to me!".

2. In 1978, a group of undergraduates from Pavia led by some teachers went to Geneva to visit CERN. Prof. Preparata, who was then a "staff member" of that laboratory, was appointed to welcome them. As part of the entertainment, they were given a very hard pull against "that crazy Selleri who wants to destroy the Quantum Mechanics". There was a reaction from the Pavia people.

⁶⁵⁶ Aldo Cossu (1922-2005): See <http://www.treccani.it/enciclopedia/aldo-cossu/>. Accessed 24 September 2020.

⁶⁵⁷ Giuliano Preparata (1942-2000): See https://en.wikipedia.org/wiki/Giuliano_Preparata and [http://www.treccani.it/enciclopedia/giuliano-preparata_\(Dizionario-Biografico\)/](http://www.treccani.it/enciclopedia/giuliano-preparata_(Dizionario-Biografico)/). Both Accessed 20 September 2020.

⁶⁵⁸ The INFN President at that time.

The reaction consisted in the fact that Prof. Piazzoli, director of the INFN section of Pavia, invited me to hold a seminar in that Institute with the explicit motivation of wanting to balance the destructive work of Preparata towards me, showing the graduates that our research was valid.

3. In the last competitions for the full professorship, the one who was intended to be my main supporter [...] was elected and drawn in the jury. To my great despair I received a series of phone calls from him at the beginning of the 80's in which he told me that Preparata was doing a "terribly convincing" destructive work against me. In essence [...] he told me to no longer count on his support. Months of hell followed, even if the story ended very favourably because other commissioners of the competition supported me beyond my own expectations. Everyone in the Institute knows that my being among the winners of the competition was a defeat for Preparata and it does not seem normal that this should be the case.

4. The climate among Bari's theorists, created by Preparata, was such that episodes of this kind were possible: there was a meeting of Bari's theorists, from which I was absent, when the outcome of the professorship competitions was known. The youngest of them [...] spoke out to denounce the anti-scientific choices made by the national theoretical commission, such as, for example, "having given a chair to Selleri". The episode was reported to me [...].

5. Our group's 1981 INFN fund applications were not submitted by Preparata to the National Coordination of Theorists, with the result of damaging our business. Trips and seminars were suspended for about 6 months. The situation was reported by me in a letter of 11 March 1981 to the INFN President. It was only in June 1981 that the INFN was able to examine and finance our research. There is a counter letter from Preparata that escapes the substance of the problem.

These are the main facts. There are others concerning the PhD and our exclusion, etc., which I can enter if you think it is appropriate.

However, it seems to me that the picture is quite clear. Many friendly regards⁶⁵⁹ (Selleri to Aldo Cossu, Bari 1 October 1981. AGAB-Istituto di fisica e dipartimento di fisica-Personale Dip Fisica Armadio D settore 21 palchetto 9 busta 2)

I did not find which were the consequences of Selleri's letter to Cossu on the climate in the Institute of Physics.

But during his interview, Cufaro Petroni made an analysis of how *The Group* was seen by the rest of the scholars, giving also some information of the role and figure of Giuliano Preparata in those quarrels.

⁶⁵⁹ The Italian original text is in Addendum 3, pp. 1018-1019.

Obviously, I have not reported the names of the people involved, given the sensitive situation introduced by Cufaro Petroni:

Those who did ordinary physics, such as field theory, high energy physics, etc., always saw the Selleri group and those who were interested in FQM as freaks, as crackpots. There has never been any mystery about this. This is because Selleri's group activities were linked to a point of view, to a way of seeing that had already been overcome by facts and that was purely dogmatic (according to the ordinary physicists). Selleri has always been seen as someone who does not want to be convinced. And the members of the group as people who waste their time, and that it is better to let go. This has always been the ordinary point of view.

For instance, with respect to Bell's inequality, it is accepted that it is violated, and that this is seen experimentally. The ordinary physicists say, well, that's the way it is, that's accepted!

Whether one wastes one's time thinking that we can interpret quantum mechanics in another way, or that the Aspect experiment does not work because there are these additional hypotheses and this low efficiencies, most people get annoyed. And so there was a lot of distance.

Moreover, Preparata was one who said these things directly and hard-faced. It was easy that there were misunderstandings. It was a turbulent period, that one in the Institute. Preparata was arguing with everyone. He quarreled with one and allied himself with the other; then he quarreled with this latter and allied himself with another, etc. He fought progressively with [several colleagues] and with Selleri. Well, among other things, there was an ideological rivalry with Selleri, because Franco was a communist and Preparata was not. There were some old things, too.

With [another colleague] Preparata had a bloody quarrel, which ended badly. He had a fight with everyone. Only one group remained tied to him in the end, probably because none of them had organizational or political ambitions (Interview with Nicola Cufaro Petroni, Bari 31 May 2019. My underline).

In another letter sent by Selleri to Aldo Cingolani⁶⁶⁰, the perception sounds as an attempt to seek an endorsement of the validity of *The Group's* work, to let

⁶⁶⁰ Aldo Cingolani (1940?-1990). It has been impossible to find any information online on him, apart from a few references in the curriculum of other scholars. On web, there are a lot of pages concerning his son Roberto. Anyway, according to Garuccio: another important character was Prof. Cingolani. He was also a character of great depth. He was the leader, here inside, of the applied physics section. The birth of the solid state in Bari is due to Prof. Cingolani. I followed his course with great interest. I was almost interested in doing my thesis with him. Very good, very involving in the students" (Interview with Augusto Garuccio, Bari, 27 May 2019).

people know about the activities on FQM. The letter is mainly significant for several details on Selleri's activity and endorsement all over the world:

I think you should be informed about the development of FQM research in Bari. In fact, you can hardly have sufficient information already now, given the scarce scientific contacts we have had so far.

I would like to start by mentioning two recent works. I have recently sent you a copy of the first one ("Can an actual existence be granted to quantum waves?").

[...] It may be interesting that A. Gozzini of Pisa is setting up an experiment to control our idea. The second ("Generalized EPR-paradox") introduces a new way of deducing inequalities from reality and separability and allows to overcome previous conceptual difficulties of a theoretical nature. I think that [...] will be very useful for the analysis of the experiment in Catania that was born on the initiative of Bari and that is about to reach the data collection.

I am deeply convinced of the fact that these two works open up new lines of research in many ways and that we will have to work hard to develop all the implications. We therefore expect a strong increase in scientific productivity in the coming year.

The Perugia [International] Conference on the dualism of wave-particle was also born on our initiative: we did not want to appear on the announcement for obvious reasons of a Bari nature. On the other hand, we cultivate an intense international activity, as shown by the seminars held in late 1981 in various European universities and by the graduate course I held in Vienna. These research activities are not separated from the production of books: *The Debate about quantum theory*⁶⁶¹ is a book that will be published first in German, translated by Prof. R. Sexl, professor of theoretical physics in Vienna, and then probably in English. *La natura dell'energia* is about to come out in these months in Italy. A textbook with a new approach to the QM is in preparation for Reidel publisher. You will notice [...] a solicitation to send work to *Foundations of Physics*: solicitations of this kind are not at all unusual for us.

These activities have brought several international awards, the most important of which are: an invitation to join the New York Academy of Sciences, inclusion in the 5th (and future 6th) edition of *Who Is Who In The World*, inclusion in the *Men Of Achievement*. Our kind of research is also expanding as evidenced by several facts of which I will mention just one: famous physicists are beginning to work on the foundations of quantum mechanics.

Three examples are Marcel Froissart, considered the best French theorist; Peter Freund of the University of Chicago, one of the founders of the symmetries of elementary particles, and Oreste Piccioni, one of the discoverers of the proton, the antiproton, etc..

⁶⁶¹ The correct titles of German and English books are *Die Debatte um den Quantentheorie* and *Quantum Paradoxes and Physical Reality*, respectively. See The Bibliography and the Addendum 1.

I will send you three files on the activities of the other researchers in the group (Cufaro, Fornari and Garuccio), sure you will find interesting information there⁶⁶² (Selleri to Aldo Cingolani, 4 January 1982-FSPA. My *italics* and underline).

Another confirmation comes from a letter to Giovanni Cicuta⁶⁶³ on the activities of *The Group*, in which Selleri, with the raw frankness that distinguished him, speaks of praise and recognition for him and his group's work against all criticism:

Dear Gianni,

Thank you for your time yesterday and the honest discussion we had. I am really grateful to you, and not only in a formal way, because it is rare for someone to have the will to tell another one unpleasant "truths" in an atmosphere not of confrontation but on the contrary of calm dialogue. I know from experience how much certain silences weigh and I find it nice that you have honestly wanted to tell me your opinion. I do not pretend here to reproduce exactly what you have told me, but to remember only one point that emerged clearly. Too many people in theoretical physics express criticism, or perplexity, or reservations about the kind of research that I and "my" small group carry out.

[...] Physics is a science, one cannot reason by hearsay, whatever the authority or (presumed) competence of the interlocutors. Perhaps you should understand that we would not have been able to practice this activity for so many years without having logical, scientific and valuable (to us) results. They have all been and are so important that they largely offset the adversities that have surrounded us. Quite frankly, I would like to invite you to be documented or keep silent.

If you want to document yourself, we are here really ready to provide you with any written and/or oral explanation on physics, and any documentation on the sometimes remarkable praises we receive in very authoritative journals and from well-known personalities. Of course it is your full right not to do any research, nor have I ever exerted the slightest pressure to do so. But if you do not inform yourself, forget the chatter in the corridors⁶⁶⁴ (Selleri to Gianni Cicuta, 30 November 1990-FSPA. My underline).

It must also be said that at the end of 1970s and the early 1980s, it was still very difficult to do FQM in the Physics Departments.

⁶⁶² The Italian original text is in the Addendum 3, pp. 1019-1020.

⁶⁶³ Giovanni Cicuta was a Professor of Theoretical Physics at the University of Parma. I did not find anything on web, except two pages in which there are some ambiguous information, like papers published in 1972, a period in accordance with his activity, but also other papers published in 2019. I preferred not to mention them here.

⁶⁶⁴ The original Italian text is in Addendum 3, pp.1020-1021.

An evidence in this respect comes from some letters I found in the FSPA, concerning some requests from Selleri to several Universities in the USA on the opportunity of a position for Augusto Garuccio in the FQM field. In the USA the topic was still seen, in those years, as something philosophical, even if some respondents showed interest in the FQM and for a comparison with Selleri and Garuccio!

Firstly, I am introducing here a letter by Philippe Eberhard⁶⁶⁵, at Lawrence Laboratories at Berkeley:

[It is] impossibile for us to offer a one or two year job to Augusto Garuccio in the group I belong to because it is an experimental group. I have worked in Foundations of Quantum Theory only on the side.

[...]Foundations of Quantum Theory are not in the mainstream of the theoretical activity in Berkeley. Most of the work in that direction has been performed by Henry Stapp who then would be most qualified to judge Garuccio's work and abilities. He is currently in Saclay. I would always happy to talk to Garuccio about Foundations of Quantum Theory, a subject for which he, you and I have a common inclination (Philippe Eberhard to Selleri, 14 November 1978-FSPA).

Then, it is the turn of Pierre Noyes, at Stanford University:

[...] Unfortunately [Garuccio] interests do not command sufficient attention with the other theorists here.

[...] I am forwarding your letter and his publication list to prof. Suppes in the philosophy department on the Stanford campus in hopes he has more flexibility.

He runs a very interesting seminar on the Foundations of Quantum Mechanics and could also provide a friendly environment.

[...] I very much like the way you and Garuccio have been refining the concept of Einstein locality, and hope to have an opportunity for discussion with you about this in future (Pierre Noyes to Selleri, 10 January 1979-FSPA).

Another answer came from two letters sent by Mendel Sachs, at the State University of New York at Buffalo:

⁶⁶⁵ He will be seminal for the development of the issue of Selleri's research on the dissimilarities between QM and LR with neutral kaons. See paragraph 2.9.

There is no money available for any research post in physics...however we still do have a temporary teaching position available (great deal of competition for this).

[...] there is unfortunately no one else [than me] in our Department who is actively interested in Foundations of Quantum Theory (Mendel Sachs to Selleri, 17 November 1978 and 23 January 1979-FSPA).

Finally, Abner Shimony at Boston University stated that:

[I shall suggest Prof.] C.A. Hooker [at the] University of Western Ontario. There is very active research on Foundations of Quantum Mechanics there. I shall suggest [Garuccio] as a speaker at the Boston colloquium for the philosophy of science (Abner Shimony to Selleri, 16 May 1979-FSPA).

As I told before, one of the main activity in Selleri's research as well as in *The Group* was strictly related to empty waves and to the experiments to reveal them. I am going now to introduce this key topic in Selleri's research.

2.8.4-THE LONG CHASE FOR A REAL EMPTY WAVE

Wave-particle duality was another cornerstone in Selleri's research, an ontological aware and deliberate choice. Within this field, empty waves (further referred to as EW) played a really compelling role for him, giving a way to try resolving the several dark outcomes of QM in a realistic and local approach. EW played also a key role in Selleri's shift to FRT.

Having already mentioned in section 2.3 empty waves, their history, the very early Selleri's approach submitted in (Selleri 1969a), and his correspondence with Louis de Broglie, I am now recalling Selleri's 1969 approach and going further developing his work in the 1980s, related to the work of other scholars inside and outside of the FQM group in Bari University. Also in the 1990s empty waves were the subject of an intense debate in the pages of *Physical Review Letters* made by Wang, Zou, Grayson, and Mandel on one side; and Croca, Garuccio, Lepore, and Moreira on the other side, also related to some proposal made by Selleri.

I would like to stress at the outset that the pivotal peculiarity of Selleri's approach, completely consistent with his philosophical view, rests on the choice of a real three-dimensional physical space for the propagation of

empty waves, which even de Broglie supported, albeit at alternate stages. In this regard, Garuccio evidenced that:

Both Franco and I started from a strong idea [...] that waves were real objects, that they had real phenomena, that they were not just waves of probability (Interview with Augusto Garuccio, Bari 01 July 2019).

Surely a view really different from the $3N$ configuration space accepted by Bohm and the late Vigier. An interesting evidence and interpretation on this different choice between Vigier and Selleri comes from Cufaro Petroni, which stated that:

Jean-Pierre [Vigier] had no problem going into the configuration space, that is, the phase space, not the 3-dimensional physical space but the $3N$ dimensional space. And there, clearly, you lose the idea that the wave function is something physical.

[...] He was much more pragmatic from this point of view, less ideological. So the fact that he was going into $3N$ space never bothered him.

[...] The wave drives the particle, in a way it has a physical entity of its own, it is a doubtful fact, largely doubtful. The problem is that usually this wave, if it has its own reality, is related to the fact that its square module is a probability density. It is difficult to say exactly what Jean-Pierre thought. Very pragmatically he modified things according to need. His point of view was never unique; if there were contradictions, he used to take points of view that might not even be consistent with each other. He adapted himself, and in this he was very skillful at adapting. He was never influenced by authority. He followed the needs of ideas. When there was a contradiction he try to circumvent it, not that he fixed that it has to be resolved in a certain way. He was tied to ideas, not to other mechanisms (Interview with Cufaro Petroni, Bari, 31 May 2019. My underline).

In (Selleri 1969a)⁶⁶⁶, the author introduced the concept of empty waves (also called quantum waves), defining it with “two words [that] are taken as equivalent to wave propagating in space and time but devoid of energy-momentum”. The advent of hidden variables theories allowed a new philosophical attitude which binds particles and waves. They are “objectively existing theories”. A real, objective property of nature, in Selleri’s thoughts.

⁶⁶⁶ I am going to recall some statements already introduced in paragraph 2.3. For the appropriate references look at that section.

This sounded as a revelation for Selleri, because “real waves associated with real particles in principle explain everything. That was the so called dual approach. Real waves and real particles propagating together. If you have that, you can understand the double slit experiment without any difficulty, even though many important books claim that it is a phenomenon going beyond the human reason”.

But soon arose the main problem behind this approach: the existing wave is not associated with observable quantities as energy and momentum, which belong and are associated to the particle. How can we see some phenomena related to the waves?

According to (Selleri 1969a), “we do not only measure energies, momenta and so on. We also measure probabilities, e.g. the lifetime on an unstable system. The wave function could acquire reality, independently of the particles associated with it, if it could give rise to changes in the transition probabilities of the systems with which it interacts”.

Hence, Selleri was pointing out that we measure energy changing processes but also probabilities. So, the empty wave could reveal its presence by modifying decay probabilities of the unstable systems! That is his central idea, as he himself states.

Selleri emphasized his preference for waves, always maintaining a strict wave-particle duality (a firm point in his realistic approach), by claiming that “It is the wave and not a localized particle-like structure which induces stimulated transitions in atoms”, giving a priority to the energy-momentum conserving processes and underscoring the untenability of Bohe, Kramers, and Slater (BKS) approach.

According to Nutricati, Selleri, together with Tarozzi, underscored the duality of electromagnetic radiation and still explained plainly the double slit experiment outcome⁶⁶⁷:

⁶⁶⁷ The italian original text is in the Addendum 3, p. 1021.

In 1978 Franco Selleri and Gino Tarozzi proposed a realistic interpretation of the wave-corpuscle phenomenon of light (“Is Nondistributivity for Microsystems Empirically Founded?”). Both [...] deny a uniquely corpuscular conception of electromagnetic radiation. [...] the presence or absence of interference is explained by the association of the unperturbed quantum wave with the corpuscle.

[According to Tarozzi] “On the basis of this interpretation it was possible to provide a physical explanation of those situations that give rise to one of the most serious paradoxes of measurement theory, [...] in which we have only one instrument inserted behind one of the slits and we consider the cases in which it does not reveal the passage of any micro object and the arrival of a particle is nevertheless recorded on the second screen. In this case, on the basis of our interpretation, the distribution of the interference can be explained not as a consequence of our knowledge of the behaviour of the micro object, acquired *not by observing* the occurrence of a certain physical event but as a result of the interaction of the quantum wave with the measuring instrument” (Nutricati 1998, pagg. 67-68. *Italics* in the original text. My underline).

As I shown in section 2.3, in the early approach introduced in (Selleri 1969a) and in (Selleri Varenna 1971), Selleri stressed a proposal of experiments with neutrinos in order to detect empty waves. I am going to recall it again: “a continuous beam of neutrinos traverses a piece of matter in which unstable entities (nuclei, excited atoms or molecules) are contained. The lifetime of these entities is measured in such conditions and compared to the lifetime of the same entities in the absence of any passing beam. If a difference is observed, its only logical explanation is that it is due to the action of the wave function, since the neutrinos are extremely weakly interacting particles and only a few of them, at most, can have interacted in the piece of matter with presently available neutrinos intensities”.

This issue was at the basis of a correspondence with de Broglie (see pages 192-196 in this dissertation)

As usual, Selleri focused on the disagreement between realist and orthodox physicists also about the nature of quantum waves. In the standard interpretation of QM waves are only probability amplitudes, with no direct physical meaning. According to Selleri, it has been de Broglie which maintained the quantum waves as objectively real with a well defined normalization: for the French physicist, a quantum system is composed of an

extended region (“wave packet”), and of a particle, guided by the wave in its propagation. Selleri paved the way with his 1969 and 1978 papers, and in the Eighties several scholars began, at least in principle, to study the opportunity to lead to the experimental observation of effects typically due to the propagation in space and time of a real wave.

Selleri mainly turned his focus on the experiments, which can discriminate between the orthodox probability amplitudes and the objectively real waves in space and time. He recognized a dozen of possible experiments proposed, which, if performed, could give rise to a radical change in modern physics.

He also recalled the first old experiments on self interference, like the neutron interferometry, developed by Rauch, Treimer and Bonse in 1974, who published a book in 1979; and with Janossy-Naray experiment in 1958 on optical photons.

I am going to show how almost four phases in the proposals of experiments for detecting EW could be recognized, always related to the enhancement of techniques and devices, even though, conversely to the Bell-type experiments, the tests on EW have never fully developed. I would like that this current rating is absolutely provisional and not thoroughly exhaustive and is subject to further improvements and adjustments, being only a personal proposal.

Leaving aside the **first** phase, still related to what I have been said about EW in relation to the earlier years, two phases (the second and the third) may be introduced during the Eighties; and the fourth in the early 1990s.

In the **second** phase, which could be defined between 1980 and 1986, the central idea rest in the modification of the decay probabilities of the unstable systems, later followed by the property of stimulating emissions from excited systems. Laser gain tube and single photons are the main characteristics.

In the **third** phase, starting the 1987 until 1990, the activity was related to photon amplifiers, to the amplification of coherent and incoherent light, being the latter a simpler system in which to operate; and to the neutronic interferometry.

In the **fourth** phase, at first developed in conjunction with the third phase and going to 1992 and beyond, enters the idea of the three-slit experiments and the quantum-optical predictions, with experiments using two-photon interference and PDC, with the contributions by several scholars as Ghosh and Mandel in 1987, Ou and Mandel in 1989, Croca, Garuccio, Lepore, and Moreira, in 1990, and the foregoing debate in the pages of *Physical Review Letters* made by Wang, Zou, Grayson, and Mandel on one side; and Croca, Garuccio, Lepore, and Moreira on the other side between 1991 and 1992. Some remarks on these phases are introduced in the reports related to the research programs for the request of funds that I identified in AGAB. Selleri explained how the problem of dualism is faced by him and his group trying to give a physical meaning to de Broglie's wave, a wave propagating in 3-dimensional space and time and that can generate observable effects, regardless of the corpuscle it carries. According to Selleri:

The idea is that the wave is a wave of probability also in a new sense, actively. That is, it could modify the probabilities of events with a fixed energy balance with which it would spatially overlap.

[...] Scientific contacts have been established with Prof. Gozzini of Pisa and with the neutron interferometry group of the AtomInstitute of Vienna (Rauch), trying to create the conditions for an experimental activity on the wave-corpuscle dualism

(AGAB-Progetti di ricerca-Richieste finanziamenti ricerca scientifica dip di fisica armadio D settore 22 palchetto 4 busta 1).

Then, with the advent of laser gain tube and single photons as basic features:

[...] We have studied the effects of the empty wave propagating in a medium containing excited atoms ("laser gain tube"). But there is still a lot to do.

[...] laser excitations from single photons, superfluorescence, neutron interferometry, give concreteness to the question of wave-particle dualism, allowing a potential study of empty waves (AGAB-Progetti di ricerca-Richieste finanziamenti ricerca scientifica dip di fisica armadio D settore 22 palchetto 4 busta 1).

As regards the second phase, a research program for detecting observables differences between the Copenhagen and the statistical interpretation of QM

started with the paper published by Garuccio and Vigier in 1980, “Possible experimental test of the causal stochastic interpretation of quantum mechanics: Physical reality of de Broglie waves”⁶⁶⁸. They formulate the proposal to modify the famous experiment of Pfleegor and Mandel on the interference between photons coming from two different lasers in order to establish the simultaneous existence of the corpuscular and undulatory aspect of matter even with the use of incoherent light source and laser amplifier instead of the two lasers. As they stated in the abstract:

Mandel and Pfleegor have shown the existence of interference effects produced by the superposition of two independent single-mode lasers even under conditions in which one photon is absorbed before the next is emitted by one or the other source. We propose here a modification of their experiment which would establish (if the interferences persist) the simultaneous existence of waves and particles (photons), a result which is assumed as basis by the stochastic interpretation of quantum mechanics (Garuccio, Vigier 1980, pag. 797).

Later they developed the idea, with a more accurate quantitative evaluation of the possible results, producing the remarkable paper published in 1981 by Garuccio, Popper and Vigier, “Possible direct physical detection of de Broglie waves”⁶⁶⁹. In the abstract they argued that:

A modified version of the Mandel-Pfleegor experiment implies conflicting measurable predictions of the Copenhagen and statistical interpretations of quantum mechanics. Their detection would help to choose between the antagonistic positions of Bohr and Einstein in the Bohr-Einstein controversy (Garuccio, Popper, and Vigier 1981).

The paper was basically the beginning of the researches on EW, still showing the interference between photons.

Concerning it, the authors of (Baracca, Bergia, Del Santo 2017, pag. 75) correctly state the impressive information that was Jean-Pierre Vigier

⁶⁶⁸ Garuccio A., Vigier J.P. (1980), “Possible experimental test of the causal stochastic interpretation of quantum mechanics: Physical reality of de Broglie waves”, *Foundations of Physics*, 10 (9-10), pp.797-801. (Garuccio, Vigier 1980) in the following.

⁶⁶⁹ Garuccio A., Popper K., Vigier J.P. (1981), “Possible direct physical detection of de Broglie waves”, *Physics Letters A*, 86 (8), pp. 397-400. (Garuccio, Popper, and Vigier 1981) in the following.

“who, around 1980, first brought Popper once again into the active debate on FQM within the community of the physicists”. And they also well claim that “Selleri established a direct relationship with Popper only in 1983, when he invited him to a conference in Bari, which was held in May of the same year” (Baracca, Bergia, Del Santo 2017, pag. 76). But they also write that:

The first documented participation of Popper to a research with some physicists dates back to 1980-81, and this led to the publication of a paper co-authored by Popper himself, Vigier and Garuccio. The latter, a collaborator of Selleri, participated to the project with Popper only because he was at that time working with Vigier in Paris, and he did not even meet the philosopher in that period” (Baracca, Bergia, Del Santo 2017, pag. 76. My underline).

Thanks to a long and deep interview made with Garuccio I could stated that the statement results in terms of my opinion partially incorrect. It is correct that Garuccio was in Paris to work with Vigier and that he did not meet Popper, but, as regards the rest of the statement, in which emerges almost an idea of a casual participation by Garuccio, only related to his presence, with maybe no contribution, Garuccio himself told a different story.

After analyzing his experience in France and the intense relationship with the French physicist, Garuccio deals with the genesis of (Garuccio, Popper, Vigier 1981), and practically revealed that the idea, and even some formal aspects, came from him:

Vigier had already come to Italy and there was now a very close relationship with Franco. And around Vigier there was a nice group of young researchers [...].

I do not remember how my choice to go to Paris matured. Perhaps at a time when Franco said that we had to diversify with respect to different types of research. We had no longer to work in groups but on different approaches.

With a student exchange organization they initially put me in a banlieue, which was of a frightening desolation. After a few days I asked to be moved to a more reasonable place. They found me a small hotel, a small dump, but close to the Poincaré Institute. And there I lived much better.

[...] The impact with Vigier was tremendous, because Vigier was a volcano! He oscillated between two opposite states. He talked about politics intensively; he talked about physics intensively. There was never any pause.

The only mental pause that I had, that I discovered, was to start talking about politics at a certain point. He would give up physics, start talking about politics and then, after a while, we would go back to physics and so on. Seven, eight hours of discussion of this kind, among other things with me who knew little, or rather nothing French. He told me to do this and that, he always filled me with material to study and review, on which the next day he would ask for enlightenment and explanations.

I started to study his proposals, but I remember that one night, desperate in this banlieue, I had arrived from 4-5 days, I was inspired by the experiment that then gave rise to the work with Popper and Vigier. The next day I arrived and did not let Jean-Pierre speak, but I said to him: “look, I had this idea, what do you think?” and I described it all to him. Jean-Pierre hit the ground running, saying that the idea was beautiful, and he immediately started to writing the article.

And so, after a few days we changed the relationship in a certain way, because it was me who now brought him the things to debate.

I came with the work done and the problems I had found. Vigier always managed to find a solution by manipulating mathematics to move forward. He had a very flexible approach to mathematics; I was much more rigorous.

He wrote the articles because he knew English and French very well. But I took care of some formal aspects. He usually handled other formal aspects. He still solved my problems.

I went so far as to say ‘look, it doesn’t work like that’ and he changed the conditions to make it work. And so from time to time we used to change our ideas about the setting.

But on this work with Popper, the idea was all mine. Vigier was enthusiastic about my idea. In those days he had to leave for London to meet Hiley and Bohm. And Popper too. And I am not sure how he persuaded Popper to sign the article.

On that occasion I did not know Popper. I was in Paris, and it was Vigier who was in London and acted as intermediary. Then he came and said: “see, Popper signed the article”. The article was published, it was a great success because it was the beginning of the researches on the subject. Maybe it was also wrong, the article. It had a somewhat naive approach. But it created a great sensation, it raised the issue of photon interference.

(Interview with Augusto Garuccio, Bari 27 May 2019. My underline)

The first paper on empty waves published in the Eighties by Selleri was “Can an Actual Existence be Granted to Quantum Waves?”⁶⁷⁰, written for Louis de Broglie’s 90th anniversary. It was a review paper, in which Selleri dealt with the issues submitted in his (Selleri 1969a) and (Selleri Varenna 1971) papers

⁶⁷⁰ Selleri F. (1982), “Can an Actual Existence be Granted to Quantum Waves?”, *Annales de la Fondation Louis de Broglie*, 7 (1), pp. 45-51. In FSPA there is a typewritten manuscript in which there are more pages. It goes from page 45 to page 71. (Selleri 1982a) in the following.

and with the neutron interferometry hypothesized in 1974 by Rauch, Treimer, and Bonse, as well as the contributions by Einstein, de Broglie, Bohr, Heisenberg and so on: all interpretation related to a wave without any associated energy-momentum. Then, he changed the perspective in the experiments, suggesting the use of photons instead of neutron. Thinking of the already introduced correspondence with de Broglie, it goes without saying that the French physicist's doubts could have produced an influence in Selleri, which at the beginning proposed neutrinos whereas de Broglie proposed the use of photons. In the abstract, he stated that:

Existence of quantum waves in space and time devoid of energy-momentum seems to be indicated by neutron interferometric experiments. A similar point of view has been discussed in famous papers by Einstein, de Broglie and Bohr. An experiment is proposed which could test this idea using a low intensity photon beam (Selleri 1982a, pag. 45).

A low intensity photon beam provides a generation of only one photon at a time in the apparatus. It could reach a certain region of a screen with a definite, measurable, probability density.

This is a way to reveal the presence of a wave normally deprived by energy and momentum. Selleri pointed out how an apparatus developed by Martinolli and Gozzini in Pisa⁶⁷¹ could permit to see the modification of the decay probabilities of an unstable system.

In the typewritten copy of Selleri's article, with a different page numbering (45-71 instead of 45-51), he described his idea of a laser gain tube (LGT, already proposed by Blake and Scarl in their 1979 paper⁶⁷² for stimulated emission), in order to show coincidences in the apparatus still revealing the EW and also studying their propagation in space and time:

⁶⁷¹ Selleri was grateful to Gozzini for private communication on the apparatus as well as for useful discussions. They experienced a correspondence on the opportunity to make the experiment on EW, without reaching in doing it, as I will show in their letter and in some Selleri's remarks.

⁶⁷² Blake G.D., Scarl D. (1979), "Two-photon correlations in amplified laser light", *Physical Review A*, 19, pp. 1948-1953. In the abstract they stated: "The authors have measured the two-photon correlation function in 632.8 nm single-mode He-Ne laser light which had been amplified by stimulated emission in a long He-Ne discharge tube. For laser light intensities which were comparable with the discharge-tube spontaneous-emission intensity into a single mode, the measured correlations agree well with those predicted for a simple mixture of laser and chaotic light. The results are consistent with the assumption that laser light linearly amplified by stimulated emission has the same statistical properties as unamplified laser light" (pag. 1948).

A very low intensity photon beam emitted by the source S is split by the beam splitter M in two orthogonal beams. On the reflected beam, a phototube P_1 is located and one concentrates attention on events in which P_1 does reveal a photon arrival. For such events no energy-momentum can propagate in the transmitted beam since no coincidences above the casual background would be revealed by a second phototube eventually put on this beam. However, the transmitted beam crosses an organic laser gain tube (LGT) where the eventual wave-packet devoid of energy and momentum has a chance to reveal its existence by generating a zero energy-transfer stimulated emission. The emitted photon could then be revealed by the phototube P_2 put after the LGT. In this way P_1P_2 coincidences would reveal the propagation of a zero-energy ondulatory phenomenon transmitted by M. The space-time propagation of this entity could be studied by checking that P_1P_2 coincidences disappear whenever an obstacle is put before LGT in the transmitted beam (Selleri 1982a, pagg. 70-71. My underline).

Selleri also mentioned the contribution by A. Szczepanski made in 1976⁶⁷³ for the reduction of wave packet in photon interactions, applying it for the verification of an active role of the empty waves.

Another paper to remember, after Selleri's further appearance as author of articles on the subject, is "New experimental set-up for the detection of de Broglie waves"⁶⁷⁴, published by Garuccio, Rapisarda, and Vigier. In the abstract they stated that:

We present a new experimental set-up to detect the existence of de Broglie waves. Since it uses only one, very weakened, incoherent source it escapes preceding objections and can test conflicting predictions of the Copenhagen (CIQM) and causal stochastic (SIQM) interpretation of quantum mechanics (Garuccio, Rapisarda, and Vigier 1982, pag. 17)

The main original idea in 1980 and 1981 was that of adding a device capable of revealing by which laser every photon that appears in the interference pattern was emitted. The new experimental set-up was proposed in order to answer to the "complex and heated discussion", (with de Mynch, Holey, Andrade e Silva, and Costa de Beauregard) which were provoked by the previous proposals made in 1980 and 1981:

⁶⁷³ Szczepanski A. (1976), "State vector reduction and photon coincidences", *Foundations of Physics*, 6, pp. 427-433. (Szczepanski 1976) in the following.

⁶⁷⁴ Garuccio A., Rapisarda V., Vigier J.P. (1982), "New experimental set-up for the detection of de Broglie waves", *Physics Letters A*, 90 (1-2), pp. 17-19. (Garuccio, Rapisarda, and Vigier 1982) in the following.

This [old] proposal, based upon a modified version of the Pfleeger-Mandel experiment, has provoked a complex and heated discussion. Besides its experimental difficulty (it is delicate to correlate the phases of two separate laser beams), its feasibility has been essentially contested on the basis of the laser nature of the source. In fact the existence of a Poisson distribution of photons in laser beams has been used to justify the point that no testable difference exists between the consequences of both interpretations [SIQM, and CIQM]. The aim of the present letter is to present an experimental programme and experimental set-up which clearly falls outside the field of the abovementioned objections to the preceding discussion and suppresses, as far as possible, experimental difficulties (Garuccio, Rapisarda, and Vigier 1982, pag. 17).

They used “a single very weakened pulsed source (i.e. a impulsed led), [which] produces, one by one, independent wave packets, which contains single photons at the same frequency” (Garuccio, Rapisarda, and Vigier 1982, pag. 19).

Basically, a very low-intensity *incoherent* source of monochromatic photons. Then the wave packets were split by a semi-transparent beam-splitter.

The authors followed the suggestion of the laser gain tube (LGT) made by Selleri in his (Selleri 1982a), which I introduced above.

Selleri proposed the use of an organic LGT which multiplies the photon number by two (on the average). Garuccio, Rapisarda, and Vigier recalled also that “gain tubes amplify externally introduced photons beams and preserve their phases with sharp temporal correlation” (Garuccio, Rapisarda, and Vigier 1982, pag. 18) and that these amplifiers were already used by Martinolli and Gozzini in Pisa.

I introduced this information because Selleri tried doing the EW experiment collaborating with Gozzini in Pisa. I will show later a correspondence between them.

Finally, with their experimental set-up, Garuccio, Rapisarda, and Vigier wanted to show how, according to CIQM, no interferences should be observed, whereas SIQM predict a detection of interferences, still find “a crucial contradiction between CIQM and reality” (Garuccio, Rapisarda, and Vigier 1982, pag. 19).

In a (undated) note of presentation of Garuccio's work and contributions written by Selleri, the latter states that in the abovementioned work (Garuccio, Rapisarda and Vigier 1982) there is:

a substantial modification of the experiment, where, while maintaining the basic idea of verifying the double corpuscular-ondulatory nature of photons by means of coincidence techniques, the two lasers of the previous versions are replaced with an incoherent light source and a laser amplifier. De Broglie's theory (unlike quantum mechanics) obviously contains no postulate about wave packet collapse. Admitting or not such a postulate leads to conflicting predictions in the proposed experimental scheme and therefore an empirical discrimination between the two theories seems possible⁶⁷⁵ (FSPA).

Always according to Selleri, the above three papers have sparked in perspective and in the subsequent years:

a wide debate and lively interest not only among theoretical scholars of the foundations of quantum mechanics, but also among the experimentalists of quantum optics. This interest focused on three points: the quantum mechanics description of a photon amplifier, the possible result of the proposed experiment and its interpretation in the light of quantum electrodynamics, the behaviour of photons on a semi-reflective mirror⁶⁷⁶ (FSPA).

In the same year, in November, Selleri published "On the direct observability of quantum waves"⁶⁷⁷, a review paper often recalled by Selleri himself in his following papers as well as mentioned by several scholars.

In the abstract he introduced quite the same issues of the unofficial typewritten paper (Selleri 1982a), which was officially published with only 7 pages instead of the 27 pages I found in FSPA:

Fundamental experiments on the dual nature of atomic entities (photons, electrons, neutrons etc) can be interpreted in terms of "empty" waves not carrying energy and momentum.

⁶⁷⁵ The Italian original text is in the Addendum 3, p. 1021.

⁶⁷⁶ The Italian original text is in the Addendum 3, p. 1021.

⁶⁷⁷ Selleri F. (1982), "On the direct observability of quantum waves", *Foundations of Physics*, 12, pp.1087-1112. (Selleri 1982b) in the following.

Recent proposals could lead to experimental tests of this idea, using low intensity photon beams, thanks to modern experimental apparatuses (Selleri 1982b, pag. 1087).

Selleri explained the difficulties led to a wave without energy-momentum, as already stated in the previous papers as well as in my quotations at the beginning of this section and in section 2.3:

A problem coming immediately to mind within Einstein's philosophy is the following: if the localized particle carries all the energy and momentum, in what sense can the wave be considered real? This problem was felt so acutely by Einstein that he referred to these waves as *Gespensterfelder* (ghost fields): an object without energy and momentum is in fact unable to exert a pressure when hitting other bodies, which means that it does not have that quality which makes us call real a normal object. Still the equation of QM describe this wave as propagating in space and time.

The difficulties associated with the conception of an "empty wave" (here and in the following these two words are taken as being equivalent to "wave propagating in space and time but devoid of energy-momentum") have led many people to discard the idea as a scientific impossibility. If the wave is empty, it cannot induces changes into physical objects; hence it cannot be observed; therefore it does not make any sense to postulate its existence. This was probably one of the reasons for the rejection of Einstein's dualism by the large majority of the physicists (Selleri 1982b, pagg. 1089-1090).

Selleri recognized that the main problem lied in the empirical meaning of an entity which cannot carry energy and momentum.

His answer lay in the fact that "it is not inconceivable that its solution could be possible using modern technological devices in the study of low intensity photon beams" (Selleri 1982b, pag. 1090).

Indeed:

The problem of the empirical meaning of the idea that empty waves exist in space and time independently of our observations was discussed in two papers [(Selleri 1969a and (Selleri Varenna 1971)], in which the general solution was indicated in the following way: the wave could reveal that it is real, independently of the localized energy and momentum which sometimes is associated with it, if it gives rise to changes in the transition probabilities of the systems with which it interacts (Selleri 1982b, pag. 1105).

In 1969 Selleri proposed a beam of neutrinos. In 1971, after his correspondence with de Broglie, as I have already showed, he turned to a source of emitting photons. And he recalled the 1974 workshop at Frascati, which unfortunately did not generate any proceedings at all. His lecture there was now submitted as “the first proposal to study stimulated emissions due to a wave which for certain has no associated energy-momentum considered the double-slit experiment” (Selleri 1982b, pag. 1106).

As a confirmation of de Broglie’s influence in Selleri’s use of photons instead of neutrinos, Selleri emphasized a peculiar acknowledgment to Louis de Broglie, being “deeply grateful for early correspondence on the nature of the wave-particle dualism” (Selleri 1982b, pag. 1110).

In a paper written with Andrade e Silva and Vigier in 1983, “Some Possible Experiments on Quantum Waves”⁶⁷⁸, Selleri recalled three experiments “which can be performed with modern techniques and which allow one to probe directly the actual existence of quantum waves in space and time” (Andrade e Silva, Selleri and Vigier 1983, pag. 503), namely, (Szczepanski 1976), (Garuccio, Rapisarda, and Vigier 1982), and a 1980 paper published by Andrade e Silva J. and M., “A possible experiment concerning the nature of the wave-particle duality”⁶⁷⁹. The third one was never mentioned by Selleri until now. This experiment was based on the lowering of fringe-visibility parameter due to presence of empty wave.

The two Andrade e Silva stated that “The fundamental idea of this experience is to measure in an interference detecting device the weakening of fringes produced from [two] beams, this weakening being a consequence of the simultaneous arrival of an empty wave having [a different] intensity” (Andrade e Silva, Selleri and Vigier 1983, pag. 507).

⁶⁷⁸ Andrade e Silva J., Selleri F., Vigier J.P. (1983), “Some Possible Experiments on Quantum Waves”, *Lettere al Nuovo Cimento*, 36, pp. 503-508. (Andrade e Silva, Selleri and Vigier 1983) in the following.

⁶⁷⁹ Andrade e Silva J., Andrade e Silva M. (1980), “Une experience possible concernant la nature du dualisme onde-corpuscule-A possible experiment concerning the nature of the wave-particle duality”, *Comptes rendus hebdomadaires des séances de l'Académie des Sciences B*, 290 (23), pp. 501-503. (Andrade e Silva 1980) in the following.

The set-up could by-passes the difficulties related to the building of “detectors able to reveal an optical photon without absorbing it” (Andrade e Silva, Selleri and Vigier 1983, pag. 508).

Later the same Selleri, still confirming the attractiveness of the idea, however declared in a note found in FSPA that the experiment could be probably impossible because a single photon should be detected three times!

Anyway, Selleri and the other scholars did a severe, strong activity in order to propose an experiment to reveal empty waves, still going through paths without a future.

In the third phase, as told before, the activity was related to photon amplifiers, the amplification of incoherent light, to stimulated emission, and to quantum-optical predictions.

The 1987 was a peculiar year in the current issue. Selleri published 5 papers, still analyzing the amplification processes using photon amplifiers, always in order to do experiments for the direct detecting of EW.

The papers were: “Calculation of Amplification Processes Generated by Empty Quantum Waves”; “Amplification of Empty Quantum Waves”; “On the Physics of Quantum Waves”; “Amplification of Light from an Undulatory Point of View”; and “Coherence Properties of Photon Amplifiers”⁶⁸⁰.

I am going to introduce only the latter two papers, referred to as (Selleri 1987a) and (Selleri 1987b), respectively. The last one received some endorsement by Karl Popper too, as I will show later.

⁶⁸⁰ Selleri F. (1987), “Calculation of Amplification Processes Generated by Empty Quantum Waves”, in *Quantum Uncertainties* (a cura di W.M. Honig et al.), New York: Plenum, pp. 41-50.

Selleri F. (1987), “Amplification of Empty Quantum Waves”, in *Quantum Violations: Recent and Future Experiments & interpretations*, June 23-27, 1986, Bridgeport Honig W.M. et al.(eds.), New York: Plenum, pp. 41-50.

Selleri F. (1987), “On the Physics of Quantum Waves”, *Transactions of Gdansk Scientific Society*, 14, pp. 160-163.

Selleri F. (1987), “Amplification of Light from an Undulatory Point of View”, *Physics Letters A*, 120 (8), pp. 371-376. (Selleri 1987a) in the following.

Selleri F. (1987), “Coherence Properties of Photon Amplifiers”, *Foundations of Physics*, 17 (8), pp.739-757, (Selleri 1987b) in the following.

In “Amplification of Light from an Undulatory Point of View”, he claimed that:

The traditional approach to light amplification is shown to be based on the idea that photons of the incoming beam act as independent particles. If atomic stimulation is instead attributed to the action of the wave, different photon statistics (but with the same average) is obtained in the final state (Selleri 1987a, pag. 371).

He illustrated how empty waves could stimulate photons emissions and how the photon distribution could be different respect to the usual distribution related to stimulated emission, making it feasible to identify the origin, i.e., the EW themselves:

The basic property of empty waves is assumed to be their ability to stimulate emissions when interacting with excited atoms. The waves are empty and thus do not contribute to the energy-momentum balance which can of course be saved by keeping into account only the atomic variables. When the wave generates instead atomic excitation, the absorbed energy is always that of an energetic quantum present in the wave. In the stimulated emission the quantum can be totally absent from the wave, or perhaps present but not near the stimulated atom. It is in all cases assumed to be irrelevant to the stimulation process.

In published papers [Selleri 1969a, Selleri 1982b, and Selleri 1984] it has been pointed out that such a model with empty waves is perfectly “physical” since it is in principle capable of falsifiable predictions.

This conclusion is demonstrated to be correct in the present letter, since it is shown that physical phenomena exist in which the predicted photon distribution is different from the one deduced from the usual approach to stimulated emission.

[...] It was shown [Selleri 1969a, Selleri 1982b, and Selleri 1984] that an experiment is possible in which an empty wave (deprived of its energetic quantum by the crossing of a beamsplitter) is revealed by detecting atomic emissions stimulated by its action. Other proposals for the detection of the empty waves have been presented and discussed by several authors. These proposals were centered on the idea that *coincidence detections* should be made of primary photons which were born together with the empty wave, and of photons whose emission was stimulated by the empty wave itself (Selleri 1987a, pag. 371).

Then he explained the goal of his work, in which, using a light crossing amplifier, it emerges a different photon statistics whether the underlying cause is the empty wave or the quantum particle:

In the present paper it will be pointed out that even simpler experiments are possible since the photon statistics of light which has crossed an amplifier is different if amplification is generated by the wave, rather than by the energy quanta. It will also be shown that the attribution of amplifications to the energy content of the e.m. field is implicit in the usual treatment of these phenomena (Selleri 1987a, pagg. 371-372).

The conclusions did not solve the problem, however, leaving future evidences, relate to the measure of the photon statistics in pulses of amplified laser light, given the average number of photons per pulse in the incoming beam:

[...] Particle-generated stimulation and wave-generated stimulation give rise to the same *average* photon number and cannot be discriminated with experiments measuring only the (average) energy of the outgoing e.m. field. This is the same as saying that present experimental evidence cannot rule out our proposal. [...] But one can say more: the experiment carried out by Blake and Scarl on two-photon correlations has shown that no time correlations exist in a beam of amplified laser light, a result strongly indicative of the validity of the poissonian distribution which was deduced here from the idea of wave-stimulation.

Anyway, the present proposal can easily be checked by measuring the photon statistics in pulses of amplified laser light, given the average number of photons per pulse in the incoming beam. Considerably different results for such an experiment [are predicted by calculations] (Selleri 1987a, pag. 3745).

The second paper I decided to introduce here is “Coherence Properties of Photon Amplifiers”. Strikingly, the beginning of the abstract is completely identical to (Selleri 1987a):

The traditional approach to light amplification is shown to be based on the idea that photons of the incoming beam act as independent particles. If atomic stimulation is instead attributed to the action of the wave, different photon statistics (but with the same average) is obtained in the final state (Selleri 1987b, pag. 739).

Then, in the following, the new development, including calculations for the expected rate of coincidences in the case of an empty wave, still arises in the abstract:

The new distribution turns out to be Poissonian, in agreement with some experimental evidence. The expected rate of coincidences for an empty-wave amplification experiment is calculated and found up to 30% higher than obtained from the traditional approach. (Selleri 1987b, pag. 739).

Better explaining in the introduction, Selleri is debating on the idea that:

[...] the essential new feature of the quantum waves, which should be introduced in order to make them concretely active in their interactions with matter, is the property of stimulating emissions from excited systems.

This idea seems amenable to experimental verification in the case of light, where excited atoms could emit an Energy quantum $h\nu$ more easily if stimulated by waves with the right frequency. The physics of laser amplifiers will therefore be discussed in some detail and it will be shown:

1. That the usual “textbook” treatment of photon amplification in lasers and in laser amplifiers is essentially based on a corpuscular approach, owing to the neglect of all interference terms.
2. That the photon distribution obtained with the previous approach is non-Poissonian in the case of laser light amplified through crossing of a laser amplifier.
3. That the Blake-Scarl experiment constitutes instead an important piece of evidence in favour of a Poissonian distribution of photons in amplified laser light.
4. That a simple treatment of amplification in terms of a wave stimulating the atomic emissions leads necessarily to a Poissonian distribution.

As a consequence of those results the proposal of [Selleri 1969 and Selleri 1982b] can be put to a stringent test. An explicit calculation shows that about 30% more coincidences are to be expected if the true mechanism of stimulated emission is due to the action of a wave, in comparison with the usual (corpuscular) treatment (Selleri 1987b, pagg. 739-740).

So, light and laser amplifiers could be another path to follow. And the great percentage difference in observable coincidences could finally detect EW.

An important step in the direction of the concrete observability of empty waves was given by the laser amplifier (a laser without initial and final mirrors, replaced by transparent walls and with excited atoms).

The basic idea was that when a coherent or incoherent beam of light enters an amplifier the modified probability of photon emission by an excited atom (stimulated emission) is due to the action of the empty wave and not to the presence of particles in the incident radiation.

In the paper's conclusions, Selleri pointed out how the photon distribution in the amplified laser is Poissonian, thus attaching it to a wave:

A very simple explanation of the Blake-Scarl experiment has been obtained: the photon distribution in the amplified laser light must be Poissonian because the proposed mechanism for stimulated emission gives rise to totally uncorrelated photons. The stimulation of atomic emissions has been described as due to the action of a wave accompanying the energetic and localized quanta in their propagation, according to the general point of view of wave-particle duality proposed by Einstein and de Broglie. The whole physics of lasers is considerably enlightened if the present point of view is accepted. Laser light shows a Poissonian distribution of photons not because of complicated randomizing mechanisms due to bouncing back and forth millions of times inside the laser, but *because the elementary mechanism of photon production from the atoms has this property* (Selleri 1987b, pag. 756. *Italics in the original text*).

Even if the results do not allow to discriminate between the two approaches to photon amplification, a possible solution lies in photon and frequency counting experiment:

The experimental evidence about the ratio of light intensities before and after amplification cannot distinguish the present proposal from the usual approach to photon amplification, which attributes stimulated emissions to a corpuscular mechanism. However, a photon counting experiment, given the average photon number in the incoming pulses and counting the frequency with which fixed photon numbers are produced in the final state, could easily discriminate between the two approaches. Photon amplification beyond a beam splitter has been shown to lead to predictions different from those of the traditional approach and immediately detectable with the coincidence method if counters are placed on both photon paths coming from the beamsplitter (Selleri 1987b, pag. 756).

In the following year, 1988, Croca, Garuccio and Selleri published “On a possible way to detect de Broglie’s Waves”⁶⁸¹, in which they submitted another experimental setup based on the neutron interferometry, in order to reveal the existence of new effects in the superposition of incoherent waves which occur right in the interferometer. In their conclusion they dealt with an argument which was normally advanced against the EW detection, namely the huge number of EW simultaneously existing in the device, making it unable to detect the interference. They answered to this claim confirming their belief and admitting that the superpositions are rare:

If empty waves exist, it is objected, there should be many of them in the apparatus and the very observation of self-interference should be impossible. The answer is that absorbers of neutron particles also shield very efficiently from empty waves.

In fact, in the realistic interpretation of the double-slit experiment quantum waves must be assumed to arise only from the slits and not from other parts of the screen in which the slits are pierced. A constant care of the experimentalists is to protect their apparatus from unwanted particles, which could generate a sizable “noise”. The same protection must then work against empty waves! [...] [The sources] do not generate infinitely long wave trains, but wave packets of very short longitudinal coherence length, as a necessary consequence of the way a “monochromatic” neutron beam is prepared. Therefore only a subset of events selected with great care [...] can arise from superpositions of different wave packets: such superpositions are in general very rare (Croca, Garuccio, Selleri 1988, pagg. 110-111. My underline)

In the same year, “Amplification of Incoherent Light Behind a Beamsplitter”⁶⁸² was also released by Garuccio, Guéret, and Selleri, introducing the undulatory model of atomic stimulated emission. They pointed out how “coincidence experiments can easily observe the difference between the two approaches if an amplifier is set on one of the two beams emerging from a beamsplitter” (Garuccio, Guéret, Selleri 1988, pag. 139).

⁶⁸¹ Croca J.R., Garuccio A., Selleri F. (1988), “On a possible way to detect de Broglie’s waves”, *Foundations of Physics Letters*, 1 (2), pp. 101-112. (Croca, Garuccio, Selleri 1988) in the following.

⁶⁸² Garuccio A., Guéret P., Selleri F. (1988), “Amplification of Incoherent Light Behind a Beamsplitter”, *Foundations of Physics Letters*, 1 (2), pp. 139-145. (Garuccio, Guéret, Selleri 1988) in the following.

They emphasized how their undulatory model could give the same outcomes and how, using incoherent light, a new framework may occur:

Our undulatory model of atomic stimulated emission gives results very similar to those of the textbook approach in the limiting situations of high amplification and high photon number [...].

[...] We expect, however, new phenomena to show up for low photon number and/or for low amplification whenever a beam of coherent or incoherent light crosses an amplifier ("laser gain tube").

Already in [Selleri 1987b] photon amplification behind a beamsplitter has been shown to lead to predictions different from those of the textbook approach in the case of coherent light pulses, if the undulatory approach to atomic stimulation is adopted. The case of *incoherent* light is investigated in the present letter (Garuccio, Guéret, Selleri 1988, pag. 140).

In 1988, Münchenheim, Lokai and Burghardt, in "Empty Waves do not induce stimulated emission in laser media"⁶⁸³, questioned the method for detecting empty waves proposed by Selleri in his (Selleri 1987b).

The three authors performed an experiment and stated in the abstract that:

Due to dispersion in a 20 m long quartz fiber an empty light wave (moving with phase velocity) arrives 1.3 ns earlier than the photons. Using a short dye laser pulse the effect of the separated empty wave on a laser medium has been investigated with negative results (Münchenheim, Lokai and Burghardt 1988, pag. 387).

The above outcome would make the proposals made in (Selleri 1987b) of little practical interest. Selleri replied quite soon, "Reply to 'Empty Waves do not induce stimulated emission in laser media'"⁶⁸⁴, stating that:

The argument of Münchenheim, Lokai and Burghardt (photonic empty waves do not induce stimulated emission in laser media) is shown to be totally unfounded because when a (photonic) wave packet, which according to our view is a physical reality, is written as a superposition of monochromatic components, such components do not have an actual existence outside the space-time extension of the wave packet itself (Selleri reply 1988, pag. 72).

⁶⁸³ Münchenheim W., Lokai P., Burghardt B. (1988), "Empty Waves do not induce stimulated emission in laser media", *Physics Letters A*, 127 (8-9), pp. 387-390. (Münchenheim, Lokai and Burghardt 1988) in the following.

⁶⁸⁴ Selleri F. (1988), "Reply to 'Empty Waves do not induce stimulated emission in laser media'", *Physics Letters A*, 132 (2-3), pp. 72-74. (Selleri reply 1988) in the following.

Then Selleri deeply described the experiment proposed by the three authors (further referred to as MLB), who, starting by the correct consideration of the Blake-Scarl experiment as a positive evidence in favour of empty-wave stimulation effects,

[...] proposed to check on the real existence of such phenomena by separating in a refractive medium a part of the empty wave from the photons. In the MLB experiment a dye laser produced pulses of 16 ns duration at 589 nm [...]. A substantial part of each pulse was directed onto the entrance of a 20 m long hard-clad silica fiber and a 0.1 mJ pulse energy arrived at the end of the fiber, corresponding to about 3×10^{14} energy quanta $h\nu$ and therefore, in the Einstein-de Broglie picture, to an equal number of photonic particles.

Inside the fiber the photon group velocity was about 1% smaller than the phase velocity. On this basis, MLB expected the empty wave to go faster than two photonic wave packet and to start leaving the 20 m long fiber a time $\Delta t = 1.30 \pm 0.02$ ns *before* the photon pulse. After leaving the fiber the pulse (preceded by the empty wave, in MLB's idea) was used as a seed pulse for a dye laser amplifier, i.e. a 20 mm long dye cell pumped 10 ns before the energetic pulse arrived. If the precursory empty wave induced stimulated emissions in the 20 mm long cell, one would expect an output pulse 1.3 ns larger than the seed pulse. The observed enlargement was however 0.10 ± 0.02 ns, and MLB concluded that "empty waves do not induce stimulated emission in laser media" (Selleri reply 1988, pag. 73. *Italics in the original text*).

Selleri then introduced his argument against the validity of MLB's conclusions, which can be presented in two equivalent forms.

The first one is simply to say that if the overall wave surrounding the 10^{14} particles is Fourier developed as a superposition of monochromatic waves, these components do not have any physical reality, but are only useful mathematics tools. Therefore MLB's attempt to detect a wave *outside the wave packet* was bound to fail! Since also in water phase velocity is larger than group velocity, a reasoning similar to that of MLB would lead to the conclusion that monochromatic surface waves are real, before the arrival of the wave packet, in a motionless part of the wave surface, which is obviously nonsense (Selleri reply 1988, pag. 73. *Italics in the original text. My underline*).

For the second form, Selleri showed how, aware that for photon emission from an excited atoms:

[...] *the choice between stimulation and inhibition depends strongly upon the relative phase of the atomic emission and of the photonic wave running over the atom.*

[...] Therefore, plane waves with different frequency entering in the Fourier development of the wave packet are completely *out of phase* outside the wave packet itself. As a consequence, even if one started from MLB's idea that a Fourier component with frequency ν can stimulate photonic emission outside the physically real wave packet, one should also consider that another wave with nearby frequency ν' relevant to the same Fourier development, having a very different phase in the same point, should simultaneously have an inhibiting effect (and *vice versa*). Summing over all frequencies the net stimulation/inhibition must obviously be zero.

Another way of saying the same thing is to stress that the well known phase-locking mechanism, responsible for a positive net stimulation effect when an electromagnetic wave packet with the right average frequency runs over an excited atom, becomes impossible with two or more frequencies out of phase with one another (Selleri reply 1988, pag. 74. *Italics in the original text*).

He finally arrived to evince that the MLB-type experiments cannot be used to prove or disprove the existence of EW, while the experiments proposed in Andrade e Silva 1980, Selleri 1982b, and Garuccio, Rapisarda, Vigier 1982 “could instead provide vital information for the solution of the same problem”(Selleri reply 1988, pag. 74).

Others attempt at detecting EW saw the new idea of a triple-slit experiments, related to the neutron interferometry, and the debate on quantum optical predictions

Selleri experienced a two months stay in Australia, during February-April 1990, in order to “collaborate with Dr. Kumar on the foundations of quantum mechanics and in particular to give a course of lectures on ‘Quantum paradoxes and reality’ ”⁶⁸⁵. During that time, he managed to publish a paper on the journal *Australian Physicist*, “Is a direct experimental detection of quantum waves possible?”⁶⁸⁶ which I reported here for an explanation of the Blake-Scarl experiment and its consequences, so basic for detecting EW in Selleri's approach.

⁶⁸⁵ Excerpt from a letter sent to Selleri by B.A. Robson, Head, Department of Theoretical Physics, The Australian National University at Canberra, 20 Sempember 1989 (FSPA).

⁶⁸⁶ Selleri F. (1990), “Is a Direct Experimental Detection of Quantum Waves Possible?”, *The Australian Physicist*, 27 (10), pp. 214-219. (Selleri Australia 1990) in the following.

Blake and Scarl have investigated laser light amplification in a laser gain tube. The gain, defined as the ratio of the intensity of the laser beam, with the discharge on, divided by the intensity with the discharge off (after the amplifier-tube spontaneous emission had been subtracted) was 1.30 ± 0.02 ns. At such low amplifications the gain is known to be linear for the intensity levels used in this experiment. In these circumstances one might expect the stimulating photon and the stimulated photon to be related in time, **if the process of stimulated emission is generated by the energy-momentum of the electromagnetic field**, since these physical entities are always detected as localised.

A beamsplitter and two photomultipliers serve as the two-photon detection system. Measuring the **coincidence rate** (number per unit time of photon detectors by the two photomultipliers having a time difference less than 1 ns), Blake and Scarl established instead that, after background subtraction, the coincidence probability was the same **for all intensities of the incoming laser light** (Selleri Australia 1990, pag. 216. **Bold** in the original text).

Selleri stressed how the explanation of Blake-Scarl experiment could be really natural thinking to empty waves:

A natural explanation of Blake and Scarl's discovery can be given by supposing that the stimulated emission is due to the action of the quantum wave. For laser light the wave has a very long coherence length and a constant probability of generating a stimulated photon, which is produced independently of the presence and position of photons in the stimulating wave. Consequently, no time correlation is expected between primary and secondary photons, as observed (Selleri Australia 1990, pag. 216).

In the paper there is also a long specific reference to the triple-slit experiment, a new attempt proposed by Croca in 1987⁶⁸⁷ in order to detect EW. Croca, in his abstract, introduced an experimental apparatus based on neutron interferometry "which can in principle decide the question whether the empty waves of de Broglie really exist or not" (Croca 1987, pag. 971). Anyway, I referred to other Selleri's paper in order to introduce the issue of triple-slit, only because there is a more developed analysis .

⁶⁸⁷ Croca J.R. (1987), "Neutron interferometry can prove (or refute) the existence of de Broglie's waves", *Foundations of Physics*, 17, pp. 971-980. (Croca 1987) in the following.

For instance, I considered the 1991 joint paper written by Selleri and Schmidt, “Empty-Wave Effects on Particle Trajectories in Triple-Slit Experiments”⁶⁸⁸, in which the authors stated:

A numerical calculation of particle trajectories for a recently proposed triple-slit experiment has been performed by starting from the Einstein-de Broglie picture of wave-particle duality.

The non-relativistic quantum potential due to the sum of three quantum waves superimposing in space and time gives rise to an interference-like distribution of particle arrivals on a second screen that is not predicted to exist in the usual (“Copenhagen”) interpretation of quantum theory (Schmidt, Selleri 1991, pag. 1)

The outcome is related to the two empty waves emerging from the other two slits. Then the authors introduce Croca’s proposal:

[...] Croca recently proposed that a neutronic particle can also be able to interact with neutronic (empty) waves arising from a different source.

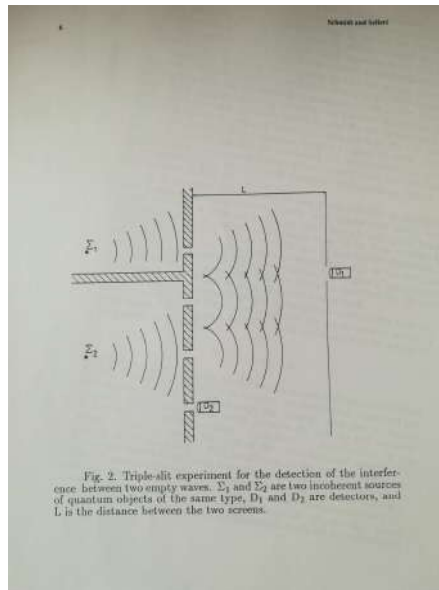
[...] Croca’s proposal is obviously of a tremendous conceptual importance, even though there are still some minor difficulties to be overcome that do not yet allow the experiment to be performed.

In spite [of the difficulties] a Croca-type triple slit experiment remains possible (Schmidt, Selleri 1991, pag. 5)

Then, they describe (Schmidt, Selleri 1991, pagg. 5-17), the triple-slit experiment, also debating on the velocity and the trajectory of the particle.

I am going to insert some citation together with my summary explanations. The triple-slit experiment has two incoherent sources Σ_1 and Σ_2 , with very low intensity emission, which emit quantum objects, one at a time and at a widely separated time intervals, with same nature and same energy, so the planned empty waves could have the same frequency. I am submitting here below a picture taken from Schmidt-Selleri’s paper.

⁶⁸⁸ Schmidt M., Selleri F. (1991), “Empty-Wave Effects on Particle Trajectories in Triple-Slit Experiments”, *Foundations of Physics Letters*, 4 (1), pp. 1-17. (Schmidt, Selleri 1991) in the following.



(Schmidt, Selleri 1991, Fig. 2, pag.6)

As they stated:

The emission from the upper source Σ_1 can hit the upper slit only. [...] only cases in which this particle is observed by D_1 will interest us. Emissions from the lower source Σ_2 can hit on the three lower slits; one of them is, however, constantly closed by D_2 and the slits that can be crossed by quantum waves are only three [in all]. Only cases in which D_2 does detect a particle interest us, and in such cases two empty quantum waves [...] can be assumed to be emitted from the second and third slit (Schmidt, Selleri 1991, pag.7).

[...] It is assumed that l as well as the group velocity u are known from previous measurements. Under such conditions every wave packet will take a time $\Delta t = l/u$ to pass through any given point. Precisely this Δt defines the coincidence time. If D_2 fires at time t_2 , then D_1 must fire at time $L/u + \Delta t/2$ later, if the double detection is to be counted as a true coincidence from electronics (L is the distance between the two screens).

This definition of “coincidence” guarantees that the waves accompanying the two particles detected by D_1 and D_2 overlap in the region in front of the second screen (Schmidt, Selleri 1991, pag. 7).

[...] the particle density on the second screen becomes proportional to the sum of three squared moduli plus an interference term. The predicted oscillations, if actually present, would constitute a truly extraordinary phenomenon, since they arise from waves that in the Copenhagen interpretation should both be set equal to zero. The difference of the two predictions is thus really striking! (Schmidt, Selleri 1991, pag. 8).

[Indeed] in Copenhagen interpretation particle detection in D_2 leads to immediate disappearance of the waves emerging from the second and the third slit, which are not physical waves but only mathematical tools for the calculation of probabilities (Schmidt, Selleri 1991, pag. 7).

After having found a difference in predictions, still to be confirmed by a likely real experiment, the authors also wrote a computer program in order to calculate particle trajectories in single, double, and triple-slit experiments. They calculated the trajectories in the case of the upper slit open only (Fig. 4a) and the three slits open (Fig. 4b), still showing a diagram in which, according to them, the effect of empty waves on the particle trajectories is evident.

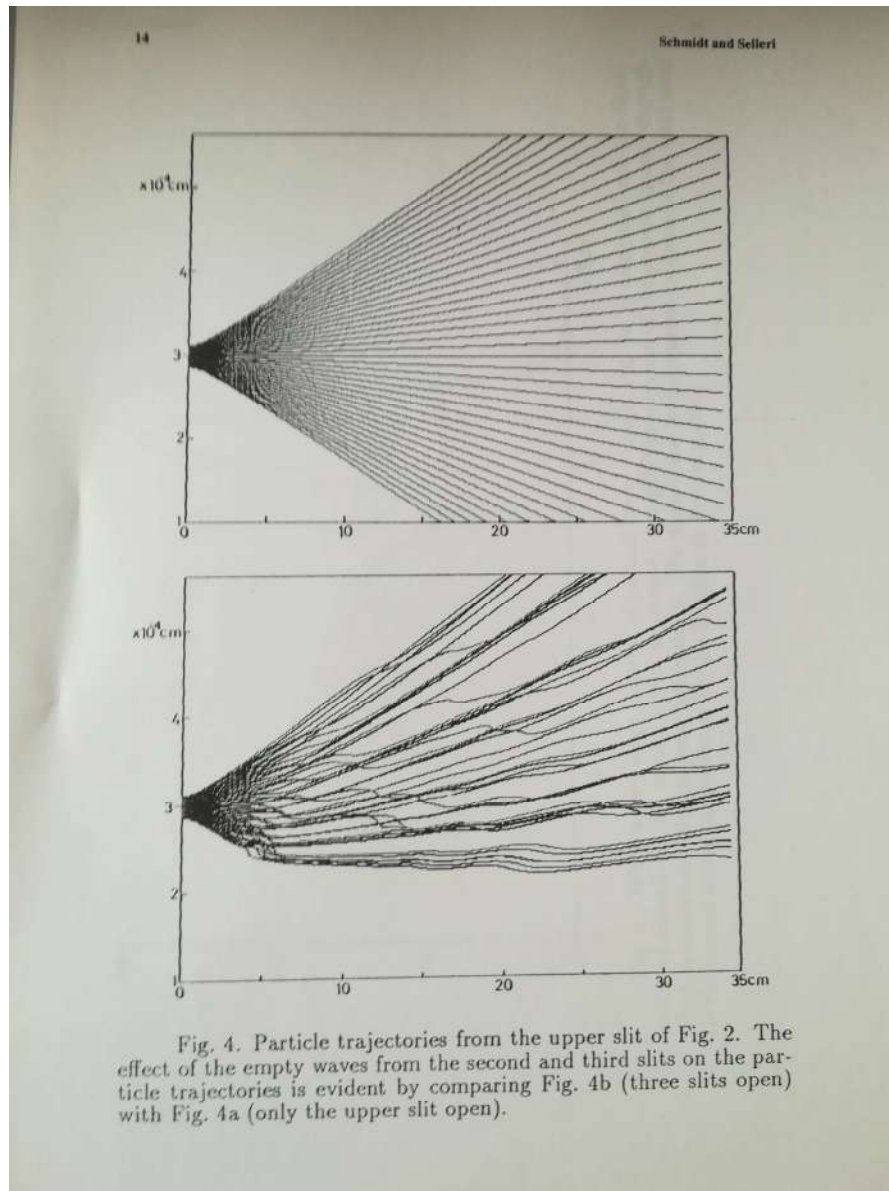
In figure 4a (at the top on the following picture), “the spreading of the Gaussian wave packet gives rise to rectilinear trajectories that increase their relative distance while propagating toward the second screen” (Schmidt, Selleri 1991, pag. 13); whereas in figure 4b (at the bottom on the following picture) it appears “a general bending away from the region they would otherwise have occupied. This is simply an effect of the presence of the empty waves Φ_2 and Φ_3 independent of their phase difference with Φ_1 . It is a new property of the quantum potential that is difficult to understand mathematically, but that is undoubtedly present” (Schmidt, Selleri 1991, pag. 13).

They also did a statistics of the 8000 arrivals on the second screen of figure 4b, seeing “maxima and minima, which hint at the presence of an interference term” (Schmidt, Selleri 1991, pag. 13).

In conclusions, the authors claimed that:

The striking difference between the two lines shows that the proposed experiment can easily give results that can discriminate between the two basic interpretations of quantum phenomena (Schmidt, Selleri 1991, pag. 17).

Here below are reported the two pictures just introduced:



(Schmidt, Selleri 1991, pag. 14)

The authors managed to do a good work, really practical, in order to show that this experiment could finally detect empty waves. The problem was always to find some laboratory which basically set-up the experiment itself. So, even for empty waves, as we shall see with the kaons, Selleri goes so far as to assume the execution of a possible *experimentum crucis*. He also brought his insights in this respect to the next phase of EW research, writing papers and having a correspondence with the main characters involved in the issue.

In the fourth phase, thanks to quantum optics and PDC, finally a real experiment was set⁶⁸⁹.

In the three years going from 1990 to 1992, even five papers⁶⁹⁰ were submitted, in the pages of *Foundations of Physics Letters* for the first paper; and in the pages of *Physical Review Letters* for the other four ones. The last three were even published in the same issue of *Physical Review Letters*. It was a really interesting debate, with two groups which discussed on the opportunity to detect EW.

In the first paper, “Quantum-optical predictions for an experiment on the de Broglie waves detection”, Croca, Garuccio, Lepore, and Moreira, explained an experimental apparatus to detect de Broglie waves, useful to disprove either the two theories, that is, the quantum optical approach and the de Broglie one:

The wave packets of two photons generated in the parametric-down conversion are overlapped in a modified Mach-Zehnder interferometer. The coincidence photodetection rate of photon pairs is evaluated, as a function of path-length of two interferometer arms, both by using the de Broglie concept of a real quantum wave and by the quantum optical approach. The different results of these two theories are compared, and it is shown that the proposed experiment can disprove either the theories (Croca, Garuccio, Lepore, Moreira 1990, pag. 557).

⁶⁸⁹ We have seen how in Bell-type experiments, the phase characterized by parametric-down conversion (PDC) still allowed to perform impressive experiments.

⁶⁹⁰ The five papers are chronologically reported here:

- Croca J.R., Garuccio A., Lepore V.L., Moreira V.N. (1990), “Quantum-optical predictions for an experiment on the de Broglie waves detection”, *Foundations of Physics Letters*, 3 (6), pp. 557-564. (Croca, Garuccio, Lepore, Moreira 1990) in the following.

- Wang L.J., Zou X.Y., Mandel L. (1991), “Experimental test of the de Broglie guided-wave theory for photons”, *Physical Review Letters*, 66 (9), pp. 1111-1114. (Wang, Zou, Mandel 1991) in the following.

- Zou X.Y., Grayson T., Wang L.J., Mandel L. (1992), “Can an ‘empty’ de Broglie pilot wave induce coherence?”, *Physical Review Letters*, 68, pp. 3667-3669. (Zou, Grayson, Wang, Mandel 1992a) in the following.

- Croca J.R., Garuccio A., Lepore V.L., Moreira V.N. (1992), “Comment on ‘Experimental test of the de Broglie guided-wave theory for photons’”, *Physical Review Letters*, 68, p. 3813. (Croca, Garuccio, Lepore, Moreira 1992) in the following.

- Zou X.Y., Grayson T., Wang L.J., Mandel L. (1992), “Zou et al. reply”, *Physical Review Letters*, 68, p. 3814. (Zou, Grayson, Wang, Mandel 1992b) in the following.

Then, they pointed out how the debate has been done on theoretical grounds, and definitely not be settled in a conclusive way. The experimental setup proposed by the foregoing (Croca, Garuccio, Selleri 1988) and based on neutron interferometry, is debated by the authors, which stated how:

Unfortunately, sources of neutron pairs are not available and, moreover, there is no experimental evidence of any interference effect generated by the overlapping of waves belonging to two different neutrons (Croca, Garuccio, Lepore, Moreira 1990, pag. 558).

But they also stress how “recent interference experiments of two photons have opened the way to clarify the problem” (Croca, Garuccio, Lepore, Moreira 1990, pag. 558)⁶⁹¹.

Croca, Garuccio, Lepore, and Moreira used photon pairs and compare the found joint detection probabilities with quantum optical predictions, find two very diverse results. According to this, the phase related to neutron interference and its difficulties could be overcome by the use of photon pairs. They asserted that:

We want to stress that the different results given by the two approaches to the experiment are essentially due to the assumption that de Broglie waves do really exist.

Thus, the proposed experiment can be considered as a tool for disproving either de Broglie’s model or the quantum optical predictions. Moreover, we wish to recall that experiments using a similar technology have already been performed; therefore, the difficulties of the previous experimental proposal, based on neutron interference, have been overcome (Croca, Garuccio, Lepore, Moreira 1990, pag. 563).

A year later Wang, Zou, and Mandel released “Experimental test of the de Broglie guided-wave theory for photons”. They made the experiment and in the abstract they stated how:

⁶⁹¹ Thanks to the contribution of the following paper, a path to start real experiments was finally made available. The contribution (also introduced in the reference n. 8 of Croca, Garuccio, Lepore, Moreira 1990) comes from: Hong, Zou and Mandel, 1987; Gosh and Mandel, 1987; Brendel, Schumtrumpf, Lange, Martienssen, and Scully, 1988; Mohler, Brendel, Lange, and Martienssen, 1989.

A two-photon interference experiment has been carried out, based on an idea recently proposed by Croca, Garuccio, Lepore, and Moreira. The experiment is designed to test a prediction of the de Broglie guided-wave theory, according to which the waves have a physical reality in addition to yielding photon detection probabilities. The results clearly contradict the de Broglie theory, but are consistent with conventional quantum mechanics (Wang, Zou, Mandel 1991, pag. 1111).

In the same 1991 this paper received a first comment, always on *Physical Review Letters* by Holland and Vigier⁶⁹². In 1992, Zou, Grayson, Wang, Mandel released their own paper in which they declared that the outcome of their second experiment do not induce coherence, still violating the Selleri-Croca proposal and even neglecting the physical reality of EW:

According to the de Broglie pilot wave theory, a pilot wave which is empty of photons is still capable of physical effects like induced coherence. We have carried out an experiment to test this prediction. The experiment involves interference of photons produced by down-conversion in two nonlinear crystals and depends on the mutual coherence induced between the two signal beams by an idler. When the photon is removed from the idler by a beam splitter, it is found that the resulting “empty pilot wave” no longer induces coherence. This violates at least the Selleri-Croca version of the de Broglie pilot wave theory makes the physical reality of the corresponding pilot wave doubtful (Zou, Grayson, Wang, Mandel 1992a, pag. 3667. My underline).

Then appeared the comment to (Wang, Zou, Mandel 1991) by Croca, Garuccio, Lepore, and Moreira; and the further reply by Zou, Grayson, Wang, and Mandel (1992b).

Concerning (Wang, Zou, Mandel 1991), the paper including their first experiment, Garuccio provides further technical details, still recalling in his 1994 paper, “de Broglie’s Wave in Space and Time”⁶⁹³, how they made the experiment on the proposal made in (Croca, Garuccio, Lepore, Moreira 1990), but they used a lightly different set-up. The calibration of this apparatus was delicate because it was “necessary to assure the spatial

⁶⁹² Holland P.R., Vigier J.P. (1991), “Comment on ‘Experimental test of the de Broglie guided-wave theory for photons’ ”, *Physical Review Letters* 67, p. 402.

⁶⁹³ Garuccio A. (1994), “de Broglie’s Wave in Space and Time”, in *Waves and particles in light and matter*, Garuccio A., van der Merwe A. (eds), New York: Plenum Press, pp. 37-47. (Garuccio 1994) in the following.

superposition in BS_1 of the empty wave and of the full wave associated with the idler photon” (Garuccio 1994, pag. 43).

Garuccio also stated two criticisms made to the experiment by himself with Croca, Moreira, and Lepore (1992); and by Selleri (Selleri two-photon 1992 see later), respectively:

As is known, the results of the experiment are in agreement with quantum mechanical predictions and contradict what is expected on the basis of the de Broglie theory. A first criticism to this experiment was based on the position of filters, before the photodetectors and not before the apparatus. The real coherence length of the wave packet inside the interferometer is 10 time shorter than the coherence length determined by the filters; then the calibration of the photon paths based on this large coherence length does not assure that the packets are superimposed in BS_2 and that two real waves overlap in the same region of space (Garuccio 1994, pag. 44).

As consequence of this first criticisms, Wang, Zou, and Mandel performed a new version of their experiment (1992b) but the results were essentially the same as before.

Then Garuccio continued explaining the criticism made by Selleri and how he suggests his variable photon detection probability:

A different criticism [...] has been proposed by Selleri and it is based on variable photon detection probability.

Indeed, if the results of previous experiment are correct, it is necessary to deny one of three postulate of the de Broglie theory. The third postulate is the less essential in the theory, and it moreover is equivalent to the Born interpretation of Ψ function of quantum mechanics (QM). Starting from a realistic and causal point of view, it is possible to develop variable probability detection models (VDPM) that split the set S of detected objects in a certain number of subsets S_i with probabilities P_i to be detected, so that the overall detection probability results from the average of different probabilities $P = \langle P_i \rangle_i$.

These models agree with QM for a single photon detection probability; but, since the average of a product is in general different from the product of the averages, it is in the case of two-particle observations that one can expect a departure both from QM and de Broglie’s third assumption on detection probability.

In particular, the model discussed by Selleri:

- (a) reproduces single photon physics;
- (b) explains the observed violation of Bell-type experiments;

- (c) is consistent with the results of the performed two-photon experiments;
- (d) is compatible, within the experimental errors, with the Wang, Zou, and Mandel 1991 experiment (Garuccio 1994, pag. 44. My underline).

In a typewritten document I found in FSPA, Selleri described in this way the contention, recalling the (Croca, Garuccio, Lepore, Moreira 1990) proposal of the experiment; its effective preliminary implementation by (Wang, Zou, Mandel 1991); the criticisms of the former scholars (Croca, Garuccio, Lepore, Moreira 1992), and the new execution submitted in (Zou, Grayson, Wang, Mandel 1992b):

The [neutron interferometry scheme for de Broglie's wave detection] was taken up by Garuccio in a 1990 work with two Portuguese physicists (Croca and Moreira) and with Lepore but with a source of photon pairs emitted in a parametric down conversion process. The experiment thus proposed was carried out in a preliminary version by Wang, Zou and Mandel who believed they could draw negative conclusions about de Broglie's wave. However, the adequacy of the experiment was criticized, in some of its essential aspects, in a subsequent article of the same Italian-Portuguese collaboration, which was able to show that the results were actually compatible with the existence of de Broglie's waves. These criticisms gave rise to a new execution of the experiment by Wang, Zou and Mandel. The results of these experiments are currently the subject of careful analysis as regards both the more properly experimental aspects and the theoretical implications.

At the present state of research it seems that only by admitting a variable probability of detection for single photons is it possible to reproduce the results of the experiment in de Broglie's approach⁶⁹⁴: moreover, the probability of detection is precisely the fundamental idea that all Italian local realists carry forward in this field. It should also be noted that this experiment is the first carried out, since the formulation of dualism, to verify the validity of the ideas of the French physicist⁶⁹⁵ (FSPA. My underline).

The substantive outcome was that the first experiment in order to detect EW was finally carried out.

⁶⁹⁴ That is the Selleri's proposal, as recalled by Garuccio at page 510.

⁶⁹⁵ The Italian original text is in Addendum 3, p. 1022.

Selleri contributed to the two photon interference debate with four papers, also describing the foregoing 1990-1992 papers submitted in the debate on *Physical Review Letters*.

I am reporting here only two of them. The first is “Two-Photon Interference and the Question of Empty Waves”⁶⁹⁶, mainly for his description of the abovementioned papers related to two-photon interference, i.e., Ghosh and Mandel 1987; Ou and Mandel 1989; and (Wang, Zou, Mandel 1991)⁶⁹⁷, as well as for his comments thereon. Firstly, Selleri pointed out the new field related to two photon detection:

The two-photon detection, with correlated photons allows “a completely new formula, never before met either in de Broglie’s or in the standard Copenhagen approach. The fourth power of the wave function has the effect of magnifying all interference phenomena” (Selleri two-photon 1992a, pag. 281).

Then he described the foregoing papers:

Two-photon interference on a screen. In an experiment performed by Ghosh and Mandel, the idler and signal photon from a parametric down conversion process hit the same screen. [...] In this experiment there are no polarizers or beam splitters and only the double-detection probability should be considered. [...] If the Ghosh-Mandel geometric factor [...] is considered, the visibility is reduced to 35%, still compatible with the rather poor experimental data. From our point of view it is not surprising that the experiment has been done only once and only with three points with large errors: a near 100% visibility for the interference figure was probably impossible to obtain. Our approach could, however, lead to a large visibility (Selleri two-photon 1992a, pagg. 282-284).

After Ghosh and Mandel, Selleri dealt with two-photon interference beyond a beam splitter, talking about the Ou and Mandel experiment:

This experiment [...] is in some respects similar to the previous one (a two-photon interference on a screen is detected), but its essential new feature is the presence of a beam splitter that introduces extra probability factors and increases the visibility of interference. (Selleri two-photon 1992a, pagg. 284-285).

⁶⁹⁶ Selleri F. (1992), “Two-Photon Interference and the Question of Empty Waves”, in *Wave-Particle Duality*, Selleri F. (ed.), London/New York: Plenum, pp. 277-289. (Selleri two-photon 1992a) in the following.

⁶⁹⁷ For details, see the references in (Selleri two-photon 1992a, pag. 289).

Selleri continued describing the 1991 Wang, Zou, Mandel experiment:

[They] performed an experiment with photon pairs produced in a parametric down conversion process. Their apparatus contained three beam splitters BS_1 , BS_2 , BS_3 that will be considered with a 50/50 transmission/reflection probability. In a coincident detection it is only the signal photon that can be detected by [the photomultiplier] D_2 , and then it is the idler photon that goes to D_1 . The experiment was designed to detect a possible influence of the signal empty waves reflected by BS_3 and by BS_2 on the probability that the idler photonic particle be transmitted by BS_1 and detected (Selleri two-photon 1992, pagg. 285-286. *Italics* in the original text).

The second paper is “Einstein-de Broglie Waves and Two-Photon Detection”⁶⁹⁸. Just for thoroughness, I report here some excerpts to compare with the detailed and accurate analysis made in (Garuccio 1994). The description according to which the photon detection probabilities could be seen as averages of probabilities applied to subensembles of the studied set of photon is easily accepted by a “realistically-minded physicist” because it is related to:

- (1) a general consequence of local realism deduced by applying the reality criterion to Einstein-Podolsky-Rosen pairs.
- (2) the conclusion that the experimentally observed violations of Bell-type inequalities of the strong type are due to the lack of validity in nature of the additional assumptions used in the derivation of such inequalities.

[...] the polarizers modify the physical properties of the transmitted photons and thus modify their detection probability as well (Selleri two-photon 1992b, pagg. 422-423).

Selleri pitched in with a good review on EW experiments until 1988 also in a deep chapter in the 1989 English version of what is perhaps his most well-known book, *Die Debatte um den Quantentheorie*⁶⁹⁹.

⁶⁹⁸ Selleri F. (1992), “Einstein-de Broglie Waves and Two-Photon Detection”, in *Bell’s Theorem and the Foundations of Modern Physics*, Van der Merwe A. et al. (eds), Singapore: World Scientific, pp. 422-427 (Selleri two-photon 1992b) in the following.

⁶⁹⁹ In the next paragraph I am going to talk about Selleri’s book and their transposition by several scholars. The German book has been translated in Japanese, Spanish, French, Polish, Greek, Italian, and English, (See Bibliography and Addendum 1). The 1989 English version is titled *Quantum paradoxes and physical reality*. At pages 123-180 it is possible to see Selleri’s review on EW.

But which were the reactions by the physicist community?

The (Garuccio, Vigier 1980) paper was challenged by Costa de Beauregard in 1981⁷⁰⁰, according to which: “the two photon problem defined by Vigier and Garuccio should be treated in the second quantization formalism; it is, thus, seen that the amplitude for finding one photon in the interference region of the two lasers remains the sum of the two interfering amplitudes” (Costa 1981, pag. 947). The feasibility of the EW experimental proposal was also questioned in 1988 by Mückenheim, Lokai, and Burghardt, with the Selleri’s reply already introduced.

In the years 1990-1992 there was the *Physical Review Letters* debate above reported.

Moreover, in (Selleri 1987a), he mentioned all the proposal made until then (by Selleri, Garuccio, Rapisarda, Vigier, and Andrade e Silva J. and M.).

In (Selleri 1987a, reference n. 3, page 375, see there for the details), Selleri quoted papers submitted until 1986 by: P.W. Milonni and M.L. Hardies; R. Loudon; L. Mandel; A. Heidemann and S. Reynaud; D.K. Umberger, P.W. Milonni and M. Lieber; A. Gozzini; C.E. Engelke and C.W. Engelke; C.E. Engelke; H.P. Yuen; L.C.B. Ryff; R. Giovanelli; and finally C. Fenech, still revealing a deep debate on the topic as well as Selleri’s care for introducing the best possible state-of-the-art.

In (Selleri 1987b, references 5 and 6, see there for details), Selleri introduces new contributions by others scholars, i.e., Helmut Rauch⁷⁰¹ and Karl Popper.

Adriano Gozzini⁷⁰² at Pisa and Raffaele Giovanelli and Vittorio Degiorgio⁷⁰³ (CISE)⁷⁰⁴ at Milan, often acknowledged, will play a role in Selleri’s tentative

⁷⁰⁰ Costa de Beauregard O. (1981), “Comments of a recente proposal by Garuccio and Vigier”, *Foundations of Physics*, 11 (11-12), pp. 947-948. (Costa 1981) in the following.

⁷⁰¹ Helmut Rauch (1939-2019): Already quoted in the earlier pages. See <https://www.fetzer-franklin-fund.org/media/helmut-rauch/> and <https://inspirehep.net/authors/1061821>. Both accessed 24 September 2020.

⁷⁰² Adriano Gozzini (1917-1994): His skills included design of laser devices and their uses in quantum optics. He also received a dedicated book: *Interaction of Radiation with Matter. A Volume in Honour of Adriano Gozzini*, a cura di G. Alzetta, E. Arimondo, G.F. Bassani e L.A. Radicati di Brozolo, Pubblicazioni della Classe di Scienze della Scuola Normale Superiore, Pisa, 1987. See also: <https://www.treccani.it/enciclopedia/adriano-gozzini/>. Accessed 24 September 2020.

⁷⁰³ Vittorio Degiorgio (1939-): Professor Emeritus at Department of Electrical, Computer and Biomedical Engineering in Pavia. His skills include Laser, Photonics, Nonlinear Optics, and so on. He was Full Professor in Physics of Matter at the University of Pavia, Italy, from 1980 to 2011, since which time he has been

attempt of establishing an experiment on EW. The following correspondence and interview could shed also a light in revealing these interests and debates.

I am going to submit here some correspondence between Selleri and Gozzini; and between Selleri, Giovanelli and Degiorgio.

The found correspondence between Selleri and Gozzini includes five letters, four of which sent by Gozzini. I reported here some excerpt. In the first letter, Gozzini expressed his appreciation for the proposal and he was very supportive about the implementation of the EW experiment, despite possible constraints:

The experience you are proposing is exciting. [...] I was not thinking about the opportunity of the “ghost wave” to induce the stimulated emission. A positive result of this experiment would be really exciting. Doing it is immediate with the device already used by Martinolli. So we are going to do it. We are doing these experiences again in a cleaner way. The experimental difficulty lies mostly in the fact that the amplifier [...] is a source that produces a background. It is necessary to avoid this, i.e. replace the lamp with a laser and place the tube holders at a certain distance very large so as to receive only the light of interest and avoid the diffused one. [...] We will talk about it when you come to Pisa⁷⁰⁵ (Gozzini to Selleri, 1 July 1981-FSPA).

In the subsequent one Gozzini continued the work, even though he began to express the difficulties inherent in the preparation:

Now [the laser] is working and we are making preparations for the test. As usual, there are many small unforeseen things that slow down the work. As for the amplifier, I think that if it works (and it has to work) it will be more difficult to make it work with a simple doubling than with a higher multiplication⁷⁰⁶ (Gozzini to Selleri, 7 February 1982-FSPA).

Professor Emeritus at the university. He was Head of the Department of Electronics at the University from 1991 to 1997 and Director of the Laser Center from 2007 to 2011. Prior to join the University of Pavia, he had been a researcher at the National Council of Research (CNR) since 1964, operating at CISE Labs, Milan, Italy. Dr. Degiorgio is a Fellow of the Optical Society of America and of the Italian Physical Society. He has been a Member of the Physics Panel of the European Union and of the Scientific Council and Directive Council of the European Laboratory for Nonlinear Spectroscopy (LENs), Florence, Italy. He is also a past Vice-president of the National Institute for the Physics of Matter (INFM). Dr. Degiorgio is the author of more than 200 articles in Photonics and Statistical Physics as well as co-author or co-editor of five books.

See: https://www.researchgate.net/profile/Vittorio_Degiorgio;

<https://scholar.google.it/citations?user=oqg0bncAAAAJ&hl=it>. Both accessed 24 September 2020.

⁷⁰⁴ CISE is the acronym for Construction, Innovation and Sustainable Engineering. See: <http://www.cise.polimi.it/category/news/>. Accessed 24 September 2020.

⁷⁰⁵ The Italian original text is in Addendum 3, p. 1022.

⁷⁰⁶ The Italian original text is in Addendum 3, p. 1022.

Selleri appreciated a lot this opportunity, revealing his hope in doing the experiment. In addition, he spoke about Nick Herbert⁷⁰⁷, with whom he had several contacts. Also here Selleri revealed himself as a scholars always ready to try helping his colleagues:

I cannot tell you how pleased I am with your amplifier and the confidence you show in its operation. It seems clear to me that if it really works essential steps will be taken, not only with ghost waves. Nick Herbert, who has emphasized more among the theorists the very significant role of the laser amplifier, needs an injection of experimental confidence, as can be seen from the letters he writes to me. Among other things, he can't get his work accepted. That is also why I suggested you write something. But it's good to wait for the amplifier to work⁷⁰⁸ (Selleri to Gozzini, 20 February 1982-FSPA).

Gozzini developed the difficulties within the experiment:

[...] the experiment looks very complicated because of the differences in the pump laser power between each pulse. I think they won't get much with the pulsed laser. [...] I have reviewed the whole thing and saw that the experiment is much easier using a continuous laser and I have calculated the experiment in detail (Gozzini to Selleri, 17 November 1982-FSPA).

[...] I sent my contribution, with much delay, due to the desire to put some experimental results into it. I couldn't manage it and so I sent what was attached. It's not a great thing but I show that the experiment can be done, and it doesn't look very difficult using a continuous laser. The idea of using a pulsed laser was wrong because the power varies too much from pulse to pulse; the frequency of repetition of the pulses is too low and moreover the laser in Florence is not very good (you need to change the gas very often). I think that with the laser of Catania the experiment can be done. I read an article [...] which states that amplification without noise is impossible because of spontaneous emissions. Of course it is impossible, the problem is to make the noise small so as to make possible the experiments you suggest⁷⁰⁹ (Gozzini to Selleri, 17 January 1983-FSPA).

⁷⁰⁷ Nick Herbert (1936-): He introduced FLASH, a superluminal communicator. In the abstract of his paper he stated: "The FLASH communicator consists of an apparatus which can distinguish between plane unpolarized (PUP) and circularly unpolarized (CUP) light plus a simple EPR arrangement. FLASH exploits the peculiar properties of "measurements of the Third Kind." One purpose of this article is to focus attention on the operation of idealized laser gain tubes at the one-photon limit." FLASH was "a scheme for sending signals faster than the speed of light using quantum entanglement".

See: [https://en.wikipedia.org/wiki/Nick_Herbert_\(physicist\)](https://en.wikipedia.org/wiki/Nick_Herbert_(physicist)); <https://link.springer.com/article/10.1007/BF00729622>. Both accessed 24 September 2020. See also (Kaiser 2011) and (Baracca, Bergia, Del santo 2017).

⁷⁰⁸ The Italian original text is in Addendum 3, pp. 1022-1023.

⁷⁰⁹ The Italian original text is in Addendum 3, p. 1023.

Anyway, in a comment submitted in a research program, Selleri in 1984 stated the difficulty to make the experiment in Pisa:

Since it was now clear that the idea of empty waves held up the tightest tests, the concrete problem of experiments arose: a difficult problem because ours is a theoretical group and it is a matter of persuading others to do the experiments we proposed.

Due to various personal events, the hope that the experiment of empty waves would be carried out quickly in Pisa has faded⁷¹⁰ (AGAB-Progetti di ricerca-Richieste finanziamenti ricerca scientifica dip di fisica armadio D settore 22 palchetto 4 busta 1).

The correspondence between Selleri, Degiorgio and Giovanelli includes four letters. In the first one, Degiorgio, being an experimental engineer, pointed out the lackness of practical details in the source he received and asked for the goal of the experiment, although he did not consider the amplifier, which instead was really crucial for Selleri:

I found neither in Selleri's articles nor in the pages left to me by Giovanelli a quantitative discussion of the proposed experiment with the pulsed laser. Before discussing many technical details I would like to be sure I understood the purpose of the experiment.

Reasoning on an ideal situation in which the effects of the spontaneous emission of the amplifier and the noise of the detectors are negligible [...] I find a probability of coincidence between the detectors equal to $4,5 \times 10^{-3}$.

My questions to you are:

- 1) Is your prediction that the probability of finding a coincidence in the ideal experiment is different from the one I calculated above?
- 2) If the answer to question 1 is yes, what is your prediction? Or if you are unable to make a quantitative prediction, what is the accuracy you require from the experiment?
- 3) Why do you need pulses with a duration of less than a picosecond to do the experiment? [...] Hoping to arrange a meeting soon⁷¹¹ (Degiorgio to Giovanelli and Selleri, 17 April 1985-FSPA. My underline).

⁷¹⁰ "Essendo ormai chiaro che l'idea delle onde vuote reggeva le più serrate verifiche si poneva il problema concreto di esperimenti: problema difficile perché il nostro è un gruppo teorico e si tratta di persuadere altri a fare gli esperimenti da noi proposti. Per varie vicende personali si è affievolita la speranza che l'esperimento delle onde vuote fosse fatto in tempi rapidi a Pisa" (AGAB-Progetti di ricerca-Richieste finanziamenti ricerca scientifica dip di fisica armadio D settore 22 palchetto 4 busta 1).

⁷¹¹ The italian original text is in Addendum 3, p. 1023.

Selleri noted how the letter, written on 17 April, was sent on 19 April and was arrived to Bari only on 27 April, emphasizing his real hurry in order to organize an experiment. Indeed, he answered only two days after, stressing how the amplifier, really pivotal for his experiment, had to play a key role, while instead Degiorgio seemed to give it no role:

Dear Degiorgio, I don't like the fact that the amplifier plays no role in your little report, whereas in our idea it is essential to allow the empty wave to be revealed. I enclose a small calculation made some months ago that allows to calculate the average number of photons $\varphi(x)$ generated by a single photon in an amplifier of width x : $\varphi(x) = \exp(P_x)$ ⁷¹², where P is a parameter that is calculated precisely by measuring $\varphi(x)$.

In the same way we can calculate the average number of photons $\varphi(x)$ generated by an empty wave: the result [...] contains the parameter P_0 that we might call, using a mediaeval term, "stimulating virtue" of the empty wave. The given numerical estimate is perhaps too pessimistic: $P_0 = P/2$ is perhaps better, at least as a semi-classical estimate.

As far as the picosecond is concerned, I think Giovanelli introduced it for reasons of maximum efficiency: even the nanosecond can go well, in my opinion. We can meet each other whenever you want, to my great pleasure⁷¹³ (Selleri to Degiorgio, 29 April 1985-FSPA. My underline)

After these explanations, Degiorgio began understanding Selleri's point of view and still continued asking for details, also pointing out the significant explanation of the two-photon case:

Through your calculation I began to understand your point of view. What experiment is Gozzini conducting on vacuum waves? What is the semi-classical estimate of P_0 ?

To complete the analysis of the ideal experiment it seems to me that we need to calculate what happens when the impulse contains two photons: if the beam splitter sends one to the right and one to the left, a photon and a vacuum wave arrive on the amplifier at the same time. Does this vacuum wave travelling together with the photon contribute, in your opinion, to the stimulated emission?⁷¹⁴ (Degiorgio to Selleri, 5 June 1985-FSPA).

⁷¹² Selleri also takes note on the following letter sent by Degiorgio on 5 June 1985, of the formulae $P_x + P_0/P (e^{P_x} - 1)$.

⁷¹³ The italian original text is in Addendum 3, p. 1024.

⁷¹⁴ The italian original text is in Addendum 3, p. 1024.

In the last letter I found, Degiorgio attached his calculations and asked for a meeting, maybe to define the details:

I enclose the calculation made starting from the Master Equation which also includes the effect of spontaneous emission. It should be the final result, unless there is a doubt related to the fact that the ME is derived in a single mode model (I have to think about it a bit). I did not have time to draw the charts in time, which should not hold any big surprises⁷¹⁵ (Degiorgio to Giovanelli and Selleri, 13 February 1986-FSPA).

In the letter there is also a comment by Giovanelli: “Overall it seems to me that we can hope for a start to research”⁷¹⁶, which expresses the presence of some difficulties and a possible solution, which, however, will not happen.

In a notebook I found in FSPA⁷¹⁷ (and about which I will talk later), Selleri held a seminar in Milan on 21 September 1989, on the opportunity to make an EW experiment at Pirelli⁷¹⁸ using optics fiber, showing once again his great efforts in order to find some laboratory in which to perform the experiment. Between the characters he met, Selleri reported a comment on the engineer Fontana:

Engineer Flavio Fontana: young, tall, smart, true driving force behind the idea of doing the empty wave experiment with optical fibres⁷¹⁹ (Notebook-FSPA).

Selleri also stressed the difficulty in the company related to the chance to do the experiment:

They have to take care of the business, they have no more time for empty waves.
[...] Giovanelli [...] is afraid that Fontana will be prevented from doing the experiment by the others at Pirelli⁷²⁰ (Notebook-FSPA).

⁷¹⁵ The Italian original text is in Addendum 3, p. 1024.

⁷¹⁶ “Complessivamente mi sembra che si possa sperare in un avvio della ricerca” (Notebook-FSPA).

⁷¹⁷ Selleri has always recorded comments on the scholars he has met, also denoting humour, sagacity and observational spirit. They seem to be made to memorize the people he met and to underline specific attitudes and possible points of contact.

⁷¹⁸ Pirelli & C. Spa: International company on tyres production and associated services founded in 1872, See <https://www.pirelli.com/global/en-ww/homepage>. Accessed 24 September 2020.

⁷¹⁹ “Ingegnere Fontana: giovane, alto, in gamba, vera forza motrice dell’idea di fare l’esperimento delle onde vuote con fibre ottiche” (Notebook-FSPA).

⁷²⁰ “Devono occuparsi delle applicazioni, non hanno più tempo per le onde vuote. [...] Giovanelli [...] teme che a Fontana venga impedito di fare l’esperimento dagli altri alla Pirelli” (Notebook-FSPA).

But, in a 1986 letter to Vladimir A. Meshcheryakov, Selleri had already underscored the impossibility to perform the experiment in Milan:

Here in Bari we are working hard now for deducing all the experimental consequences from the idea of real and stimulating waves: essentially the whole of laser physics needs to be redone. The situation with the group in Milan was worse than I hoped for: it has been cooling during my stay in Dubna. I do not worry too much, because now there is so much theoretical work to be done, and I am also sure that what we do will be noticed by the right persons, sooner or later (Selleri to Vladimir A. Meshcheryakov, 11 October 1986-FSPA. My underline).

Always concerning the transposition of Selleri's research on EW, it is worthy emphasizing the impressive correspondence he had with Karl Popper⁷²¹.

I will submit here only some excerpts concerning the EW topic, taken from six letters⁷²².

In the first one, Selleri, as done with de Broglie for his first paper on FQM, submits to Popper several details on his research, still asking for some feedback. The paper to which he referred is "Amplification of Light from an Undulatory Point of View", that is, (Selleri 1987a):

[...] **I have made some interesting advance with the empty waves.** The theory of laser amplifiers as it exists in books is very bad because: (1) It neglects all interferences and cannot therefore be considered quantum-mechanical; (2) It is in disagreement with the Blake-Scarl paper [...] where strong indications of Poisson distribution of the photon distribution in the amplified laser light have been found. The "usual" theory predicts instead a nonpoissonian distribution for amplified laser light. **A straightforward calculation based on the idea that the stimulation is due to incoming empty waves leads instead to the conclusion that the distribution of photons in the final state should be poissonian. Therefore the whole thing smells of truth, since: it agrees with experiments better than the usual theory; and it gives a far simpler result concluding that the elementary mechanism of photon amplification leads to a poisson distribution and not a complicate and ununderstood process inside the laser!**

⁷²¹ The Karl Popper Collection at University Library Klagenfurt (<https://www.aau.at/en/university-library-klagenfurt/karl-popper-collection/>. Accessed 24 September 2020) is the Popper's official repository. For all excerpts, the following sentence applies. "**With permission of University of Klagenfurt / Karl Popper Library. All rights reserved**".

⁷²² All statements and words underlined in the Selleri-Popper letters belong to the original text.

These ideas can be put in a stringent test by making some photon counting experiments on amplified laser light. Such experiments are possible now, I am told. My first paper, submitted to *Physics Letters*, is in the present envelope. Your reactions would be very, very appreciated. [...] (Selleri to Popper, 22 October 1986-Karl R. Popper papers, [Box 562, Folder 19], Hoover Institution Archives. Selleri underline, my **bold**).

Popper answers appreciating Selleri's efforts, waiting for the paper, hoping in the experiment and admitting his belief in de Broglie waves:

[...] I have just received your letter and abstract and read both. All seems to me very promising. Of course, everything will depend on the experiments of which you speak at the end of your letter. I shall write again when I have read the paper. As you may remember, I strongly believe in empty de Broglie waves [...] (Popper to Selleri, 24 November 1986-Karl R. Popper papers, [Box 562, Folder 19], Hoover Institution Archives).

Selleri sends the version of his "Coherence properties of Photon Amplifiers" (Selleri 1987b), which he defines "more detailed" with respect to (Selleri 1987a):

[...] Here I'm sending you a more detailed version of the "empty wave" paper. **It is very difficult to convince people to make experiments on these matters, but I am trying my best. Logically speaking it is practically certain that the result will come out in favour of empty waves, otherwise the Blake-Scarl expt should remain unexplained!** (Selleri to Popper, 2 December 1986-Karl R. Popper papers, [Box 562, Folder 19], Hoover Institution Archives. Selleri underline, my **bold**).

The (Selleri 1987b) paper received a strong support by Popper, which in addition, brings his own contribution:

My dear Franco, your letter of December 2nd with your beautiful paper 'Coherence properties of Photon Amplifiers' reaches me only today. **I feel confident that it will decide in favour of empty waves.**

[...] **the preceding results are convincing. I believe even more: I believe that wave-generated stimulation can give rise where particle-generated could not** (because there may be no particles for seconds on end, or for minutes on end).

In other words, I believe that completely empty waves may be amplified: the wave-amplitude is a continuous magnitude, and so it its possible stimulating effect upon amplification devices.

Or to put it still differently: while amplification is of course an energy-conserving effect, stimulation, the act of stimulation - does not need to supply energy. The quantum theory tells quite clearly that so little energy as would be needed for stimulation does not exist. So let us take this seriously. (The term 'photo-multiplier' is totally misleading: you cannot multiply zero and obtain a result.)

Why so I say so? Why do I conjecture this? Very simple: the pictures transmitted from the space ships - from Jupiter, from Saturn. I came to this conclusion - that the waves during this transmission must have been empty of photons during longer intervals of time than were needed for fixing a point of the picture - some considerable time ago. And when I met one of the physicists who had been engaged in the transmission + amplification, I asked him. And his reply was "According to our calculations we should have got no pictures. We were most surprised and simply could not explain it". "My reply was: the explanation is a simple conjecture: there is no lower limit to the wave intensity of empty waves".

(This encounter happened in Pavia when I gave a lecture there at the end of May or the beginning of June 1986. The physicist was an Italian working at MIT. I have forgotten his name, but I can find it out, if you are interested).

In these cases it must be an amplification that reaches a state where the waves might be empty for, say, an hour. But a second would be enough to clinch the issue of the stimulation through empty waves.

Your experiment will of course clinch the issue. **I should not wonder if you would get far better results than you expect.** [...] My best congratulations to your paper.

[...] Yours ever [...]

(Popper to Selleri, 12 December 1987-Karl R. Popper papers, [Box 562, Folder 19], Hoover Institution Archives. Popper underline, my **bold**).

Selleri replies showing his thankfulness and his excitement and hope for a future experiment:

[...] thank you very much for [...] your kind opinion about my paper 'Coherence properties of photon amplifiers'. [...] As you can imagine I am very excited of the obtained results and eager to see it if works in practice, as I believe it will [...] (Selleri to Popper, 23 February 1987-Karl R. Popper papers, [Box 562, Folder 19], Hoover Institution Archives).

The last letter I mentioned is referred to another idea on EW which Selleri was developing, which had no direct answer by Popper. Maybe Selleri did not find the opportunity to perform an experiment to his foregoing proposal (see Selleri 1987b); or he did not find a positive outcome. So he thought to another path:

Dear Karl, here is yet another possibility to detect de Broglie's waves. I find this idea extremely nice: a particle coming from path A reveals the interference between the empty wave packets which came from paths B and C, even if A,B,C overlap only in the region of arrival! Is it not fantastic? The Copenhagen ideology would say that the detection should not exist because it corresponds (for them) to the interference of nothing with nothing! The paper will appear on *Physics Letters*. Your comments would be greatly appreciated [...] (Selleri to Popper, 1 July 1989-Karl R. Popper papers, [Box 562, Folder 19], Hoover Institution Archives).

Leonard Mandel⁷²³ undoubtedly play a very remarkable role in EW, being the author of key papers and experiments. Garuccio and Selleri had several contacts with him and his group. I have already submitted Garuccio's experience with Mandel with some excerpts by the interview I had with him (see pages 400-402 in this dissertation). Here I am introducing another evidence released by Garuccio, which also stresses that the diverse outcome found by Mandel et al. (Zou, Grayson, Wang, Mandel 1992b), had a chilling effect on Bari's group:

Both Franco and I started from a strong idea that was also partly shared by Vigier, that the waves were real objects, that they had real phenomena, that they were not only waves of probability and that all those who were on this subject had a correspondence with us.

An experiment on de Broglie's waves was one that we tried to do with Croca, who also came from the realistic school of Andrade and Silva, a pupil of de Broglie. Our early experiment wanted to try to prove the existence of a wave effect that could be revealed even without the need of the photon.

The experiment carried out by Mandel, who repeated it anyway because there was something wrong with his first apparatus, showed in a certain way that this thing does not work and this too has a bit frozen our interest. Because you had to find other solutions, other general frameworks. However, it is not easy to find an apparatus in which you can highlight this aspect (Interview with Augusto Garuccio, Bari, 01 July 2019. My underline).

⁷²³ Leonard Mandel (1927-2001): He was Lee Dubridge Professor Emeritus of Physics and Optics at the University of Rochester (1964-2001). Other institutional affiliations included Imperial College London and Imperial Chemical Industries, Ltd. His research interests included quantum optics and laser physics. See: <https://history.aip.org/phn/11606004.html>; <https://www.nap.edu/read/11522/chapter/16>; <https://physicstoday.scitation.org/doi/full/10.1063/1.1404856>; <https://iopscience.iop.org/article/10.1088/2058-7058/14/4/14>. All accessed 24 September 2020.

And I reported other evidences made by Selleri during the interchange of proposal and experiments made between the Italian-Portuguese group (Croca, Garuccio, Lepore, and Moreira) on one hand; and the US Mandel-Wang-Zou-Grayon group on the other hand.

I found a very worthwhile fax sent by Selleri to Mandel. It tells a lot on the difficulties in doing experiments and is an evidence on the challenge between the two foregoing groups, as well as on the role played by Selleri in proposing some diverse path to be investigated.

The central problem remained the positioning of the three beamsplitters, in order to truly revealed a possible contribution by empty waves:

Dear Professor Mandel,

[...] **Given the great importance of your experiment I was a little bit disappointed to learn that the positioning of BS_1 , BS_2 and BS_3 had a chance of not being perfect. But I am very happy now to learn that you have repeated the measurements several times.** A further, probably very minor objection could be the following. In your fax you say that your shifts cause “i- BS_3 to become progressively shorter and s- BS_2 to become progressively longer”. Now for an exact positioning of the three splitters it could happen that both BS_2 and BS_3 need to be taken nearer to BS_1 , while your previously quoted sentence, if taken literally, seems to imply that you have only tried to increase their distance from BS_1 .

If your experiment is correct, as it now would seem, then 50% of the sky of empty waves becomes black. The residual possibility is that empty waves are able to stimulate photonic emissions from excited atoms as I suggested in papers and in my [...] (Quantum Paradoxes and Physical Reality, 1990). **If also this does not happen the Einstein-de Broglie idea can be considered dead.**

In all cases I have great logical difficulties in reconciling your present result with the 1967 Pfleegor Mandel evidence that photonic particles can be carried by a wave coming from a different laser.

But maybe I am thinking too much in terms of de Broglie, and the world is instead really not understandable to us as anticipated - for no good reasons - by the Copenhagen school.

Who has done the measurements on the present research: you personally or somebody else? I would feel much better in the former case! (Selleri to Mandel, 11 September 1990-FSPA. Underline in the original text. My **bold**).

As seen from these lines, the fax seems to be of paramount importance! Also because, after five months, Mandel sent a letter to Selleri, in which the US scholar describes some changements in the experimental set-up (surely subsequent to Selleri's recommendations), also admitting his slight acquaintance of the de Broglie's theory:

As regards the question of alignment of the signal and idler beams in our experiment, we usually make use of an auxiliary He:Ne laser and several 1 mm apertures to determine the photon trajectories. We estimate that the signal and idler photons were aligned at least a 3 or 4 mrad, and probably a little better, along the line BS_3 to BS_1 .

This is more than adequate to show up the interference minimum in the coincidence counting rate of detectors D_1 and D_3 . On the other hand, it is still a little larger than the diffraction angle. If the interference predicted by the de Broglie theory requires alignment to within the diffraction angle, then the visibility would be reduced, perhaps three or four-fold in our experiment. I admit that I do not understand the de Broglie theory well enough to be able to make a reliable quantitative estimate (Mandel to Selleri, 18 February 1991-FSPA).

In the interview with Freire Jr, Selleri answered to a precise question made by the Brazilian historian on Mandel, telling his experience and also talking about experimental physicist, so seminal for his proposals of experiment in EW but also on diverse topic in PP and QM:

[...] Of Mandel I had a very good idea because he had done so many interesting experiments. To mention just one, the Pfleegor-Mandel experiment, published on *Physical Review*, in which you have two lasers of very low intensity superimposing their beams and interfering. [...] In my opinion that experiment gives a strong confirmation of the objective reality of quantum waves. And so Mandel was the most attractive experimentalist we could see because of the nice experiments he had done and we invited him to participate in the Bari conference.

[...] in my opinion the later Mandel was different from the earlier. There had been a switch in his scientific personality, especially in the latest part of his life. Probably, this is my conjecture, he was trying to get the Nobel Prize for his research and was putting this ahead of everything. As a consequence, sometimes the interactions with him were frustrating. I had suggested an experiment to be done and he decided to do it.

Then he sent me a reprint to see what I could say about his conclusion that my idea was wrong. Then I read it and then I said to myself, "Well, this is impossible.

There is a conceptual mistake. It is not possible that Mandel has made such a trivial mistake.” I read it again and again. I wrote to him. And then he said, “Oh yes, yes, you are right. Then I must do it again, but nothing will change.” How could he know before doing an experiment correctly that nothing would change? That was just to keep me quiet.

And then he did the experiment a second time with the same conclusions, he sent me the reprint, I again found an error. I told him. He said again, “Nothing will change,” and he repeated it for the third time and of course it came out the way he wanted, but I wonder how much I should trust that experiment having such a particular history. In the last part of his life Mandel often published papers on *Physical Review Letters*. Some of them are really strange because they contain very poor statistics where the form of the interference figure is crucially important.

It is clear that it would have been possible to do it with much better statistics and the research is important enough to improve it, but he published that poor result and I wonder why (AIP 2003. My underline).

It is worthy pointing out that Mandel repeated the Selleri proposed experiment for three times!

Selleri talks about experimental physicists, having changed his idea on them after his experience with Mandel. He gave two examples as a confirmation of his new thoughts:

Basically interacting with Mandel I developed a critical attitude toward experimentalists. Before I had a rather “dogmatic” approach according to which an experimentalist is right by definition. And after this I understood that you must be very careful also with experimentalists because you never know what they cook up in their laboratories. I have a second story about a different group. I’ll tell you some experimental experience.

When I was advertising my experiments on empty waves I went in several laboratories — for example in a laboratory in Paris. I proposed my experiment and I had a well-known physicist who was very, very attentive and really interested, judging from the questions he asked. The conclusion of the long discussion was this: “Selleri, your ideas are very, very interesting, but unfortunately I cannot risk the scientific credibility of my laboratory by doing such an experiment”.

He was illuminating because you see, probably other people saw the matter in the same way but did not say it openly. He said it. If you do an experiment on the foundations of quantum mechanics by the very fact of doing it you are in trouble. You should not do experiments of a certain type because if you find that quantum mechanics is correct you have thrown away money and you are stupid because everybody knows that it would come out that way.

If you find the quantum mechanics doesn't work, it's a wrong experiment and one should be very careful before giving you some more money for research. So you see, experimentalists are also men and they are strongly influenced by philosophy.

Third example: I had interacted with an experimental group in Europe who had performed an experiment on Bell's theorem that I had suggested. It was a variant with three polarizers, not with two, and then when they started getting results I could not resist - I was young and very active, so I ran there and I talked to them and they told me they found a violation of quantum mechanics. Then the head of the group came. He denied that it was so, and the thing was covered up and later on they published something completely agreeing with quantum mechanics.

So I don't know how the experimental world works. Certainly you don't have to take literally many published experiments, but you must be very attentive to everything. It seems that you are interested in this side of the story.

All what I tell you can be documented. At home I have two large libraries full of books and documents. Letters. I have many drawers full of correspondence. They are organized geographically. One drawer is for France, another one is for Germany and so on, so a large collection of letters (AIP 2003. My underline).

To sum up, Selleri's research in EW covered a strong period, still proposing several kind of possible experiments until 1992, but basically, no experiments seem to have been performed, apart from those carried out by Wang, Zou, and Mandel, which, however, have also followed the indications of Selleri, as evidence of his role in the debate. As partial justification for this lack, it must also be remembered that Selleri and the others scholars who tried performing an experimental proposal was fundamentally theoretical physicists and that being able to carry out an experiment is definitely not an easy thing. As a theoretical physicist he could not manage to set up any experiment and he needed to find some laboratories which could perform it. And I illustrated how even the experimental physicists have faced several setbacks. Anyway, even researches are still going on, currently no positive answer has been found to EW topic.

Nevertheless, historically speaking, it is always interesting to study paths and trajectories that do not find positive results, in order to underline the importance of research and of different visions in science, even those considered "losers", which in any case make a contribution to knowledge.

**2.8.5-A TIRELESS, HECTIC ACTIVITY: TRAVELS AND
CONFERENCES ALL AROUND THE WORLD, PROPOSALS FOR
EXPERIMENTS AND COLLABORATIONS. POPPER AND MORE.
REMARKABLE BOOKS PUBLISHED AND APPRECIATED. A
STRONG DISSEMINATION ACTIVITY**

During the same years of his intense activity on EW, Selleri always experienced researches and papers on EPR and Bell's inequality. For instance, he published the massive (Selleri History 1988), already introduced in the early sections.

That is for emphasizing his tireless scientific work, which passed also for many journeys, a lot of conferences, the publication of several books and dissemination papers on social issues.

2.8.5.1-ORGANIZING CONFERENCES

In Addendum 2 I will exhibit all conferences attended and organized by Selleri all over the world, as I managed to find in his Archive, and also one of his personal list with the most important international conferences until 1991, according to his view.

Here I am going to submit some of them, with the aim at displaying how he managed to bring the most important scientists of the time, experts in the field of FQM and Relativity Theory, to Bari and other locations. And to organize accurate conferences that were appreciated by the guests. I have already explained his role in the organization of Varenna 1970 as well as his commitment inside the SIF for other workshops.

On 4-6 June, 1974, at Frascati National Laboratories, Selleri and the Physics Department of Bari University, joint with FNL itself, and INFN, planned the conference on “Mutamenti e prospettive della fisica fondamentale: i fondamenti della meccanica quantistica”.

In his talk Selleri dealt with the experiments on FQM. A lot of noteworthy Italian scholars attended it, and even though there were no proceedings, to the disappointment and even anger of some participants, the conference

remained an impressive landmark for those in Italy who would later engage in FQM research, also for the wide topics debated there.

In the poster of the conference that I found in the FSPA, it is stated the motivation, the lecturer (which I have arranged in alphabetical order) and the addressed issues⁷²⁴:

“In Italy an increasing number of researchers are interested in FQM. Therefore arose the twofold need to compare the initiatives undertaken and to introduce to this issue those who are approaching it now”.

Speakers:

Agodi-Baracca-Beltrametti(?)-Brigaglia-Di Toro-De Marzo-Faraci-Fortunato-Galgani-Garuccio-Ghirardi-Notarrigo-Rapisarda (?)-Recami-Ruggiero-Salandin-Selleri-Tonietti

Issues:

- introductory seminars:

- QM and objective reality: EPR, measurement theory, second type state vectors; wave-corpuscle dualism

- The problem of causality: problem of hidden variables, von Neumann's theorem and its overcoming; Bohm's ideas, Bell's inequality

- Stochastic processes: Stochastic interpretation, uncertainty principle and Schrödinger equation from Fürth to Nelson equation, zero point energy and Planck distribution.

- key issues

- causality and Bell's inequality: local hidden variables and non-dichotomic observable, sensitive and indifferent observables, matrix methods in local TVN, on the sufficiency of Bell's inequality.

- experimental issues: FQM experiments

- QM and Education

(FSPA).

In Freire's interview, Selleri answered on a specific question of the remarkable conferences on FQM held in Italy during the 1970s and 1980s: Varenna 1970 and 1977, Erice 1976, Bari 1983, still describing his attitude in sharing contacts and spreading ideas:

⁷²⁴ The Italian original text is in Addendum 3, pp. 1024-1025.

[...] when I started doing this activity I felt it was important to create contacts and have people give talks and try to spread the ideas as much as possible, so not only I organized conferences but I tried to push other people to organize conferences. Some conferences in Greece, Poland, Portugal were held after my suggestion. And so we had really a lot of activity and then we went in touch with a large number of physicists at those times. Our conferences were of course open also to those orthodox people who wanted a real confrontation of ideas (AIP 2003).

Specifically, Selleri organized the following international conferences, whose proceedings were published and appreciated:

- Perugia 1982 (*The wave particle dualism*)
- Bari 1983 (*Open questions in quantum physics*)
- Urbino 1985 (*Microphysical reality and quantum formalism*)
- Cesena 1991 (*Bell's theorem and the foundations of modern physics*)
- Trani 1992 (*Waves and particles in light and matter*)
- Olympia 1993 (*Frontiers of fundamental physics*)
- Athens 1997 (*Relativistic physics and some of its applications*)

I decided to quote in Addendum 3 the whole huge number of scholars who attended them⁷²⁵, in random order (alphabetical, as appeared in the proceedings or in the posters, and so on), so as to indicate how the best scientists in the field were present at the above conferences, still broadening the collaboration and the ideas. I report here, with a mere and purely subjective and not exhaustive personal choice, a few names, between the plenty of them, which I am most familiar with and that were present there: A. Aspect, O. Costa de Beauregard, L. Mandel, K.R. Popper, J.P. Vigi er, G.C. Ghirardi, P. Caldirola, J.A. Wheeler, B. d'Espagnat, P.K. Feyerabend, M. Jammer, O. Piccioni, A. Zeilinger, J. Bub, P.H. Eberhard, S.J. Prokhorovnik, H. Arp, D. Bohm, J. Andrade e Silva, E. Santos, J. B. Bastos Filho, and so on. A real *parterre de Rois*, actually!

⁷²⁵ I have reported in Addendum 3, at pp. 1025-1026, all people which submit his work on the proceedings, in addition to those actually present as confirmed by correspondence or posters.

The 1997 conference in Athens experienced a painful genesis, with the risk of being cancelled several times⁷²⁶.

As Selleri stated to Freire with respect to the importance of creating contacts between people, a lot of scholars were the addressees of some correspondence with Selleri. And some of them, like D. Bedford, P.H. Eberhard, S.J. Prokhovnik will be intertwined with Selleri further research on Popper EPR experiment, on neutral Kaon pairs and FRT, respectively.

Regarding Cesena 1991 conference, Selleri received an appreciation letter by Max Jammer:

You have to be congratulated for having organized and conducted a most stimulating and successful conference. Everybody with whom I spoke recognizes the indebtedness to you for this important academic event (Max Jammer to Selleri, 15 October 1991-FSPA).

And another letter sent by Max Jammer emphasized the great commitment of Selleri:

I greatly admire your efficiency and capability of actively participating at so many conferences, of writing such profound books and scientific papers, and, last but not least, of being actively engaged in physical research (Max Jammer to Selleri, 1 November 1987-FSPA).

2.8.5.2-SELLERI AND POPPER, A VIBRANT RELATIONSHIP⁷²⁷

Karl Raimund Popper⁷²⁸ needs no introduction at all, being surely one of the most famous scholars of the 20th century. Selleri experienced a long and deep correspondence with him.

⁷²⁶ In the archive of Michele Barone I found all the correspondence with the European Union for the funds and letters to the various scientists for the postponement, cancellation and finally the ultimate implementation of the conference.

⁷²⁷ As told before, the correspondence between Selleri and Popper is collected in Austria at The Karl Popper Collection at University Library Klagenfurt. For all excerpts, the following sentence applies. **“With permission of University of Klagenfurt / Karl Popper Library. All rights reserved”**. In the list of archival at the beginning of my thesis, this Archives are abbreviated with KPC (Karl Popper Collection)

⁷²⁸ Karl Raimund Popper (1902-1994): He was an Austrian-born British philosopher, academic and social commentator. One of the 20th century's most influential philosophers of science. See:

<https://plato.stanford.edu/entries/popper/>;
<https://www.britannica.com/biography/Karl-Popper>;

Selleri wrote in 1991 with A. Van der Merwe the paper “Karl Popper at Ninety: Highlights of a Lifelong Intellectual Quest”⁷²⁹, in which he painted an all-round portrait of the Austrian philosopher, analyzing, even with a critical eye, his numerous contributions to the philosophy of science. Selleri and Van der Merwe stated that:

To the great names [who disagreed sharply with the Copenhagen idea that the final formulation of the theoretical paradigm satisfactorily solved all quantum problems], that of the greatest philosopher of our century must be added. Karl Popper termed the cultural, philosophic, and scientific situation surrounding quantum theory, “the great quantum muddle” (Selleri, van der Merwe 1991, pag. 1375).

[...] Popper’s greatness does not and cannot lie in a series of proposals about the nature and problems of contemporary physics that are “all correct”; it resides instead in an overall framework of ideas of exceptional interest that have filtered through the progressions (and regressions) of scientific thought of this century. One has to heed Popper’s ideas if one is to understand anything essential about modern science. His proposal concerning *propensities* looks like an unescapable fundamental ingredient for any future realistic physics. And, to pick but one last example, his conceiving of scientific development as resulting from the continual interplay of conjectures and refutations [...] opens up an important vista for the relation between physics and philosophy. Specifically, if any conjecture is a free *creation* of whoever makes it, it is not far-fetched to expect that the prejudices of its creator will become part of the creation itself. How and why Bohr introduced Kierkegaard’s ideas into quantum theory thus also becomes clear (Selleri, van der Merwe 1991, pag. 1385. *Italics* in the original text).

According to (Baracca, Bergia, Del Santo 2017):

Karl Popper had moved criticisms towards the standard interpretation of quantum mechanics since the 1930s, when he also had intense discussions with several physicists, Einstein and Bohr among them [...].

Due to a mistake he made in a proposed *Gedankenexperiment*, Popper become discouraged for some time about his contributions to the foundations of quantum mechanics, but after the 1950s he kept working on his own on possible realist

<https://www.economist.com/democracy-in-america/2016/01/31/from-the-archives-the-open-society-and-its-enemies-revisited>;

<https://iep.utm.edu/pop-sci/>;

<https://scholar.google.com/citations?user=jmXp2pUAAAAJ&hl=en>. All accessed 24 September 2020.

⁷²⁹ Selleri F., Van der Merwe A. (1991), “Karl Popper at Ninety: Highlights of a Lifelong Intellectual Quest”, *Foundations of Physics*, 21 (12), pp. 1375-1386. (Selleri, van der Merwe 1991) in the following.

interpretations of quantum mechanics. Such an activity was widely illustrated by Popper in his book *Quantum Theory and the Schism in Physics* [...]. And it is at the very beginning of the 1980s as well, that Popper started an active collaboration with some of the physicists concerned with the new critics to quantum mechanics (Baracca, Bergia, Del Santo 2017, pagg. 75-76).

They affirmed also that Selleri established a relationship with Popper just before and during the Bari 1983 conference. Moreover, they have been ascertained that it was Vigier who introduced the two.

In a letter sent by Popper to the French physicist, the former stated that: “I got a letter from Bari and lost it! Please post the enclosed to the man in Bari (I do not know his name and address) and please send me his name and address!” (Popper to Vigier, no date-Karl R. Popper papers, [Box 247, Folder 18], Hoover Institution Archives).

In another letter, the first of their correspondence, Selleri wrote to Popper in order to invite him to the conference which was held in Bari on May 1983.

Selleri, together with some organization stuff, assures Popper on the presence of Vigier, which was the intermediary:

Dear Prof. Popper [...] I am happy to invite you to the Physics Department of the University of Bari during the first week of may for a workshop on the physics of quantum waves. [...] Jean-Pierre Vigier has arranged to come to London in order to travel with you.

[...] as you can imagine there is great excitement for your arrival in our University. I would suggest that besides taking part in the workshop and talking about your ideas you accept to give a public speech on the role of science in the modern society, or anything else of your choice (Selleri to Popper, 5 March 1983-Karl R. Popper papers, [Box 247, Folder 18], Hoover Institution Archives).

I managed to find in Garuccio’s archive (AGPA) the whole 38 pages typewritten document of the public Popper speech held at “Teatro Comunale N. Piccinni” in Bari on 7 May 1983. Its title was *Evolutionary Epistemology*. I decided to reveal here some excerpt, in order to underline the greatness and the clarity of Popper. He begins with the concept of evolution for human language and scientific knowledge:

This term, Evolutionary Epistemology, is the invention of an American psychologist, Professor Donald T. Campbell: in a book published in 1974 by Professor Paul Arthur Schilpp, Campbell described certain views of mine as “Evolutionary Epistemology”. [...] Donald Campbell called my epistemology “evolutionary” because I look upon human language, upon human knowledge and upon human science, as a product of biological evolution, especially of Darwinian evolution through natural selection. I look upon the following as the main problem of evolutionary epistemology: the evolution of human language and its part played in human knowledge; the ideas of truth and falsity; the delineation of states of affairs and their picking out from the complexes of facts that constitute the world-reality. To put this at once briefly and simply in the form of two theses:

First thesis: the specifically human ability to know, and also the ability to produce scientific knowledge, are the results of natural selection. They are closely connected with the evolution of a specifically human language. This first thesis is almost trivial. My second thesis is perhaps slightly less trivial.

Second Thesis: the evolution of scientific knowledge is, in the main, the evolution of better and better theories. This is, again, a Darwinian process.

We are always faced with practical problems; and out of these grow sometimes theoretical problems; for we try to solve some of our problems by proposing theories. In science these theories are highly competitive. We discuss them critically; we test them and we eliminate those theories which we judge to be less good in solving the problems we wish to solve: so only the best theories, those which are most fit, survive in the struggle. This is the way science grows. However, even the best theories are always our own inventions. They are beset by errors. What we do when we test our theories is this: we try to detect the errors which may be hidden in our theories.

[...] Third thesis: what enables a human scientist like Einstein to go beyond the amoeba is the possession of the specifically human language. While the theories of the amoeba are part of the organism of the amoeba, Einstein could formulate his theories in language - if needed, in written language. He could in this way put his theories outside his organism. This enabled him to look upon a theory as an object; to look at it critically; [...].

[...] My three theses together give an outline of my evolutionary epistemology.

[...] My fourth thesis (which I have been teaching and preaching for more than 60 years) is:

Every single aspect of the observationist philosophy of knowledge is mistaken.

- (a) Sense data or similar experiences do not exist.
- (b) Associations do not exist.
- (c) Induction by repetition or generalization does not exist.

Our perceptions may mislead us (“Evolutionary Epistemology”, public lecture concluding the Workshop on *Open Questions on Quantum Physics*, Bari, Italy 1983 May 7”-Karl R. Popper papers, Hoover Institution Archives. Underline in the original text)⁷³⁰.

The paper is really impressive but obviously I cannot submit here too much excerpts, because the argument, which is deep but very philosophical, is not relevant to the purpose of the thesis. I wish to stress only the great approach related to Popper to the great issues of our times.

Concerning the link between Selleri and Popper, it has also been investigated by (Nutricati 1998), according to which, they revealing clear disagreement on political and social matters as well as the real weight of Bell’s theorem:

[...] The theorem is considered by Selleri not only significant but, for its scientific-epistemological relevance, decisive in the battle between realism and antirealism.

On the contrary, Popper considered such a theorem avoidable, since he had a strong belief in the feasibility that quantum mechanics could be interpreted from the point of view of local realism, and therefore that Bell’s theorem was, in essence, false⁷³¹ (Nutricati 1998, pag. 40).

All disagreement were made in a huge atmosphere of respect and esteem:

My dear Franco, [...] I have [...] been reading your book again. I like it very much, but I disagree.

(1) there is no reason to regard QM (by this I mean the formalism in propensity interpretation) as non-local. The Copenhagen interpretation is now local, but not the formalism [...].

There were 2 argument (invalid) for non-locality: (a) the so-called “collapse” (or the “reduction”) of the wave packet.

(b) the Bell-inequality [...].

[...] there cannot be a proof of the non-local character of Q.M. [...] there is no reason to regard Q.M. as non-local. It is a local theory. It is, of course, incomplete. This goes without saying. I suspect that we shall moves have a complete physical theory [...]

⁷³⁰ This is a typescript version. The lecture was published in two different books: Pollard J.W. (Ed.): *Evolutionary Theory: Paths into the Future*, Chichester usw: Wiley 1984, pp. 239-255; Tarozzi G., van der Merwe A. (Eds.), *Open Questions in Quantum Physics*, Dordrecht: Reidel, pp. 395-413.

⁷³¹ “[...] Il teorema è da Selleri considerato non solo importante ma, per la sua portata scientifico-epistemologica, decisivo nella battaglia tra realismo e antirealismo. Al contrario Popper considerava tale teorema evitabile, poiché aveva la viva convinzione nella possibilità che la meccanica quantistica potesse essere interpretata dal punto di vista del realismo locale, e quindi che il teorema di Bell fosse, in sostanza, falso” (Nutricati 1998, pag. 40).

These are just one or two of the points I should love to discuss with you, more than with any other physicist now living (Popper to Selleri, 30 November 1983-Karl R. Popper papers, [Box 348, Folder 12], Hoover Institution Archives. Underline in the original text. My **bold**).

They also experienced with a fruitful cooperation in the struggle against the Copenhagen interpretation of quantum theory:

In the scientific-philosophical field, what has positively impressed Selleri is the idea of “propensity” developed by Popper.

[...] this idea [...] is considered prominent because it represents a form of probabilistic realism⁷³² (Nutricati 1998, pag. 40).

In another letter, Selleri expressed its attractiveness towards the study of the propensities:

[...] I recently studied for the first time in depth your idea of propensities. I find it fascinating because it is clearly the most general formulation of realism that I can think of. This is the real strong idea that we have in common and I am always very grateful for the great battle you fought and you fight against the idealistic conceptions of the Copenhagen school. You gave us some water in which we can now try to swim (Selleri to Popper, 28 November 1989-Karl R. Popper papers, [Box 562, Folder 19], Hoover Institution Archives. My underline).

I have already explained the importance of the probabilistic realism for Selleri, in order to bypass the criticism to the deterministic old statement in EPR criterion of reality.

On the propensity concept, Selleri stated in (Selleri, van der Merwe 1991):

Popper’s conception of propensity expresses a tendency belonging to the object (or set of objects) under study to produce particular outcomes when it is made to evolve under certain conditions. Propensity has to be understood with some numerical “intensity” and not as a simple qualitative possibility to undergo a particular evolution.

⁷³² “Nel campo scientifico-filosofico, ciò che ha positivamente colpito Selleri è l’idea di ‘propensione’ sviluppata da Popper. [...] questa idea [...] è considerata importante, perché rappresenta una forma di realismo probabilistico” (Nutricati 1998, pag. 40).

The frequency with which that evolution will show up among all the possible evolutions will be a quantitative measure of the “intensity of the tendency”, that is, of propensity (Selleri, van der Merwe 1991, pag.).

The strange thing is that Selleri write here above in 1989 about his depth study of propensities, where instead he already talk about propensity in a 1985 paper, i.e., in the first of the three papers in which Selleri cited Popper’s propensities, “On the Notion of Probability for Pairs of Correlated Systems”. The others are: “Propensity, Probability and Quantum Physics”, with Barretto Bastos Filho; and “Local Propensities and Quantum Theory”⁷³³.

In the (Selleri propensities 1985), Selleri introduced propensity as a solution between single systems and statistical ensembles, stating that:

In the direction of introducing probabilities for individual systems goes Popper’s propensity formulation of quantum theory [...]. Popper argued *against* the view “that a singular probability cannot be objective, and that an objective theory of probability must be a *purely* statistical of frequency theory” (Selleri propensities 1985, pag. 275. *Italics* in the original text).

[...] Popper’s propensity formulation of probabilities in quantum physics tries to bridge the gap between objective properties of a single system and objective probabilities in the statistical ensemble of many repetitions of the considered system. It allows the single system to have objective tendencies or propensities of varying strengths and shows that, if these are interpreted as tendencies to realize themselves, they lead to corresponding statistical frequencies in a virtual (or in a real) statistical ensemble (Selleri propensities 1985, pag. 287. *Italics* in the original text).

Already in 1985 he showed a clear knowledge of propensity and its use, quite contradictory with his foregoing 1989 letter in which he recognized his deep

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Selleri F. (1985), “On the Notion of Probability for Pairs of Correlated Systems”, in *Determinism in Physics*, Bitsakis E. et al. (eds), Athens: Gutenberg, pp. 275-288. (Selleri propensities 1985) in the following.
Barretto Bastos Filho J., Selleri F. (1995), “Propensity, Probability and Quantum Physics”, *Foundations of Physics*, 25 (5), pp. 701-716. (Bastos Filho, Selleri propensities 1995) in the following.
Selleri F. (1995), “Local Propensities and Quantum Theory”, in *Fundamental Problems in Quantum Theory: A Conference Held in Honor of Professor John A. Wheeler*, Greenberger D.M, Zeilinger A. (eds), New York: New York Academy of Sciences, pp. 798-811. (Selleri propensities 1995) in the following.

study of them. Anyway, in the paper with Bastos Filho, Selleri claimed for a objectivistic reinterpretation of quantum theory:

[...] Popper's idea of propensities constituting the physical background of predictable probabilities is reviewed and developed by introducing a suitable formalism compatible with standard probability calculus and with its frequency interpretation. Quantum statistical ensembles described as pure cases ("eigenstates") are shown to be necessarily not homogeneous if propensities are actually at work in nature.

An extension of the theory to EPR experiments with local propensities leads to a new and more general proof of Bell's theorem. No joint probabilities for incompatible observables need to be introduced

[...] The idea of propensity was advanced by Popper in order to obtain a unitary description of the physical reality in probabilistic terms. One of the main task to be achieved in this way is an objectivistic and realistic reinterpretation of quantum theory (Bastos Filho, Selleri propensities 1995, pag. 701).

In this paper, also their disagreement on Bell's theorem and the probabilistic local realism is pointed out:

A similar program was discussed by one of us [FS] but with the difference that it was soon realized that any objectivistic and local formulation of probabilities would lead to the validity of Bell's theorem and thus to the impossibility of a real agreement with existing quantum theory. Popper had instead considerable difficulties in reconciling himself with the existence of Bell's inequality (Bastos Filho, Selleri propensities 1995, pag. 701).

[...] In the present section it will be shown that the formulation of probabilistic local realism can naturally be presented in the language of propensities. We stress that the words "probability" and "frequency" are essentially identical in the present paper, in the sense that the probability of an event is considered nothing but its measured (or predicted) frequency in a given statistical set (Bastos Filho, Selleri propensities 1995, pag. 703).

In the latter paper, the link between reality and propensitiy is introduced, as well as how the only measurable propensity is related to a set of atomic systems, being considered as an average. In addition, locality with propensities is applied to wider set of probabilities:

[...] We assume, therefore, that the reality behind every stable and predictable probability is always a local propensity (Selleri propensities 1995, pag. 799).

[...] From a single act of measurement, one cannot deduce a probability and the individual propensities are thus not measurable. One can, however, introduce a propensity of a set of atomic systems to give different results with different frequencies. The latter propensity is instead easily measurable and can be considered as some kind of physical average of (undetectable) individual propensities (Selleri propensities 1995, pag. 800).

The words “probability” and “frequency” are essentially identifiable, in the sense that the probability of an event can be considered as nothing but its measured (or predicted) frequency in a given set of similar objects. A probabilistic reality criterion will now be given stating essentially that there is a real propensity behind every probability that can be predicted correctly before it is measured (Selleri propensities 1995, pagg. 800-801).

[...Our probabilistic reality criterion (PRC)] is not an attempt to “define” a physical reality, but only a criterion useful for recognizing the existence of an objective physical property (propensity) once a realistic attitude has been adopted. The [...] PRC provides a natural generalization of the famous deterministic reality criterion of Einstein, Podolsky, and Rosen (Selleri propensities 1995, pag. 801).

[...] it is important to stress that the present formulation of locality is strictly analogous to the quantum mechanical locality of probabilities (which is well known to hold in spite of the overall nonlocal nature of the theory). The present formulation of locality is only applied to a wider set of probabilities (Selleri propensities 1995, pag. 809).

As told before, the meeting between Selleri and Popper took place in Bari, on May 1983.

In Freire’s interview, Selleri explained how the conference in Bari was thought with the clear purpose of having Popper, which maybe had also a role, according to Selleri’s words here below, in Selleri’s interest for Special Relativity Theory. Selleri stressed the importance of Popper as the most influential scholar in the framework of the Philosophy of Science of 20th century, also underlining their disagreement about the philosophy behind the EPR paradox:

[...] the Bari conference was organized at the time to attract Karl Popper whom I understood then to be a very important philosopher for us, because Karl was very critical of the Copenhagen approach. And by the way, he was also critical of special relativity and he was also critical of some developments of statistical thermodynamics and I think that every time he was critical he was right — which is not like saying that his philosophy was all correct, I do not know, I am not philosopher. Sometimes I find it complicated. Instead as a critic of the foundations of modern physics he is a genius in my opinion. He has done something fantastic - because he always could take the point and see exactly what was the difficulty. Eventually he told me, “Franco, what do you think of the theory of special relativity?” and I gave a vague answer because I did not have clear ideas (the episode was before I started doing research on the foundations of relativity) and he was very unhappy with me because of that. That was in the ‘80s, perhaps in 1985. In 1982⁷³⁴ he had come to Bari. He arrived at the airport, and it so happens that the day before I had received the first copies of my first book. My first book was entitled “Die Debatte um die Quantentheorie” [...].

[...] I went to take him at the airport, and I gave him a copy of the book. The following morning there was the conference starting. He arrived at the conference with my book in his hand and started saying, “I have here a very nice book by Professor Selleri that I have read during the night and in my opinion it is basically right.”

He was a very, very kind person. I had prepared for that conference a talk. You see I knew that Karl Popper had difficulties in understanding the philosophy of the Einstein-Podolsky-Rosen paradox.

So I prepared a talk to explain to him in the easiest possible way what was really the essence of the paradox. He did not like it at all. He said later, “You did not give a nice talk. That was an academic type of talk.” To me it did not look so because I had even prepared little dots, red and blue, to show the elements of reality and so on. But anyway after the Bari conference with Karl there were longer contacts also with exchanges of letters. I went to visit him in London at his house and we had discussions. And we agreed on quantum mechanics very much. It was very important to me meeting Popper, because he wrote whole books critical of quantum mechanics. Also he was a man of fantastic clarity. Surely you have read something by Popper. He obviously has a lifetime of exercising in being clear and he succeeds. I mean his ideas are presented in a fantastically clear way. Also orally when I talked to him he was very clear. He had a very, very attractive personality. In my opinion he was the best philosopher of the 20th century, the best philosopher of science, by far, without comparison (AIP 2003. My underline).

Concerning the conference, Popper wrote to Selleri showing his appreciation:

⁷³⁴ Selleri recalled wrong, or maybe there is some mistake in the interview transcription, because the first letter from Selleri to Popper is dated 5 March 1983 and the conference was held in May 1983.

My dear Franco-Lieber Freund Franco,

It was a wonderful conference, probably the best I ever attended. And for me the best was meeting you, and becoming your friend. [...] I am deeply indebted to you for inviting me (Popper to Selleri, no date-FSPA).

The importance of Bari conference in the relationship between Selleri and Popper, specifically about Popper's experiment, has also been underlined by (Baracca, Bergia, Del Santo 2017):

In his introductory talk at this conference, Popper presented a new variant of the EPR experiment that can decide between a realistic interpretation and Copenhagen [...]. The same experiment was actually already published [...], but in Bari the author was directly addressing the community of the physicists concerned with the interpretations of quantum mechanics, and explicitly asked them for an experimental realization of his proposed experiment (Baracca, Bergia, Del Santo 2017, pag. 76. My underline)

How emerges from some letters, Selleri drew Popper to Bari also considering the opportunity to find people for Popper's experiment. So the expectations of Popper found a support by Selleri:

Dear Prof. Popper, I read with great interest your proposed experiment and think that it can be done. Some experimentalists will be in Bari during the meeting and perhaps we will find somebody who does it (Selleri to Popper, 8 April 1983-Karl R. Popper papers, [Box 247, Folder 18], Hoover Institution Archives).

Dear prof. Popper,...some experimentalists are coming to see if they are able to do your experiments, dr. De Martini from Rome is one of them (Selleri to Popper, 22 April 1983-Karl R. Popper papers, [Box 247, Folder 18], Hoover Institution Archives).

Popper was finding an opportunity for his experiment⁷³⁵ and the two scholars exchanged an intensive correspondence on this.

Selleri truly tried to perform the Popper experiment, albeit the difficulties he met from the beginning. Looking at some correspondence he always updated the Austrian philosopher:

⁷³⁵ Flavio del Santo published several papers on the matter.

See <https://scholar.google.it/citations?user=RNDI9ygAAAAJ&hl=en>. Accessed 24 September 2020.

Augusto Garuccio and I are now working at your experiment-proposal. There are troubles with the source: positronium annihilation gives rise to collinear γ'_+ but with such a high energy that it is impossible to do slit-diffraction experiments. The Aspect source is a beam of excited Ca-atoms and the three body decay $Ca^* \rightarrow Ca + \gamma + \gamma$ does not guarantee γ collinearity. Perhaps one could use Mössbauer effect (recoillest emission) which is possible also with cascades, but a piece of solid Ca would not let one of the photons pass through. Perhaps an atomic deposit of Ca on some other transparent crystal would help in solving the problem. Of course, we work theoretically.

I went to visit De Martini in Rome but was deceived from his way of doing physics (Selleri to Popper, 19 September 1983-Karl R. Popper papers, [Box 247, Folder 18], Hoover Institution Archives).

I was hoping to see you in Athens, but we could only hear a paper of yours read from the young and brilliant Chris Dewdney. During my talk I discussed your proposed experiment from the point of view of Heisenberg relations applied to the source, since there are many physicists saying that your experiment is in principle impossibile because the collinearity requirement cannot in principle be satisfied. I consider such opinions wrong and I think I proved them so. This argument was presented in Athens and will be contained in a paper I am preparing, which shows that no difficulty exists in principle against your experiment (Popper to Selleri, 5 October 1984-Karl R. Popper papers, [Box 562, Folder 19], Hoover Institution Archives. My underline).

With respect to Popper's experiment, Selleri also debated with other scholars.

Tullio Regge⁷³⁶ wrote to Selleri after reading the Bari proceedings:

I write to you informally after reading Popper's proposal in your report "Open questions...". Popper's idea finds me completely dissenting and not because of his opposition to Heisenberg, I'm not a fanatic of the QM and I follow the controversies and the developments always hoping that something will renew or send the Copenhagen school goind down and opening new horizons.

I believe instead that the classic nature of the emitter is mistakenly assumed in the discussion. This is still subject to uncertainty [...].

[...] therefore I do not see a principle of uncertainty violated in indirect measures but a direct statement about the emitter.

⁷³⁶ Tullio Eugenio Regge (1931-2014): See: https://en.wikipedia.org/wiki/Tullio_Regge; <https://www.sif.it/riviste/sif/sag/ricordo/regge>; <https://www.treccani.it/enciclopedia/tullio-eugenio-regge/>. All accessed 24 September 2020.

I therefore do not believe in Popper and do not accept his proposals or conclusions. On the other hand I do not trust philosophers⁷³⁷ (Tullio Regge to Selleri, 14 December 1983. FSPA. Underline in the original text).

Selleri answered to Regge, talking about the Popper's experiment, which is feasible in principle, but also endorsing his own research, considered better than Popper's idea:

[...] Popper's experiment is in principle possible: philosophers do not always say bestiality. Practical difficulties remain in finding the right source.

[...] let me remind you that in recent times here in Bari we have obtained results on the dualism and the theorem of Einstein, Podolsky and Rosen, which to me seem even sharper and more significant than those of Popper. In particular we know why like Aspect-like experiments do not allow to conclude against the separable reality⁷³⁸ (Selleri to Regge, 20 January 1984. FSPA. My underline).

Popper's experiment went not well. Among several disagreement, Selleri and Bedford published a paper, "On Popper's New EPR Experiment"⁷³⁹, which explained the reasons behind their rejection of the experiment, due to its unfeasibility.

A letter sent by Selleri to Popper emphasized the deep approach by the former and his regret for the conclusion of the matter. He sent to Popper the preprint of the paper, which will be sent to *Lettere al Nuovo Cimento* on January 1985 and published on April 1985:

The enclosed paper is the result of a collaboration with Don Bedford, a nice canadian who works in South Africa and who spent some time here in Bari during a sabbatical leave. It is a development of those thoughts which I presented in Athens and about which I wrote you already last October. It is also a dialectical development in the sense that after long discussions Bedford and I concluded that your experiment is impossible in principle (that is, given your interpretation of Heisenberg relations) in all those cases in which the emitting source disappears in creating the "collinear" pair; an example of this is $e^+ e^-$ annihilating in two gamma rays.

⁷³⁷ The Italian original text is in Addendum 3, p. 1027.

⁷³⁸ The Italian original text is in Addendum 3, p. 1027.

⁷³⁹ Bedford D., Selleri F. (1985), "On Popper's New EPR Experiment", *Lettere al Nuovo Cimento*, 42 (7), pp. 325-328. (Bedford, Selleri 1985) in the following.

If the source does not disappear (atomic emission) the situation is very different and the experiment would be possible if it were possible to find collinear emissions.

However two-particle emission without the disappearance of the emitting system is a three body final state and the final momentum is divided in different ways between the final bodies, so that between any two of them there is not, in practice, a fixed correlation. I am afraid that the experiment becomes here impossible in practice, but perhaps I am wrong.

I am sorry of this somewhat negative conclusions, I like your way of thinking very much, but of course truth comes first! (Selleri to Popper, 12 December 1984-Karl R. Popper papers, [Box 562, Folder 19], Hoover Institution Archives. My underline).

In a slide showing the experiment, there are some Selleri's comments. Specifically: "large perturbation of p , lack of collinearity→Experiment impossible in principle" (Selleri to Popper, 12 December 1984-Karl R. Popper papers, [Box 562, Folder 19], Hoover Institution Archives).

In another letter, Selleri wishes to explain more the decision taken with Bedford:

[...] what Bedford and I think to have shown is that your interpretation of Heisenberg's relations doesn't allow your EPR experiment to be made. That is if we are not wrong. Also, our conclusions hold only for those cases in which the source disappears in living rise to the pair of particles. In the other cases like Aspect's experiment you have a three-body final state and the two interesting particles are not collinear anyway (Selleri to Popper, 31 January 1985-Karl R. Popper papers, [Box 562, Folder 19], Hoover Institution Archives).

Finally, some excerpts taken from the abovementioned paper by Bedford and Selleri are the following:

[...] It is scientifically reasonable to search for particular statistical ensembles to which the standard quantum rules (like Heisenberg relations) do not apply: these rules would instead be applicable to more general statistical ensembles (which might be called "standard quantum ensembles"). Along these lines, Popper has recently proposed what he calls "a new version of the EPR experiment" in which the Copenhagen and statistical interpretations of QM apparently lead to distinguishably different predictions. In this way he hopes, like EPR, to demonstrate the incompleteness of QM, perhaps in a concrete experimental situation:

a particle can possess both position and momentum, in direct conflict with the principle of complementarity upon which the Copenhagen interpretation rests (Bedford, Selleri 1985, pag. 325).

[...] Popper's experiment, if [...] an ideal source existed, would be possible, at least as far as the limitations imposed by the momentum-position Heisenberg relations are concerned.

It happens, however, that two-photon atomic emission is never collinear *in practice*. Perhaps this reflects the action of the angular-momentum/angle uncertainty scatter. We prefer to avoid this discussion, however, given the uncertain status of the latter relations. We merely notice that they might become meaningful precisely if interpreted *à la Popper*, as scatter relations. Apart from this, we do not see any way to rule out *in principle* the possibility that a suitable source of collinear particles (e.g., double Mössbauer effect) might be found. The problem of finding one seems, however, very hard, at best, to solve in practice.

Finally, we should comment on the relationship between Popper's proposal and the original EPR, in so far as the analysis above might be taken to have some bearing on the latter. EPR did *not* consider a situation in which the correlation between pairs of separated particles was the result of an evolution from a prior situation in which the pair was localized in some region of space. They considered a two-particle state [...].

In this state the particles do have the position and momentum correlations required for their classic argument against the completeness of QM. But this state cannot result from an evolution of the kind (emission from a localized source) required by Popper for his experiment to be performed, as is shown (Bedford, Selleri 1985, pag. 328).

2.8.5.3- WRITING BOOKS⁷⁴⁰ AND RECEIVING GOOD REVIEWS

There are 26 books written and edited by Selleri, and published in different languages. Among them, some have reached a significant number of copies sold, as for instance *Che cos'è l'energia*, which accomplished in six years the third edition and twenty thousand copies sold (See Addendum 1). Several have been reviewed by various newspapers and journals. As declared by Selleri himself, his best book is undoubtedly, *Die Debatte um den Quantentheorie*, which was then translated in seven languages (namely, Japanese, Spanish, French, Greek, Italian, English, and Polish).

⁷⁴⁰ See Bibliography and Addendum 1 for all book specifications.

As regards the early German edition, Selleri received an impressive letter of appreciation by John Archibald Wheeler:

Dear Professor Selleri, Professor Roman Sexl did me the honor and gave me the great pleasure of presenting me with a copy of your *Die Debatte* [...].

Last night I finished reading it and want to thank you for the service you have done the whole community by this study of the leaders, their ideas and where the subject has gone since the golden days. Among points I found especially illuminating or interesting or both are Jordan's position on the existence of an external world (page 29); the positions of Christian Bohr and Niels Bohr on teleology (page 116); Einstein's view of positivism (page 114); the references to William James (whom Bohr loved to read) (page 121); the ideas of our wonderful colleague, Eugene Wigner, with those position on consciousness, however, I do not always agree (page 123); Einstein's letter to born, about what he called a spooky action at a distance (page 136); and what could perhaps be called "Dirac's defeatism" (pages 136-137). Congratulations and warm good wishes (John Archibald Wheeler to Selleri, 6 February 1984-FSPA. My underline and *Italics*).

Selleri answered 17 days after, still introducing the far-reaching stay in Nebraska, and his research, including kaons:

It was for me a great surprise and pleasure to receive your letter of Feb 6 with the kind words about my book *Die Debatte um die Quantentheorie*. **I have been working hard for many years on this kind of problems starting essentially in 1971 when a year spent at the university of Nebraska gave me the opportunity to read the original papers on quantum theory.** The idea to publish the book first in german came from contacts with Roman Sexl who has done so much to help me in this enterprise.

I am also grateful for your very interesting papers: some ideas I have used in my teaching of QM here in Bari, like your example of the game of the twenty questions for understanding the Copenhagen relationship between quantum physics and reality: a really illuminating idea, thank you for letting me know about it!

I take the freedom to send you some of my recent papers: the sense of this research is that the difference between realism and other philosophical approaches such as positivism is not merely philosophical but physical.

Einstein's elements of reality, taken seriously, lead to empirical consequences which contradict some well established "truths" in domains in which no experiments are available yet: this is the case, for instance, of the KK-pairs produced in electron-positron collisions. Also the idea of "empty waves" shows that new empirical predictions follow from it, which can prove this idea wrong if the experiments are carried out.

If these experiments turned instead out to be compatible with the idea, this would mean that a space-time description of elementary phenomena would become possible, in spite of all which has been said against it. Of course there are difficult “sociological” problems in convincing experimentalists to devote their time to such problems, but optimistically something is starting to move.

Last point, I believe that the Orsay experiments do not settle the problem of nonlocality because of the incredible superficiality both of data handling and of conceptual analysis: one of the papers deals with this matter. Please write me again if you fell like doing so: it is for me a great honor to exchange views with a physicist of your level and fame (Selleri to John Archibald Wheeler, 23 February 1984. Underline in the original text. My *Italics* and **bold**).

Selleri asked Popper to write a Preface for the Spanish, French, and Greek version of *Die Debatte um die Quantentheorie*, although in an early letter Selleri asked for Spanish, Greek and English version. But the latter was the last to be completed, being published only in 1990, seven years after the early German version, too late for applying the Popper preface, a little bit out of date. Besides the fact that English version was very different by the original. In a letter of October 1984, Selleri wrote to Popper in order to have a Preface for his book:

The editor of the spanish edition have given a positive answer for the publication of Die Debatte um die Quantentheorie.

[...] as I told you some time ago, I would be extremely grateful if you could write a preface, which I could use immediately for the spanish translation of the german book. I know that I am giving you additional troubles which add to your many activities and I am sorry for that. Perhaps you could simply allow me to use the words you spent for *Die Debatte* at the Bari workshop (Selleri to Popper, 5 October 1984-Karl R. Popper papers, [Box 562, Folder 19], Hoover Institution Archives. Underline in the original text. My *Italics*).

Popper answered only 23 days later, attaching his preface, which is full of adjustments in the original, being perhaps a sign of a troubled genesis:

My dear Franco, I just sent a preface for your book. I estimate it to be 2 ½ pages long. I do not think that it is good. If you don't like it, we just throw it away.

If you like it but wish to clearage it, just make your proposals or suggestions [...] (Popper to Selleri, 28 October 1984-Karl R. Popper papers, [Box 253, Folder 1], Hoover Institution Archives)

Selleri appreciated a lot and asked the permission to use it in the Spanish, Greek and English version, as told before:

Warm thanks for the nice Preface you wrote for my book. It is all right as it is and I have no changes to propose.

With your permission, it will be used for the Greek, English and Spanish editions, which will probably appear in that order.

The second German edition is printed already, we cannot use it there. I am very grateful for your writing this preface which will be very helpful for spreading realistic ideas (Selleri to Popper, 18 November 1984-Karl R. Popper papers, [Box 348, Folder 21], Hoover Institution Archives)

The right version which used Popper preface are in the following letter:

My book ("Die Debatte...") has now been published with your preface in french by Flammarion. It is a very nice book, probably better than the german one. Also the greek and the spanish edition, which will appear soon, incorporate your preface. Thank you very much once more for writing it, I am sure that it will contribute strongly to the success of the book. I could not include your preface in the Italian and in the English books because they are very different from the one you read (Selleri to Popper, 22 October 1986-Karl R. Popper papers, [Box 562, Folder 19], Hoover Institution Archives).

Selleri also showed pragmatism to achieve something, as when he wrote, approximately ten years later, a letter to Ludwik Kostro in order to obtain the Polish preface of his book,:

[...] thanks for the message and the good news about my book in Polish. The title you choose is fine with me. Instead, I'd rather avoid using Popper's preface. After all, it is not a big deal, and then he is gone and I would not want to abuse his words. Could you write a presentation for the Polish public? I would like to⁷⁴¹ (Selleri to Kostro, 10 December 1997-FSPA. My underline).

⁷⁴¹ "[...] grazie del messaggio e delle buone notizie sul mio libro in polacco. Il titolo che scegli a me va bene. Invece preferirei evitare di usare la prefazione di Popper. In fondo non è un gran che, e poi lui non c'è più e non

It is not so gratifying to say that Popper's foreword was not a great one, even though he then respected his disappearance by trying not to abuse his words, which could also have meant an added value, but that it would have been a speculation.

Selleri collected in an handmade booklet (which contains several excerpts taken from newspapers, journals, and so on) lots of reviews related to *Quantum paradoxes and Physical Reality*, the English 1990 version of *Die Debatte um den Quantentheorie*⁷⁴², which was also proposed for the Lakatos Award⁷⁴³ of the London School of Economics (LSE) for the year 1991, without winning it, though. Several scholars endorsed the book for the award. For instance, A.A. Tyapkin, Chief of Physical Department at Joint Institute for Nuclear Research at Moscow at the time, endorsing the book, stated that:

With full enthusiasm I support proposal for the Lakatos award of professor Franco Selleri.

[...] his book [...] was great achievement in philosophy of science of the last time. In this book author gives deep critical consideration of the very important scientific problem about development of interpretation of the quantum mechanics.

My purpose [...] is to emphasize that the critical consideration of all problem and scientific conclusions from analysis in Selleri's book are bright example of employment of Imre Lakatos method of reconstruction of scientific development.

Actually, philosophical significance of the Selleri's monograph consist in substantiation of the objective status of quantum mechanics on the base of new understanding of the wave-particle duality. Quite new was the conception of the de Broglie's wave as "empty" wave, wave without energy. This idea was expressed by Selleri in article in 1969. Then he has proposed an original experiment to find this wave on basis of use of the quantum amplifier ("On the observability of quantum waves"). Carrying out this experiment will be fundamental significance for physics and the theory of knowledge.

Selleri's book convince of reader that is why along with investigations carried out at the leading edge of theoretical physics where the majority of theoretical physicists are working. The investigations is needed in the deep rear of theoretical physics aimed at eliminating the lack of understanding of the earlier established laws and at searching for more complete understanding of these phenomena.

vorrei abusare delle sue parole. Potresti scrivere tu una presentazione per il pubblico polacco? A me farebbe piacere" (Selleri to Kostro, 10 December 1997-FSPA. My underline).

⁷⁴² I submit the contents of *Quantum Paradoxes and Physical Reality* in the Addendum 3, pp. 1027-1029.

⁷⁴³ Lakatos Award: See <https://www.lse.ac.uk/philosophy/lakatos-award/>. Accessed 24 September 2020.

Then physical theory will become maximally clear and vivid after obvious presenting absolutely different aspects of quantum separable reality, microscopic particle with real energy and real wave but without free-energy. This conception of professor Franco Selleri have foundational significance for philosophy of science (FSPA).

Going back to the review, I am enclosing below some excerpt belonging to the above booklet with the references on where they have been published:

- *Quantum Paradoxes and Physical Reality* [...] is an excellent introduction to this subject and an authoritative guide through its precarious intricacies. [...] It has the didactic advantage of leading the reader, step by step, to a deeper understanding of the problem discussed in order to reach soon the level of sophisticated critical scholarship. [...] Selleri has written with rigor and clarity a superb book which is understandable and informative for every one interested in quantum mechanics and which is stimulating and thought-provoking also for those who do not share the author's philosophical conviction (Max Jammer, *Foundations of Physics* 21, 1335 (1991)).
- In this work Professor Franco Selleri of the University of Bari turns in a remarkable virtuoso performance in which historically and philosophically sensitive discussions, on key issues surrounding the debate between quantum mechanics and local realism, accompanies a careful and thorough presentation of the requisite technical details. [...] This shows a commitment, in the grand tradition, to the unity of human knowledge (James Cushing, *American Journal of Physics* 58, 797 (1990)).
- Here is a book I have been waiting a long time for, a sympathetic rendering of the reasons why a small band of physicists believes in various heresies such as that quantum theory is incomplete and that there must be a causal interpretation of quantum phenomena. [...] Professor Selleri is almost uniquely qualified for this task - a true skeptic who nonetheless has a deep grasp of the "Copenhagen interpretation". If you add to this an unflagging intellectual honesty and a basic sense of fairness that is very evident throughout this book, you can appreciate what a special document it is. [...] From my own perspective, as one who believes that the standard theory [...] offers an easier way to view phenomena than the causal position, I am nonetheless deeply impressed by the depth of scholarship that Selleri has shown in presenting his alternative view. [...] I warmly recommend the book to anyone who is bothered by quantum theory, whether he accepts the standard theory or not (Daniel Greenberger, *Physics Essays* 4, 436 (1991)).
- Selleri has also achieved a part of what Einstein had in mind: to find a possibility to conceive causal and real processes as the foundations of statistical quantum theory without falling into logical contradictions. [...] Those who believe that, besides the beaten paths in quantum physics, there are no other ways that promise to produce exciting results will change their minds after reading Selleri's book (translated from German) (Erwin Kerkenberg, *Journal for General Philosophy of Science* 22, 177 (1991)).

- Declared partisan of “physical realism”, but impartially open to other options, expert on the interpretative problems of quantum mechanics, and author of constructive discussions and suggestions, Franco Selleri embarks in this book on a grand exploration that will be read with interest and profit by every engaged specialist and every curious spectator.
[...] I *strongly recommend* the reading of Selleri’s book, not only because of its technical interest, but also for seeing how the *inevitable* perplexities of any other quantum theorist are viewed by a declared partisan of realism (translated from French) (Olivier Costa de Beauregard, *Ann. Fond. L. de Broglie* 15, 239 (1991)).
- [...] The author pursues his research with great care and painstaking caution, striving to discover an approach more satisfying than the Copenhagen brand. On reading these pages one is overcome with the impression that the Copenhagen interpretation is not the only possible one. [...] although I personally do not agree with Selleri’s choices, I do believe that this book makes us understand the essence of quantum mechanics, an understanding that is never available in regular textbooks of physics (translated from Japanese) (Mikio Namiki, *Butsuri* 53, 795 (1991)).
- [...] Franco Selleri provides a perceptive and critical discussion of quantum paradoxes from the vantage point of recent advances. [...] Great care has been taken in the book to represent faithfully opinions from different perspectives. The persuasive style of the book is contagious and seems justified by the need to convey the excitement of research in this field to a general audience. And this sense of excitement is not achieved at the expense of accuracy. The work described here is a fascinating story, documented with meticulous details, of human inquiry into some of the most intriguing fundamental questions of science (Dipankar Home, *2001*, November 1990, 37 (1990)).
- [...] This book is a tour de force in the thorough way it reviews the fundamental problems of quantum physics, with a welcome attention paid to the principles and detail of many of the key experiments that marked the progress of our understanding of the subject. Some of these have been largely forgotten over the years. [...] This volume is a major contribution to the literature on the foundations of quantum physics. [...] It should be an important reference book in the field for years to come (Alastair I.M. Rae, *Foundations of Physics* 23, 133 (1993)) (FSPA).

I am aware that, as for instance in the case of an obituary, a review may historically be an unreliable document, too inclined to highlight positive things. But, unlikely the former, a review could decide of the future of a book, the book author is still alive, and the reviewer puts himself at stake in making judgments that can be challenged and disregarded, which would be to his/her detriment. So I am inclined to confirm how the above mentioned reviews are a further evidence of the probative and all-round contribution of Selleri to the debate within the scientific community, also revealing his

impartiality openness to explain the other options, his fairness and technical accuracy.

Selleri collected also other reviews on newspapers and journals, introduced “as a cross section of published opinions on the original version, or forerunner, of the present book” (book presentation brochure, probably edited by the publishing house, Kluwer Academic Publishers-FSPA). The dates of the reviews are missing, though:

- Even though he is himself opposed to the ‘Copenhagen’ interpretation that Bohr was able to impose, Franco Selleri is able to explain, without factiousness and in simple terms, its essential features - and this is no easy task. He has clarified Einstein’s criticisms and Bohr’s answers (*Le Monde*-French newspaper).
- I therefore find it exceptionally meritorious that Franco Selleri in his book [...] once more takes up the old question of the meaning of quantum theory (Hans Peter Dürr, *Spektrum der Wissenschaft*-German magazine).
- I think Selleri dispenses justice to some commonplaces that have too hastily been advanced in order to prove the ineluctability of the positions advocated by Bohr and Heisenberg. [...] It is a remarkable book, whose reading is advisable for our colleagues, in the first place. In fact it offers a chance to those engaged in the daily business of research to gain a true and knowledgeable understanding of our activities as physicists. Moreover, our experimental colleagues should take seriously the experiments proposed by Selleri (Bruno Carazza, in *Il Nuovo Saggiatore*-Bulletin of the Italian Physical Society).
- The book by Professor Franco Selleri, which I read when it appeared in its first German edition, has not only a great historical importance but also a great charme (Karl Popper, in the Preface to the French edition).
- For philosophers the greatest advantage to be gained from Selleri’s book is its potential to arouse them from a dogmatic repetition of platitudes misjudged to be of critical importance (Francesco Barone, in *La Stampa*-Italian newspaper).
- The history of the EPR argument has been told many times. However, the presentation of this author stretches the mind. [...] deftly written, the book allows for easy reading (*Science et Avenir*-French magazine).
- Also this is Selleri’s merit - to have shown us, whether the story will end the way he wishes or not, that the idea that everything in physics is decided by calculations and experiments is only a philosophical umbrella in bad shape (Marcello Pera, in *Nuova Scienza*-Italian magazine).

- The popularization of theoretical physics is beyond doubt one of the most difficult enterprises. Franco Selleri lately tried to do that with an agile volume, in which he spares the reader those technical developments that necessarily have played a dominant role in other works of his. The result is stimulating (*Il Corriere della Sera*-Italian newspaper).

Selleri received good reviews also for others book, written and edited, as, for instance, *Open questions in quantum physics*, basically the Proceedings of Bari 1983 conference; and *The wave-particle dualism*, but I thought it could be enough enclosing only the foregoing ones.

As regards *The wave-particle dualism*, Selleri wrote an e-mail to Tobias Schwaibold at Springer (currently undated in my research), in order to a list of his books to be saved, perhaps for the end of the rights acquired by the old publishing houses and which Springer maybe wanted to reissue. Selleri clarified a curious duality on two books concerning...Duality!

[...] The reason why [WAVE-PARTICLE DUALITY (Plenum, 1992)] is not in the Springer list of books to be saved is probably due to the strong similarity of the title with another one of my books, THE WAVE-PARTICLE DUALISM (Reidel, 1984). Let me stress that “dualism” implies an irreducible antagonism of two parts of a system, while “duality” simply refers to the double nature of something composed of two parts, elements or principles. In physics these meanings hold too, so that the Reidel book focuses on the orthodox (or Copenhagen) point of view, while the Plenum book describes the contemporary research and its different starting points. For this reason I prefer the Plenum book and I suggest that you substitute it to the Reidel book. Maybe you had other reasons for choosing the Reidel book: in such a case, of course, I am ready to drop my request and to respect your choice (Selleri to Tobias Schwaibold-FSPA. My underline).

Selleri's clarity in writing papers and books created a huge number of passionate, curious, strange characters, together with school teachers of all levels, which during the years wrote to him hundreds of letters and emails in order to ask for his works, and for opinions on their ideas on physics related issues. And Selleri really tried to answer to all of them, also being harsh with some arrogant people, and selective with those who did not think he could stand the speech.

But these last approaches happened always after a series of letters or emails in which initially he was always very helpful and willing to explain and make people understand.

Even from these pieces of information on the reviews of his books and the letters of those who read them, the role and interest that Selleri's research created in the scientific community is tangible.

2.8.5.4-DISSEMINATION ACTIVITY

Selleri has always considered to be his precise mission the divulgation of science-related issues, which also could flows into the social and political field. I have already mentioned several examples so far and in Addendum 1 there is a huge number of articles on newspapers. Here, thanks to Michele Barone, I mentioned some notes, ideas and correspondence concerning a completely unreleased book on Galileo, Giordano Bruno, and the numerous viciousness of the Church over the centuries, which included many repressed or killed characters, and several notes written about their life and belief.

Nome	Data	Nota	Numero
La chiesa cattolica in crisi			1
La Riforma			2
Il Concilio di Trento (1545-1563)			3
La Controriforma			4
La Inquisizione (1564-1804)			5
La Inquisizione			6
1577	1577		7
1578	1578		8
1579	1579		9
1580	1580		10
1581	1581		11
1582	1582		12
1583	1583		13
1584	1584		14
1585	1585		15
1586	1586		16
1587	1587		17
1588	1588		18
1589	1589		19
1590	1590		20
1591	1591		21
1592	1592		22
1593	1593		23
1594	1594		24
1595	1595		25
1596	1596		26
1597	1597		27
1598	1598		28
1599	1599		29
1600	1600		30
1601	1601		31
1602	1602		32
1603	1603		33
1604	1604		34
1605	1605		35
1606	1606		36
1607	1607		37
1608	1608		38
1609	1609		39
1610	1610		40
1611	1611		41
1612	1612		42
1613	1613		43
1614	1614		44
1615	1615		45
1616	1616		46
1617	1617		47
1618	1618		48
1619	1619		49
1620	1620		50

(draft handwritten by Selleri on the characters to be included in the book on the persecuted by the Church-Courtesy of Michele Barone-MBPA, Athens)

In a letter to Umberto Bartocci (a mathematician at Perugia University)⁷⁴⁴, Selleri stressed how, during some research for a book on the Renaissance (whose writing and publication is totally unknown), he found the harsh reaction of the Church to the ideals of the Renaissance. Probably his idea on a book on the prosecuted by the Church could be born here:

I am now writing a book on the Copernican revolution for Laterza. I have studied a lot and I am impressed by the intensity of the clerical reaction to the Renaissance. Philosophers and scientists were investigated, questioned, censured, tried, convicted, imprisoned, tortured, executed. I did not know that the phenomenon was so vast, usually we only talk about Galileo and Bruno! In my opinion the real martyr was Campanella. It is true that Bruno was burnt alive and he was not, but the number, the duration and the kind of torture he underwent surpasses all imagination. So much so that he converted (in good faith) to Catholic truth as a result of torture! No high school book tells these things, but there are two diverse Campanella, the one before and the one after the tortures: even La città del Sole is later. That the second one is a bit an asshole and reactionary should not really be surprising, given the treatment. It's the first one that counts⁷⁴⁵ (Selleri to Umberto Bartocci-FSPA. Underline in the text).

In another letter, three years later, he collected some bibliography for this project:

Here then is a bit of bibliography:

E. Garin, *L'Umanesimo italiano*, Laterza (1990)

E. Garin, *Medioevo e Rinascimento*, Laterza (1984)

E. Garin, *Scienza e vita civile nel Rinascimento italiano*, Laterza (1985)

E. Garin, *Lo zodiaco della vita*, Laterza (1982)

P. Redondi, *Galileo eretico*, Einaudi (1988)

M. Ciliberto, *Giordano Bruno*, Laterza (1990)

G. La Porta, *Giordano Bruno*, Bompiani (1988)

S.F. Mason, *Storia delle scienze della natura*, Feltrinelli

E. Namer, *La vie et l'oeuvre de J.C. Vanini*.

Information about the “repressed” by Holy Mother Church is scattered over a vast literature largely unknown even to me.

⁷⁴⁴ The date of the letter is unknown, probably prior to 1993, because of some information contained in the rest of the letter, which is not relevant here and therefore not reported.

⁷⁴⁵ The Italian original text is in Addendum 3, p. 1029

In fact, I would still be grateful for additional information not contained in my notebook⁷⁴⁶ (Selleri to Rocco Trigiante, 29 November 1996-FSPA).

The presence and discovery of this notebook will be definitely very intriguing.

Selleri has always been clearly communist, atheist and strongly anticlerical, even though he has spoken with many Church representatives in some meetings, has published with the Jaca Book publishing house and has also attended the Rimini Meeting⁷⁴⁷, the latter two always considered (perhaps even in a simplistic key) as being linked to the positions of the Church or, in any case, of the Centre parties, and certainly not places deputies of the Left. In this regard, Vincenzo Paticchio stated that:

I do not understand why he went with Jaca book, and in Rimini with Comunione e Liberazione [CL]⁷⁴⁸. There are these strange things, these contradictions, never all black and white. Anyway, this activity was important too, because in this way Franco broke with the rigid vision of CL (Interview with Vincenzo Paticchio, 29 May 2019).

On 30 October 1989, Selleri wrote a letter concerning the Rimini Meeting:

[...] this is not good. [...] I took part [...] in the round table substantially dedicated to Halton Arp, a name that posterity will remember more than any other astrophysicist of our century. The spirit of confrontation between different cultural and philosophical positions that exists at the Meeting, seems to me progressively obscured by an increasingly marked political instrumentalization, and not so much for what one wishes to enhance in a positive way, since fortunately everyone is free to choose the friends he or she wants, but for the closures and anathemas that are launched on characters, parties, and categories. That the largest mass youth movement of our day lends itself so much is certainly a sign of the serious political crisis we are going through [...] ⁷⁴⁹ (FSPA).

⁷⁴⁶ The Italian original text is in Addendum 3, pp. 1029-1030.

⁷⁴⁷ Rimini Meeting: It is the former Meeting dell'Amicizia di Rimini.

See <https://www.meetingrimini.org/en/what-is-the-rimini-meeting/>

⁷⁴⁸ Comunione e Liberazione is an Italian Catholic movement founded in 1954 by Fr. Luigi Giussani. The official name is Fraternità di Comunione e Liberazione (Fraternity of Communion and Liberation). Its aim is to present the Christian event in a way which is in tune with contemporary culture, making it a source of new values for the modern world. The name Communion and Liberation first appeared in 1969 and it synthesizes the conviction that the Christian event, lived in *communion*, is the foundation of man's authentic *liberation*. See <https://it.clonline.org/cl>; https://en.wikipedia.org/wiki/Communion_and_Liberation. Both accessed 24 September 2020.

⁷⁴⁹ The Italian original text is in Addendum 3, p. 1030.

According to Michele Barone, there is also another unpublished book that, however, could come out posthumously.

The book would be completed, and it would also have a title, *The Greek Science-Elliniki epistimi*. But the Greek publisher Kostarakis, who also published the posthumous version of Selleri's book on relativity, encountered several difficulties, also linked to the enormous Greek economic crisis of the last decade, sadly known in the chronicles. And anyway there is a contract between the publisher and the family and therefore it is not viewable.

Moreover, I found the clipping of *Le Monde* of Wednesday 24 October 1979 which hosted an article by Selleri, "Franco Selleri: une réaction irrationnelle après d'autres", who, talking about his attendance at a congress⁷⁵⁰, enters the debate among physicists on the social history of the discipline, underscoring his disagreement with the animist and spiritualist positions inherent in modern physics, with notable figures such as Heisenberg, Jordan, Pauli, Eddington, who have marked modern physics with very heavy irrationalistic statements.

In Selleri's introduction made by the newspaper, it is worthy stressing how he is remembered as "author on the other hand of the discovery of infinite inequalities deriving from Einstein's principle of locality"⁷⁵¹.

Selleri has also been very involved in seminars, books and meetings in order to counteract obscurantism and anti-scientific trends, which are also inherent in modern theoretical physics, with links between physicists and pseudosciences (see Addendum 1-Against obscurantism trends). Some examples are: "Scienza e potere in 1984", "Ma l'oroscopo serve a qualcosa?"⁷⁵². Another paper, "I pericoli del movimento oscurantista"⁷⁵³, is mentioned in a letter in advance to Massimo Polidoro, at CICAP.

⁷⁵⁰ Perhaps Selleri deals with SCIENCE ET CONSCIENCE, held in Cordoba on 1-5 October 1979 and organized by France Culture.

⁷⁵¹ "auteur d'autre part de la decouverte d'inégalités infinies derivant du principe de localité d'Einstein" (*Le Monde*, 24 October 1979).

⁷⁵² Selleri F. (1984), "Scienza e potere in 1984", in *Ideologia Linguaggio Potere: 1984 di George Orwell*, Pantaleo N. (ed), Università di Bari: TEUTH, pp. 91-106; Selleri F. (1995), "Ma l'oroscopo serve a qualcosa?", *Nuovi Orientamenti*, XVII (75), pp. 36-38.

⁷⁵³ Selleri F. (1999), "I pericoli del movimento oscurantista", *Scienza & Paranormale*, VII (27), pp.34-39.

Polidoro has written to Selleri in order to thank him for a foreword to a book published by Dedalo Edizioni, a small but active publishing house in Bari:

I would also like to thank you on behalf of CICAP for mentioning the committee in the preface to Hy Ruchlis' excellent book. There is certainly a great need in our country for critical contributions like this, especially if they are specifically addressed to youth. [...] we would be very pleased, if possible, to host a speech by you on the pages of *Scienza & Paranormale*⁷⁵⁴ (Massimo Polidoro to Selleri, 16 February 1998-FSPA. My *Italics*).

Selleri answered very early, promising the above paper:

The discovery of this author and the decision to translate it into Italian is thanks to the Dedalo Publishing House which has unusual rationalistic inclinations. [...] I will be happy to contribute an article. I would think of preparing something new, for example a review/comment on the denunciation of paranormal activities made by Gerald Holton, one of the greatest living epistemologists, a profound connoisseur and admirer of Einstein's work. It is so rare that a true philosopher of science deals with these things that Holton's ideas deserve, in my opinion, to be brought to the attention of the readers⁷⁵⁵ (Selleri to Massimo Polidoro, 24 February 1998-FSPA).

Everything reported above shows once again the frenetic and all-round activity of Selleri, who really tried to intervene everywhere for a healthy dissemination of science, also mentioning scholars working in this direction, such as Halton Arp, Hy Ruchlis, and Gerard Holton.

2.8.5.5-TRAVEL NOTEBOOK 1989-1990

As last confirmation of the huge activity on several fields held by Selleri for all the 80's, I found in his archive the following notebook. It is extremely rich and full of handwritten annotations and curiosities that Selleri made during his travels for meeting, conferences and other, including many portraits of various scholars met by Selleri and of their intervention at conferences; and also the self-judgments that Selleri gave himself after the seminars held, with the strategies to be adopted for a better dissemination of ideas.

⁷⁵⁴ The Italian original text is in Addendum 3, p. 1030.

⁷⁵⁵ The Italian original text is in Addendum 3, p. 1030.

But it contains also logistic details with publishing houses for his books and for the birth of new magazines proposed by him (or to him); or the count of the money in his own hands during the trip, with the corresponding expenses marked, taxis, food, and so on. And, of course, it is also full of thoughts on the subject studied until now. It goes without saying, that I will try mentioning preferably the latter.

Here I have included some excerpts from Selleri, duly reported (in *Italics*); and some summaries of mine, leaving the format of notes and sketches⁷⁵⁶.

I have used bold in Selleri's statements, in order to highlight something I deemed appropriate. I have made a sweeping selection in the documentary material, so the exhaustiveness of the notebook is far from being reached.

The bulk of the pages (140 out of 154) concern the 1989, a very intense year. 50 pages are only related to a trip to Europe for various seminars lasting 15 days; and another 58 pages for a one-month tour. In 1990 the pages are very few, with very long time intervals.

Since the first day, his rich activity emerges:

12/04/89-Milan, Jaca Book

Jaca Book: comparison on Arp's book advancement work.

workshop organized by Libreria Rinascita (PCI) for the 23/5 at Modena with Arp on a new cosmology.

Selleri has to give an operative answer because Bagnoli [from Jaca Book] seemed really interested in reprinting 4 works: FQM, special relativity, universe without big bang, expanding earth and even for a book with Guaragnella. Also for small books, 4 or 5, in several languages, on diverse fields of physics, 60 large folders, like the prolusions at the College de France, in order to create a passion for the matter.

13/04-Vienna at Thirring & C.

Institut für Theoretische Physik.

Talk "Present status of the epr paradox".

About 30 people, various questions even quite far from the topic. I give myself 6/10 as a vote because the seminar was poorly prepared and little meditated and could have succeeded better.

⁷⁵⁶ The whole Italian text (Selleri's original and mine summaries) are in Addendum 3, pp. 1031-1037.

Thirring

He gave me a place for the short time I was here.

There was no mention of refunds for me.

Flamm

Provocative but off-centre (as usual) questions. He works on the foundations of the q.m. in his own way.

He says that it is hard to work on FQM in Vienna.

He does not quote me in his work.

Bertlmann:

He says he does a lot of things: chromodynamics, Ciulli's maths stuff etc. and that FQM are only a minor interest for him (you can see it from his work, I think). He says that he believes that the additional hypotheses are true. Here maybe I have modified his ideas a little bit.

Visited exhibition dedicated to Hans Thirring, born 101 years ago. Very interesting is the book "AntiNietzsche, AntiSpengler" published in 1947 and, says WT [Walter Thirring], written in Vienna during the war. It is, however, a book in German.

14/04-Vienna at Rauch

At the Atominstitut, a seminar with 14 people with a generally unconvincing air. Rauch himself showed at the end that he had not grasped the spirit of the thing well.

Did Rauch want the seminar on Friday? Yes, it was!

Notice the difference between the seminar at Rauch's years ago (big classroom, lots of people) and this: just because of the discourtesy?

Rauch's message: his new points for deterministic absorption / stochastic absorption are very impressive because "8 neutrons are capable of observably modifying the behaviour of 50 neutrons". If they were corpuscles this would not be possible!

17/04-Monaco (Max Planck)

Seminar: 35-40 people generally attentive. Many questions, during and after.

I got the right tone: no controversy from me, EdB's paradigmatic inferiority confessed, careful and calm answer to all their questions. Much better than in Vienna. The right spirit is to put oneself in their shoes and think about this "strange object" that comes to them to talk about EdB. So you almost have to sympathize with their objections and answer them calmly and logically. Avoid the mistake of arguing polemically against Bohr and/or Heisenberg. Only, there is the logical possibility of EdB and it must be investigated.

All in all, I am happy with the way things went, although we will have to see how far they will go (none?).

Two people said that the seminar was “very interesting”. Compared to Dortmund⁷⁵⁷ it is clear that here that you are good they do not tell you (they think you already know it yourself). This re-evaluates it. A strong positive impact is also very likely in München.

18/04-Monaco (Max-Planck)

I wrote to Gondesén:

*I proposed to him a German translation of *Che cos'è l'energia*. Currently it reached 20 thousand copies sold. Rewritten for 3rd edition, new figures. Perhaps Dürr could present this book in RFT* (*on second thought, better Bonse).*

Prof. Hans-Peter Dürr, director of Max Planck. Very keen interest for global problems, according to K.E.E. At the seminar he was correct in the questions, with some protective vein towards others by EdB-type ideas.

Dr. Ivo Derado, invited me and took care of all the concrete aspects (advertising at the seminar also individual, refund of 450 DM, guest house, car to go to Garching, etc.). He expressed his reservations about my activity.

Afternoon/evening at Arp.

Met his tall, dry, French, nice wife and his little daughter (Delphine, 5 years old, usually called Delina).

He wants Bertola to correct his book in the Italian translation. OK

He wants a foreword by Bertola, or maybe Maffei. He had to choose.

He gave me a copy of an article in Nature

He will come to Modena.

He will come to Rimini.

It sound strange how Selleri wrote he was in Munich two days, 17 and 18 April 1989 and that in the first date he was waiting for a good seminar also in Munich the day after. Maybe on 17 April he was elsewhere.

19/04-Tübingen

Möllenstedt is not here (he had surgery 8 weeks ago and is recovering). Lichte takes care of everything, precise, efficient, unpleasant. He gives me 250 DM of reimbursement.

20-25 people at the seminar, silent, unlike München.

Prof. F. Lenz, he gave good lessons on coherence length. At the seminar a bit hostile with the supplement of a comment that you do not have to get ideas first and then look for experiments, but wait until a theory goes into crisis on its own before discussing it!

⁷⁵⁷ Maybe Vienna, because Selleri's talk in Dortmund was made on 21/04.

Dr. Franz Hasselbach had the courage to come and congratulate himself on the seminar and ask that I send him some work (also on EPR). Nice.

Thomas Lenthner young graduate wants to know more about EPR

Dr. M. Nicklaus, a talented young experimentalist working on the Sagnac effect⁷⁵⁸ for electrons (not yet seen, says Lichte) showed me the formation of interference fringes, magnificent. He gave me his papers. His supervisor is Hasselbach.

21/4/89-by train towards Dortmund

Lessons learned:

- 1) Many people are amazed to see theories at EdB. I must begin the seminar with a hint at overcoming von Neumann's theorem.**
- 2) the work of Aspect, Bell, etc., suggests that there is no more room for local hidden variables models (this was clear in München). Reopen the space by talking in each seminar about strong inequalities.**
- 3) Never present yourself too kindly. Here efficiency and availability are not automatically liked (Derado and especially Lichte). Better a little hard and confident.**

22/4/89

Yesterday's seminar (21/4 Dortmund) went very well. They listened, about 30, in religious silence, taking notes. Comment by Bonse of his colleagues: "the best physics seminary we had". Learning from previous mistakes has helped! Bonse believes that I have widened the space for my speech.

Bonse is a conservative in politics, religious, friendly. He immediately forgave my forgetfulness of the seminary at 8 a.m. (!) and rescheduled it at 3 p.m. Various exchanges of ideas, he does not seem to want to made the experiment with me. He was happy with the success that my seminary (organised by him) had with his colleagues. Bonse was positively prepared for my coming, so much so that in the presentation he said that it was "an honour" for them to have me as a speaker. So he easily overcame my time error. Datta's merit?

This is another lesson to meditate on. Those who are busy want to gain sympathy in their environment, and not the opposite.

⁷⁵⁸ Already in 1989 Selleri was interested in studying the Sagnac effect, a cornerstone in his research on FRT.

Vendite

Wpd (11/83) 638 successfull

Oqqp (12/84) 660 successfull (best)

Mrqf (2/88) 111

NEW JOURNAL (I do not think it came out afterwards. Interesting to see the preparation, with all the logistic details)

Provisional title:

FOUNDATIONS OF QUANTUM MECHANICS, RELATIVITY AND COSMOLOGY

Publisher:

Me, but it would be good if there were 2 more for USA, Far East.

Publications:

Zero cost for the publication of articles. 50 reprints for free. Additional reprints are paid for.

Administration:

It is much better for them to have it in Dordrecht. Articles from authors sent to them, from them to me in Bari, then referee, then back to them if accepted.

Start

*January 1991, with a 1st issue at the end of 1990. material ready for May-June 1990.
Publication November 1990*

Periodicity

Must come out on time bimonthly at the beginning, then early monthly

Editorial board

15 names. Compromise between famous/active names. Geographical distribution: Australia, India, Japan, USSR.

Dynamic structure that really changes every 3 years. Saying right from the start that this will be the case.

Initial composition reflecting the purposes and geographical distribution of the active research.

Free copies

3 free copies / file to the publisher

1 free copy / file to each member of the E.B.

⁷⁵⁹ Currently is Wolters Kluwer N.V.. It is an American Dutch information services company. The company is headquartered in Alphen aan den Rijn, Netherlands, and Philadelphia, United States. Wolters Kluwer in its current form was founded in 1987 with a merger between Kluwer Publishers and Wolters Samsom. It has new fields, quite far from Physics.

Questionnaire

(65% of responses = success)

DL must write a questionnaire to ascertain the desirability of the magazine. I must provide him with 100 names immediately. They must be meaningful names, people who will help us when the magazine comes out. He needs “draft scope”: 100 words on the type of articles we will publish.

Quality

They must be work of great scientific quality and must be selected with a high frequency of waste by the referees. We must and want to receive unsolicited papers.

There must be an active role for me in imposing the first numbers. Kluwer will do “a lot” in investment and advertising.

- 1 invited paper each issue as a review of the results of a certain line.

- great role of the experimentalists.

- relativity and astrophysics.

- a bit of history and philosophy (T.S. Kuhn in the E.B., even if it is a magazine of hard physics)

Initial document

I have to prepare 100-200 words with the great ideas “conferences done”; “because existing magazines are not enough”; “ideas for the next millennium”; “success of FP”. He has to make a report to his superiors.

25/4/89-Bruxelles at Reigner

At the seminar about 10 people: Reigner, Vigier, Brijtebier, Sjödin, De Baere and friend, another 3-4 younger and 1 older Italian working for free at Prigogine.

Vigier leaves before the end because “he has to attend a political meeting against a 350 km French missile”. Reigner asks to photocopy the last slides of the seminar. He seems really interested.

Opus Dei, organised Jews and the Masons are the masters in his university. He and another, he says, are the excuse for democracy. He was invited to talk about QM at a meeting of the Masons but, he says, he has nothing to do with them.

11 e 12/5/89 Rome II University at Carsetti’s workshop “Scientific Model of Physical Reality”.

It is hard to believe that the collection of Copenaghians was not wanted by Carsetti. Impressive homogeneity “on the Right”, with d’E even denying the existence of the electron, Primas the most in line of all, Mittelstaedt maybe just a little bit blurred, Götschl philosopher friend of Carsetti all in line with Copenhagen, Cini with a strange theory (to be heard) in which he believes he can locate the particle even in the presence of interference (!) [of course it is “wrong” at his levels, but why?].

*When I spoke there were 15-20 people. No objection. d'E's question about Stirling's experiment. It was found that the EPR experiments are not conclusive.
Climate not favourable for me, given the premises, but I came out of it well, all things considered.*

15 e 16/5/89 -Bari. Workshop "Modelli Scientifici della Realtà Fisica"

Garuccio: *"perhaps nature is made in such a way that Ferrero-Santos' 0.82 limit can never be exceeded for the efficiency of photomultipliers".*

Minazzi: *he says he is surprised that I spoke well of Kuhn-Popper-Feyerabend. Apart from the first one, the others "use the history of science like a supermarket". (true, it is!). Nice materialistic quotation from Leopardi (Lo zibaldone): "the matter thinks".*

His optimistic phrases: "the militant scientist thinks the reality of the objects that his science investigates". "those of science are standards of thought elaborated within the historical scientific tradition that has led to so many successes" (this was presented as a guarantee of the positiveness of contemporary scientific thought).

He admits that the "new philosophy of science" seems to have a positive role in science (this is always K-P-F). I made fun of him in private: "I recommend K-P-F" and he answered: "K is fine, but the rest...". Evident Geymonattian influences. Nice, though.

We should reflect on the relationship (intense, I think) between Crusader idealism and Italian Marxism.

I would like to point out again how always stands out Selleri's thought for the study of the history and philosophy of science, and for the political context.

25-27/5 89 at Brest

Bufo chose me to speak in public. This is consistent with his initial approach to the conference, all focused on my book. Initial setting then largely absorbed by the French participations (d'E, LL, V).

Conference (une physique quantique causale et dans l'espace-temps est elle possible?):

Only 20 people attended, including V, LL, d'E, Bufo, Isabelle Stengers, Cini.

They listened, without comments. Basically it is the line of silence, of isolation, of rejection of confrontation. Long live physics which is getting better and better! And yet I explained the triple slit experiments to them.

D'ESPAGNAT Close Realism

Réalisme proche (the elements of reality are all familiar concepts). But: space-time, curved space, creation-annihilation prove that "r.p." is not good.

Weak objectivity = inter-subjectivity. He has always said so, others have made him say different.

A minority believes that close realism is good: Vigier, Selleri, Bohm, Bell.

He accused them of sterility (I then answered him: triple slit).

Bell thinks that it is necessary to renounce relativity on the ontological plane: "There are at the most beautiful bullshit metaphysics".

He also rejects the mathematical realism according to which formulas are in reality. The QM is clearly a phenomenological theory, not ontological, also mathematically speaking. The QM is like a parable that religions use to speak in a "deguisé" way about an indescribable reality. The reality is there, but it is not knowable.

He and JPV are on a first-name basis. JPV becomes dumb, in his presence, on certain fundamental problems.

CINI during lunch says "I do not want to be confused with these" (→me).

He considers himself more realistic than d'Espagnat but less so than me and Vigier. At the table I ask him: if the particle is located on one trajectory, what is on the other? He answers with coughs, turns of phrase, waltzes of concepts.

STENGERS is for strong realism and says so also about Prigogine. Sforza Cini who wants to be "on the rope" halfway between d'E and me+V.

She put herself in philosophy because MQ shocked her (triggering cause). She finds "laides" theories like that of the parallel universes of Everett-de Witt and that of the consciousness of Wigner's observer (I added the delayed choices in an intervention). Totally sympathetic from a philosophical point of view. He confesses himself materialistic. She confesses to be historicist at my request, but there are limitations that come from reality.

LEVY-LEBLOND Always the same but even more to the Right. He says he was ready, by now, to follow d'E who criticized the ontology of his "quantons". On 6 d'E's criticisms, he essentially and explicitly agrees with him on everything.

There is nothing new in what he said about an article he sent me several years ago. He always says the same things, by now he is an old-fashioned of the apologetic QM.

12-14 October 1989-Roma-Accademia dei Lincei

For the level of the speakers, the nobility of the place and the firm hand of the organizers, this was the best conference I attended this year.

3-9 September 1990 London-Imperial College-PIRT.

About 40 people. My paper on the theories of ether and special relativity was very well received. Choir of homages with the only final exception of Prokhovnik.

DUFFY great organizer of these conferences from which mystical tendencies are discovered. Wegener told me that he belongs to the "Salvation Army". Then he made Craig speak, definitely mystical, then he gave space to Cavalleri, emeritus head of the Opus Dei. With me he kept a disagreeable attitude, I did not really understand why (Cavalleri's work? Magazine issue?).

ROSCOE

Nice. He said that for him I am the first Italian who does not believe in the big bang.

PROKHOVNIK

He says: "I am a mathematician". He's a smart guy and has been successful. But he told that he would have proved my work wrong (against the general choir) but then he did not mention it. I bought his old reprinted book in Australia (about 65 years old) for £2.

During this 1990 PIRT, there are two significant evidences for Selleri's future in FRT:

Selleri wrote in his notebook about Winterberg at PIRT that: "He suggests that I publish my work on *Zeit. F. Naturforschung* (with Pfirsch as publisher, sensitized by him)"⁷⁶⁰.

And actually, his second papers will be published right there as: Selleri F. (1990), "Space-Time Transformations in Ether Theories", *Zeitschrift für Naturforschung*, 46A, pp. 419-425.

Concerning his talk at PIRT, listened by Winterberg too, it will firstly published on the PIRT Proceedings as: Selleri F. (1990), "Special Relativity as a Limit of Ether Theories", in *Physical Interpretations of Relativity Theory*, C. Duffy (ed.), London: British Society for the Philosophy of Science, pp. 508-514.

So, also thanks Winterberg, Selleri began publishing his FRT papers not only on the Proceedings of a Conference.

The second evidence came from the contact with Ludwik Kostro, to which Selleri proposed: "to translate his thesis into Italian and publish it as Dedalo book"⁷⁶¹.

Kostro found the proof of a reconsideration made in 1916 by Einstein of the ether, previously rejected in 1905 within the Theory of Special Relativity. As the same Kostro revealed in a interview I made in Bari:

⁷⁶⁰ "suggerisce che io pubblichi mio lavoro su *Zeit. F. Naturforschung* (Pfirsch, editore, da lui sensibilizzato)" (Selleri travel notebook 1989-1990).

⁷⁶¹ "Gli ho proposto di tradurre in italiano la sua tesi di laurea e di pubblicarla come Dedalo libro" (Selleri travel notebook 1989-1990).

[...] On the basis of documents taken in Munich, where I was paid by the Deutches Museum, I was able to read all Einstein's works.

I also received documents from Boston, Don Howard was in Munich, and I also had access to the collected papers of Albert Einstein.

I discovered a letter from Einstein to Lorentz in 1916 where he introduced a new notion of ether. Howard invited me to Boston and there I also saw unpublished letters from Einstein in which he talked about ether.

Franco liked it and he helped me a lot in the publication of my book. He asked me to translate a book on my thesis into English. Franco found Apeiron in Canada to publish it. Then, after the publication in English Franco translated it himself into Italian without putting his name. Franco helped me a lot in my career.

I really liked the personality of Franco, an intellectually and morally honest man. His teaching was excellent, with clear explanations that were also a stimulus for me. Accuracy and clarity of ideas. He was a good teacher (Interview with Ludwik Kostro, Bari, 25 September 2017).

Selleri quoted Kostro's work and outcomes in almost every paper he made in FRT, always pointing out the diverse philosophical approach experienced by Einstein.

Finally, to sum up the tireless Selleri's activity, I found a note, sort of recollections regarding the research he made in the first half of Eighties⁷⁶²:

The researches I am carrying out for several years on the foundations of QM have as basic inspirational reason the attempt to deduce theoretically from certain very general ideas (the elements of reality of Einstein, the empty waves) some consequences that can be verified (better, falsified) experimentally. In the period that ends with 1980 I was interested in Bell's inequality as a theoretical fact, trying to clarify in the most lucid and clear way the now stable incompatibility between empirical predictions of QM and the principle of separable reality. The limitation of this research was only that of not abandoning theoretical outlines in which the alternative to the lack of separability was basically a deterministic image of the world. In the three years 1981-1983 I worked in a probabilistic framework in which reality is made of statistical sets rather than of single systems and in which the real features are the predictable probabilities of the various self-values. Two works have shown that this generalized idea of reality leads to empirically significant consequences in the form of a more general inequalities of the now classical one of Bell which is violated by the QM at least for efficiencies of the counters not below

⁷⁶² In order to avoid a too long footnote, I inserted the original written in Italian in the Addendum 3, pp. 1037-1038.

90%. Therefore the incompatibility with the idea of a separable reality also remains in probabilistic conditions, a result that nobody had disclosed.

The experiment launched in Catania on Bell's inequalities can easily verify the validity or the violation of these inequalities too. In another work the principle of separable reality predicts for the system of two neutral kaons a rate of <12% of that foreseen by the usual framework. Here the critique of determinism is less important since we are dealing with strong interactions for the detection of K mesons and obtaining efficiencies close to 1 should not present insurmountable difficulties. I know that the French physicist J. Six of Orsay wanted to do an experiment on these predictions but I think he did not have the time-machine at CERN.

The Bari conference "Open question in quantum physics" organized by me, led to a breakthrough in the analysis of the experiments of Aspect *et al.* at Orsay. An Italian-British-Spanish collaboration was born and it led to three works in which it was firstly shown that the high intensity of the atomic beams used by Aspect involves a remarkable resonant rescattering of photons in the beam (about 35%), as confirmed by my calculation including the Doppler effect.

Useful results could come from Catania to which we recommend an atomic density $\leq 10^9$ atoms / cm³ for not having more than 1% of resonant rescattering in the beam. Another result of the Italian-British-Spanish collaboration is the discussion of models that admit that the probability of detecting a photon can be different from photon to photon, always giving on average the quantum efficiency of photomultipliers. All the physics of the single photons remains unaltered, only for pairs of correlated photons there may be differences. These models can be formulated to reproduce the correlation function of the QM. One would only have a small oscillation proportional to the cosine by four times the angle between the axes of the polarizers. Not so small as not to be measurable even at CT. Development are in an article with Garuccio in 1984. It has been shown that experiments with three polarizers show effects that are easy to prove experimentally if the probabilities of detection are variable from photon to photon. In four works were discussed the opportunity that Einstein's empty wave, an idea similar to that of virtual wave that Bohr proposed in 1924, can act on excited systems stimulating a more rapid de-excitation. There are experiments that already give positive indication in this sense.

Think of the emission of light by atoms in the proximity of mirrors. The experiment was discussed in three international conferences [also co-organized by me] (Perugia 1982, Athens 1982, Bari 1983). It has received a good reception both from experts of foundations of QM and from other physicists and has produced several works by Italian, British, French, Austrian and US physicists that are coming out in the main magazines. The most obvious realization could be obtained with a laser gain tube (photon duplicator) but also with superfluorescence.

Two review works summarize the ideas of works that tackle the problems posed by the nature of the wave/corpuscle dualism and the incompatibility between the idea of a separable reality and certain empirical predictions valid for the unusual situation of atomic and subatomic systems from different points, separated by a macroscopic distance (entanglement, decoherence). These problems will certainly have an acceptable solution for all if the experimental activity continues, the theoretical one having reached a satisfactory level both from the quantitative and qualitative point of view (FSPA. My underline).

The underscored statement brings us to the new paragraph, referring to the proposal of Selleri about the opportunity of an application of the EPR paradox to particle physics, an experiment with neutral kaon pairs, in order to discriminate between QM and LR. As I will explained, he started exploring this road since 1983, another remarkable path of research experienced by Selleri in a very busy decade.

2.9-KAONS AND THE UNFEASIBLE *EXPERIMENTUM CRUCIS*.

A SUBJECT RELATED TO SELLERI'S SECOND TURNING POINT

In his research for the resolution of EPR paradox and a real comparison to distinguish between strong and weak Bell's inequalities, Selleri, which started his activity on PP, found an opportunity in the application of the EPR to particle physics using neutral kaons produced in a ϕ -factory. The high efficiency of the detectors in these processes would in practice make additional assumptions unnecessary. Selleri will argue in lots of his papers how: "Hopes for a final resolution of the EPR paradox lie in the study of $K^0 K_{\text{bar}}^0$ pairs produced in " ϕ -factory" accelerators and of $B^0 B_{\text{bar}}^0$ pairs produced in "B-factory" accelerators. In both cases the high efficiency of high energy physics particle detectors makes the whole game with the additional assumptions useless and the weak inequalities practically coincident with the strong ones".

Referring to various contributions written by Selleri⁷⁶³, I am introducing here a definition of the kaons⁷⁶⁴ and the processes that concern them and that interest our debate. In the following I will show Selleri's approach to the topic.

A Kaon, also called K meson, is any of a group of four mesons distinguished by a quantum number called *strangeness*. Kaons have proved to be a copious source of information on the nature of fundamental interactions since their discovery in cosmic rays in 1947. Kaons have played a distinguished role in our understanding of fundamental conservation laws: CP violation, a phenomenon generating the observed matter–antimatter asymmetry of the universe, was discovered in the kaon system in 1964 (which was acknowledged by a Nobel Prize in 1980).

Moreover, direct CP violation was discovered in the kaon decays in the early 2000s by the NA48 experiment at CERN and the KTeV experiment at Fermilab. We met the NA48 in the following.

In (Afriat, Selleri 1999, pp. 155-156) they introduced kaons as metastable mesons, with mass less than $2000 \text{ MeV}/c^2$. They are four, that is, $K^\pm$, K_S^0 , and K_L^0 (short and long, S and L, are referred to the lifetimes), and they are called “strange”. Considering the families of isospin, in which the electric charge is the variable physical quantity, “the K's are considered doublets (doublet of particles, K^+ and K^0 with strangeness +1, and doublet of antiparticles K^- and K_{bar}^0 with strangeness -1); they therefore have isospin $\frac{1}{2}$ ”.

The $K^0 K_{\text{bar}}^0$ neutral kaon pairs were produced either at ϕ -factory accelerators or in antiproton annihilations at rest in hydrogen, that is, $\Phi \rightarrow K^0 K_{\text{bar}}^0$. Later Selleri experienced also the opportunity to use mesons $B_d^0 \bar{B}_d^0$, $Y(4S) \rightarrow B^0 \bar{B}^0$, a line of research developed at Stanford and in Japan.

⁷⁶³ In this explanation I am referring most to Selleri F. (1997), “Incompatibility between local realism and quantum mechanics for pairs of neutral kaons”, *Physical Review A*, 56 (5), pp. 3493-3506. But in almost all his papers there is a similar description. It is only to introduce the issue, then I will analyze the single papers, in order to see Selleri's answer to the topic.

⁷⁶⁴ See <https://en.wikipedia.org/wiki/Kaon>. Accessed 24 September 2020.

This ideal experiment is related to a copious production of ϕ meson which decays into two neutral kaons, providing an almost perfect angular correlation between the two kaons, a nearly 100% efficient high energy particle detectors, and an absence of noise.

Selleri desperately sought an opportunity to create an interest in doing an experiment really pivotal to discriminate between QM and LR.

The experiments have been made, first with the purpose of finding only the CP violation, which was completely useless for Selleri's aim to the above discrimination. Secondly, with the implementation of KEK in Japan, with a test closer to Selleri's ideas, even though with additional assumptions. Both cases show a consistence with the prediction of QM, as I will show later.

At the end of this section, I am going to reveal with three evidences taken from interviews, how, failed all the ways, including this one of an *experimentum crucis*, and failing to affect the QM, Selleri will totally turn to the critical analysis of the SRT (already began at the end of 1980s), consistent with his view of a wide and deep research on the great foundations of the two main theories of the twentieth century.

In order to introduce the matter related to kaons production, it is known that in QM the state vector for a $J^{PC} = 1^{--}$ system decays into $K^0 K_{\text{bar}}^0$. Immediately after decay it is given by

$$|\Psi\rangle = 1/\sqrt{2} \{ |K_S\rangle_a |K_L\rangle_b - |K_L\rangle_a |K_S\rangle_b \}$$

where a (left) and b (right) denote the directions of motion of the kaons and K_S and K_L are the usual states for short and long lived kaons, respectively. The small effect of CP nonconservation is neglected and the $CP = \pm 1$ eigenstates are identified with the short and long kaon, respectively.

The individual kaon has his own time evolution:

$$|K_S(t)\rangle = |K_S\rangle \exp(-\alpha_S t), \quad |K_L(t)\rangle = |K_L\rangle \exp(-\alpha_L t)$$

where t is the particle proper time and

$$\alpha_S = \frac{1}{2} \gamma_S + im_S, \quad \alpha_L = \frac{1}{2} \gamma_L + im_L$$

with γ_s and m_s (γ_L and m_L) which denote the decay rate and mass, respectively of the S (L) meson. Units $\hbar=c=1$ have been adopted.

Studying kaon pairs requires however a different time evolution operator:

$$| \Psi(t_a, t_b) \rangle = 1/\sqrt{2} \{ | K_S \rangle_a | K_L \rangle_b \exp(-\alpha_S t_a - \alpha_L t_b) - | K_L \rangle_a | K_S \rangle_b \exp(-\alpha_L t_a - \alpha_S t_b) \} \quad (A)$$

The difference between the two exponentials generates $K^0 K^0$ and $K^0_{\text{bar}} K^0_{\text{bar}}$ components. The two pairs denote a difference in the decay rate and mass.

$$\gamma_S = (1.121 \pm 0.002) \cdot 10^{10} \text{ s}^{-1}, \quad \gamma_L = (1.934 \pm 0.015) \cdot 10^{10} \text{ s}^{-1},$$

$$\Delta m = m_L - m_S = (0.535 \pm 0.003) \cdot 10^{10} \text{ s}^{-1}.$$

Using the inverse time unit, we have:

$$\gamma_S = 1, \quad \gamma_L = 1.716, \quad \Delta m = 1/2.10.$$

It is this difference, related to the difference in the probability of observation of the kaon pairs, which has to be studied.

Since his first paper, written in 1983 and published really soon, “Einstein Locality and the $K^0 \bar{K}^0$ System”⁷⁶⁶, Selleri stated that:

By applying Einstein locality to the $J^{PC} = 1^{--}$ state of two neutral kaons we obtain for correlated observations of two K^0_{bar} interactions a rate 12% lower than predicted by QM (Selleri on Kaons, 1983).

The year is confirmed in a subsequent letter that Selleri sent to Erwin Gabathuler:

[...] The following 10 pages contain a paper on the EPR paradox for neutral kaon pairs submitted to *Physics Letters B*⁷⁶⁷. They are the arrival point of a research that started in 1983. I am very satisfied about it, and I hope you will like it too (Selleri to Erwin Gabathuler, 11 January 1999-FSPA. My underline).

In this realistic and local model Selleri developed by Selleri, the LR violates the prediction of the QM without the need for additional assumptions. This could be definitely the best way to bypass the loophole related to the low-efficiency of detectors. Then, according to him:

⁷⁶⁵ I also used to call them $K^0 K^0_{\text{bar}}$ for technical character simplicity.

⁷⁶⁶ Selleri F. (1983), “Einstein Locality and the $K^0 \bar{K}^0$ System”, *Lettere al Nuovo Cimento*, 36 (16), pp. 521-526. Received 7/2/83. Published 16/4/83. (Selleri on Kaons, 1983) in the following.

⁷⁶⁷ The paper is: Foadi R., Selleri F. (1999), “Quantum Mechanics versus Local Realism and a Recent EPR Experiment on $K^0 \bar{K}^0$ Pairs”, *Physics Letters B*, 461 (1-2), pp. 123-130.

All the frustrating theme related to strong and weak Bell's inequalities can therefore be avoided because, for efficient detectors the two types of inequalities tend to coincide and the comparison between LR and MQ can be studied directly without resorting to additional hypotheses.⁷⁶⁸ (AGAB).

Going back to (Selleri on Kaons, 1983), he neglected the CP nonconservation in K^0 decay. According to the three key statements related to LR, namely, reality, separability and time arrow (in this paper referred to as EPR reality criterion, separability, and no retroactive causality), Selleri pointed out the irreconcilability with QM at the empirical level and the need to decide which predictions of the theory are to be accepted in a reasoning:

We will accordingly consider as acceptable the following quantum mechanical predictions:

- P1) Kaon decays are individual random processes at constant rate. Populations reduce exponentially.
- P2) All probabilities have left-right symmetry: for all purposes a K_S going left and a K_L right are physically equivalent to a K_S going right and a K_L left.
- P3) All probabilities have particle-antiparticle symmetry.
- P4) Assuming exact time-reversal the rate of every transition equals that of the opposite transition.
- P5) If the left (right) kaon has been observed to be a K_S then the right (left) kaon can be predicted with certainty to be observed as a K_L at any future time.
- P6) If, instead, the left (right) kaon has been observed to be a K^0 at proper time t_0 then the right (left) kaon can be predicted with certainty to be observed as a K_{bar}^0 at equal proper time t_0 (Selleri on Kaons, 1983, pagg. 522-523).

Starting from the above quantum mechanical predictions, Selleri introduced, in a local realistic approach, two element of reality, related to CP and strangeness:

[...] Einstein locality allows us to attribute elements of reality to $K^0 K_{\text{bar}}^0$ -pairs described by the state vector (A).

⁷⁶⁸ “Tutta la nota frustrante tematica relativa alle diseguaglianze di Bell forti e deboli può quindi essere evitata perché per rivelatori efficienti i due tipi di diseguaglianze tendono a coincidere e si può studiare direttamente il confronto fra LR e MQ senza ricorrere ad ipotesi addizionali” (AGAB).

[...] every kaon of every pair described by the state vector has an associated element of reality λ_1 which determines a well-defined CP value if and when a CP measurement is carried out.

A second element of reality, connected with strangeness S, can be introduced from Einstein locality.

[...] every kaon of every pair described by the state vector has an associated element of reality λ_2 which determines a well-defined S-value if a S measurement is carried out.

[...] The important difference between λ_1 and λ_2 is that, while λ_1 describes a time-independent property, λ_2 describes an instantaneous property (Selleri on Kaons, 1983, pag. 523. *Italics in the original text*).

Then Selleri noticed that:

[...] Einstein locality has brought us outside QM: no state vector is in fact known which can describe a given kaon as having *simultaneously* well-defined CP and S. This is of course a standard situation, since the attribution of elements of reality (“hidden variables”) is known to lead generally to the existence of dispersion-free ensembles which find no description within the existing quantum theory.

[...] All transitions must be symmetrical in every pair: if one kaon changes its strangeness also the other one does and at the same proper time.

Consistency with Einstein locality requires two local and symmetrical mechanisms regulating transitions which are in all cases simultaneous when expressed in individual proper times. Although individual behaviour appears to be deterministic, one can speak the language of probability over the statistical ensemble of $K^0 K_{\text{bar}}^0$ -pairs because the individual deterministic mechanisms regulating transitions can in general be different from pair to pair (Selleri on Kaons, 1983, pag. 524. *Italics in the original text*).

Selleri found an overall probability for the presence of two K_{bar}^0 ’s at proper times, revealing how “this upper limit differs from the quantum-mechanical prediction by the absence of the interference term. Quantum mechanics leads, therefore, to a prediction [...] with a maximum violation that can be shown to be about 12%” (Selleri on Kaons, 1983, pag. 526).

In the acknowledgment, Selleri cites Beneventano, Roos, and Six. The latter two published before him a study on a possible evolution of the Furry hypothesis for correlated kaon pairs.

In another paper written in 1991 with Home, “Neutral Kaon Physics from the Point of View of Realism”⁷⁶⁹, talking about the potentially fruitful topic of kaons, Selleri recalled how the investigations on kaons were motivated by a Bell’s outlook made at Varenna 1970 (see pages 354-357 in this dissertation), even if here are cited Bell’s works made in a radio interview in 1986⁷⁷⁰:

Neutral kaons provide an instructive example of a two-state system. A remarkable feature is the experimentally verified quantum mechanical prediction that, in the presence of weak interactions, neutral kaons will not only decay through the modes K_S and K_L (CP eigenstates) but will also display transitions (“oscillations”) between the two states K^0 and K^0_{bar} (strangeness eigenstates).

In view of the realization we now have that many quantum mechanical phenomena, such as the double-slit interference effect, can be reproduced by applying the point of view of realism (based on the notion that the quantum mechanical entities possess well defined attributes independent of measurements and are amenable to causal space-time description), the question arises as to whether the quantum mechanical predictions for decaying and oscillating neutral kaons can also be reproduced using such a model.

This issue is investigated in the present paper and we obtain an affirmative answer to the above question. We also analyze the phenomenon of coherent regeneration of K_S from the same point of view.

Such investigations are motivated by an outlook articulated in the following words of John Bell: “*One wants to be able to take a realistic view of the world, to talk about the world as if it is really there, even when it is not being observed. [...] our business is to try to find out about it, and the technique for doing that is indeed to make models and to see how far we can go with them in accounting for the real world*”.

It is this spirit of realism that we pursue in this paper (Home, Selleri 1991, pag. L1073. *My Italics*).

The authors clearly introduced the differences between LR and QM and emphasized their willingness to neglect the effects of the CP violation:

Based on “realism” we assume that at any given time t an individual neutral kaon exists in a state which has simultaneously well defined eigenvalues of strangeness S and CP. Obviously, such a state has no meaning in quantum mechanics.

⁷⁶⁹ Home D., Selleri F. (1991), “Neutral Kaon Physics from the Point of View of Realism”, *Journal of Physics*, A24 (18), pp. L1073-L1078. (Home, Selleri 1991) in the following.

⁷⁷⁰ Davies P.C.W., Brown J.R. (eds) (1986), *The ghost in the atom. A discussion of mysteries of quantum physics*, Cambridge: Cambridge University Press. See Bibliography.

[...] [The introduced] probabilities are defined essentially in the sense of realism; the key assumption is that there exist elements of reality that determines the values of strangeness and of CP for every kaon, and the probabilities are taken to be relative frequencies of the different possibilities in the statistical ensemble.

[...] the (small) effects of CP violation will be ignored throughout our treatment. Interesting consequences of CP violation for the neutral kaons in the EPR context have been discussed by Datta, Home, and Raychaudhuri [in 1994⁷⁷¹] (Home, Selleri 1991, pagg. L1073-L1074).

In their conclusions, the authors stated what they did and the next step, which included the perspective of an experiment to perform at Frascati:

The particular form of realism assumed in our paper is the one that follows from the application of the EPR reality criterion, in conjunction with the locality condition, to the EPR-type example involving spatially separated and correlated $K^0 - K_{\text{bar}}^0$ pairs. It is remarkable that exactly the same elements of reality which must necessarily be introduced for the correlated $K^0 - K_{\text{bar}}^0$ pairs reproduce exactly the physics of single kaons. The remaining issue is, therefore, the compatibility of locality with the quantum mechanical predictions for the correlated $K^0 - K_{\text{bar}}^0$ pairs.

On this point one may refer to the recent paper by Six⁷⁷² [the 1990 paper, not the 1982 one] who has claimed to have reproduced the quantum mechanical results for the $K^0 - K_{\text{bar}}^0$ system with a specific local realistic model. However, his model contains negative probability functions which are physically unacceptable.

Having demonstrated the compatibility between QM and realism in the context of decaying, oscillating and regenerating neutral kaons, the next step will be to investigate this issue with reference to the correlated $K^0 - K_{\text{bar}}^0$ pairs. Such investigations assume considerable importance, particularly in view of the proposed Φ factory project of Frascati⁷⁷³ which would enable crucial measurements of joint probabilities for the $K^0 - K_{\text{bar}}^0$ system (Home, Selleri 1991, pag. L1078. My underline).

In an unpublished paper written maybe in 1994, “Einstein-Podolsky-Rosen paradox for kaon pairs at the DAΦNE accelerator”⁷⁷⁴, Selleri made, as

⁷⁷¹ See the reference 4 in (Home, Selleri 1991, pag. L1078).

⁷⁷² See the reference 6 in (Home, Selleri 1991, pag. L1078).

⁷⁷³ See Cocolicchio 1990 in the reference of (Home, Selleri 1991, pag. L1078).

⁷⁷⁴ Selleri F. (1994?), “Einstein-Podolsky-Rosen paradox for kaon pairs at the DAΦNE accelerator”, Unpublished-FSPA (Selleri EPR for kaons at Dafne, 1994) in the following. The paper was in a USB-pen and was written with an old program which made it hard to read with today’s media. The useful bibliography which Selleri put in the paper is in Addendum 3, at pp. 1038-1039 in which it is possible to find the precise references to the authors above expressed.

usual in his approach, an historical overview of the state-of-the-art before and after his contributions on kaons, analyzing almost “thirty years of research on the possibility to test quantum mechanics versus local realism for neutral kaon pairs”. The entries came from H.J. Lipkin, who in 1968 noted that there is a remarkable consequence of the state. The observation of a particular decay mode for one kaon implies that the other kaon is forbidden to decay, at equal proper time, in this mode.

Then, starting by the Furry hypothesis on the spontaneous evolution of the state vector into its components, J. Six in 1982 considered a possible evolution for correlated kaon pairs.

He considered non-physical probabilities, which were analyzed by Selleri in a 1991 paper with Home and Lepore “Local Realistic Models and Non-Physical Probabilities”⁷⁷⁵:

We give a general argument showing that one can always reproduce the quantum mechanical results for non-factorizable state vectors of correlated systems by hidden variable models using non-physical probabilities, as recently done by Six for the particular case of correlated $K^0 K_{\text{bar}}^0$ pairs.

[...] Plan of this paper is as follows. We first show explicitly the essential negativity of the probabilities used by Six. We then proceed to outline the proof of a general argument demonstrating that the quantum mechanical results for non-factorizable or entangled states can always be reproduced by hidden variables models provided one allows for non-physical probabilities in the intermediate step. We conclude by making a critical comment on the logical tenability of using such probabilities (Home, Lepore, Selleri 1991, pagg. 357-358).

According to Selleri, the main difficulties in studying experimentally the EPR paradox with kaon pairs in the usual accelerator physics are twofold:

- (i) the cross sections for producing F mesons are normally rather small;
- (ii) a large background usually hides the signal composed of the two neutral kaons arising from F decay.

D. Cocolicchio in 1990 proposed to overcome these difficulties, by using a ϕ -factory.

⁷⁷⁵ Home D., Lepore V.L., Selleri F. (1991), “Local Realistic Models and Non-Physical Probabilities”, *Physics Letters A*, 158, pp. 357-360. (Home, Lepore, Selleri 1991) in the following.

He pointed out that a machine luminosity of $10^{32} \text{ cm}^{-2} \text{ s}^{-1}$ as expected for the DAΦNE accelerator, could easily allow one to detect the 12% discrepancy predicted in 1983 by Selleri [(Selleri EPR for kaons at Dafne, 1994)]. This latter paper contained the first systematic application of local realism to kaon pairs. Elements of reality connected with strangeness and CP were shown to be necessarily present in each kaon. These elements of reality are the basic ingredients of any theory developed according to local realism (Selleri EPR for kaons at Dafne, 1994-FSPA).

A very long section of the paper was devoted to the criticism by Ghirardi, Grassi and Weber (GGW) in 1992 to (Selleri on Kaons, 1983), to which Selleri answered with the news on his paper written with Privitera:

A critical discussion of the theoretical studies of the EPR paradox for kaon pairs was made by Ghirardi, Grassi and Weber (GGW) in 1992. This paper is a good example of how people can jump to wrong conclusions by starting from some generally correct premises. Their general conclusion was: *“the ϕ -factory facility does not seem to open new ways of testing quantum mechanics versus alternative general schemes of the type which are usually regarded as worth considering in the debate about locality and quantum mechanics”*. We will show that this pessimistic statement is not correct. Exactly the opposite is true: fundamental experiments can be performed and they can lead to a decisive choice concerning the validity either of local realism, or of quantum mechanics.

Let us briefly review the arguments put forward by GGW.

The three authors started by observing that Bell’s inequality written in terms of four different times of flight of the kaons is not violated by the quantum mechanical two-time joint probability for correlated kaonic behaviour. This conclusion is correct and has been reached also by Ciochanco [in 1996], who pointed out that a priori a violation is expected for large time differences, just as in the EPR experiments with atomic photon pairs it is obtained for relatively large angles between the polarizers’ axes. But the decay factors in the kaonic wave functions decrease exponentially and large times make it impossible to overcome numerically Bell’s limit. When the degree of violation is plotted as a function of the artificially varied kaon lifetime (e.g. by keeping the short/long lifetime ratio constant) one observes that for real kaons no violation exists, but that a discrepancy shows up when the kaon lifetime is about twice larger than the real one, or more.

Of course, the correctness of the GGW conclusion concerning Bell’s inequality does not mean that a compatibility exists between local realism and quantum mechanics for the real kaons. In fact, Bell’s inequality is only one of the many consequences of local realism.

[...] it was shown that local realism does not only predict the validity of Bell's inequality, but leads necessarily to an inequality for any linear combination of correlation functions. We [disagree] on their general statement that meaningful tests of local realism are impossible with a Φ -factory facility. The coming part of the story shows that just the opposite is true, and that the years following 1992 have produced new results which are decisive in this sense. The pessimistic conclusion reached by GW has probably been generated by their transparent conviction that the Orsay "clear-cut" experiments [...] have really shown that nature is nonlocal at least as far as the photon correlation experiments are concerned. Actually the logical situation is much less simple: the photon correlation experiments have violated only inequalities much stronger than the original Bell's inequality, deduced with the essential help of arbitrary and untestable additional assumptions. It was impossible to test the validity of the original Bell's ("weak") inequality because of the low efficiency of photon detectors. Highly efficient detectors exist instead in elementary particle physics. For this reason the kaon-pair option seems to be the best choice for a really penetrating discrimination between local realism and the existing quantum theory (Selleri EPR for kaons at Dafne, 1994-FSPA-My underline).

Actually, Selleri stressed how already in his 1991 paper written with Home, "the seeds for overcoming the GW objection had been sewn", still arguing that:

These authors discussed only single kaon physics (and not the EPR problem) and showed that at least one local realistic model existed which could exactly reproduce the kaonic quantum mechanical oscillations in strangeness.

In this model kaons undergo sudden jumps of strangeness and the quantum probabilities are reproduced by summing over several different individual behaviours. The existence of this model eliminated the "mystery" surrounding the quantum mechanical properties of kaons.

[..] It was not the task of HS to find the most general local realistic model for single-kaon oscillations, but only to show that nothing in these oscillations contradicted the realistic point of view (Selleri EPR for kaons at Dafne, 1994-FSPA).

Anyway, the crucial point for Selleri was to perform the experiment.

In the correspondence I found in FSPA, Selleri asked to many people a support for his purpose. A really hard mission, though. Alternating with his subsequent works, I will show a series of letters confirming his extensive search for support and practical opportunities of having access to the

laboratories to carry out the experiment. Finally, some interviews and a very important Selleri e-mail, given to me by Michele Barone⁷⁷⁶, show the negative outcome of both the experiments done and the opportunity of being able to do them.

A deep correspondence which could shed light on the matter emerges between Selleri and P.H. Eberhard⁷⁷⁷, who played a key role, among several other reasons, in orienting the experiments at Frascati ϕ -factory in a mere research of CP-violation, actually preventing them from searching the Selleri's idea.

In the letter sent by Eberhard to Selleri, the first hint related to the ϕ -factory appeared. This letter has been written before the new paper published in 1992 by Selleri and Privitera, which overcame, according to Selleri's belief, the criticism by Ghirardi, Grassi, and Weber:

Dear Professor Selleri:

thank for the reprint of the paper by D. Home and yourself, entitled "Bell's theorem and the EPR paradox". Reading the Table of Contents at page 1, I noticed your Section 2.6.2 which suggests a test of quantum mechanics at ϕ -factories.

[...] I am interested in ϕ -factories and trying to analyze under which conditions tests of this sort can be done. Your suggestion is interesting in spite of what Ghirardi, Grassi and Weber have said about it, because I think nature is not trying to trick us whenever it can, and thus supplementary hypotheses are okay. Let me just point out that the measurement of t_1 and t_2 , which you are concerned about as to its accuracy, can be done rather easily around an asymmetric ϕ -factory where both K^0 's may travel very fast at about the velocity of their CMS.

[...] However, I have heard statements according to which "people who are unhappy about quantum mechanics are people who do not understand it". This is a prejudice. It is false. Nevertheless, misstatements by realists about QM feed that prejudice. The founders of relativistic quantum field theory have set the principles and the equations in such a way that their theory prevents FTL [faster than light] communication. That is what R. Ross and I realized when we wrote the paper of your reference [...]. The founders succeeded in doing what they wanted to do. I think it is better to recognize the fact.

⁷⁷⁶ Who gave me also the technical articles for DAΦNE and KEK, and projects as well as the Eberhard paper, whereas Vincenzo Paticchio gave me the specific book on FINUDA project.

⁷⁷⁷ P.H. Eberhard: See https://www.researchgate.net/profile/P_Eberhard. Accessed 24 September 2020.

Another reason why I care about this issue is that [...] I would not be surprised if FTL communication was actually possible in practice, contrary to what the theory predicts, because I do not consider that theory as the absolute truth. What we need now are experiments that may find quantum theory at fault.

Quantum theory interdiction of superluminal signalling is actually one more reason to look for a possible failure, somewhere, of its predictions. However, misstatements about what quantum field theory predicts are very counter productive.

To avoid any possible misunderstanding about what I mean, let me mention my demonstration that reality and locality are acceptable concepts even if an EPR experiment free of supplementary hypotheses is performed and yields results according to predictions of quantum theory. To do that I built a model which is realistic and local and which reproduces every prediction of quantum theory, not completely but up to an arbitrarily high degree of accuracy. The model is not 100% Lorentz invariant (see *Quantum theory and pictures of reality*, W. Schommers, ed, page 167). When building the model, I found out that there are very particular experimental setups where FTL signalling is predicted by the model (page 196).

Of course, it happens only in a restricted domain where the model's predictions differ from quantum theory. Therefore I do not claim that FTL signalling is actually impossible. I just claim that it would imply a break down somewhere of orthodox relativistic quantum field theory.

By the way, I think that this model is one that best respects principles I know you have been defending all along, except Lorentz invariance. If a supplementary hypothesis free experiment is done and confirms quantum theory, you may be interested in some of the ideas that are behind it.

I realize much of what I am saying here are oversights on your part. I do not expect a controversy about them.

The motivation of this letter is essentially to stress the importance that I give to them for our common good (P.H. Eberhard to Selleri, 15 November 1991, FSPA).

Basically, Eberhard, a declared realist and localist as Selleri, smashed the article to bits, tearing it apart, still revealing a certain faith in the work of the Selleri, which ends up leaving reality aside. The main thing is that Eberhard will write a seminal paper on the priorities experiments to be performed at Frascati. And maybe it could be significant in the decision of not carrying out the Selleri's desired experiment.

Selleri answered to the above letter, still maintaining his positions and promising to send his new paper, namely the 1992 one written with Privitera.

He also apologized for his inaccuracies and asks for some references on the problem of minimum and maximum kaon proper time;

Dear Professor Eberhard, [your letter] was very interesting to read, because, as you know I am a realist of the local type. I think that your position is untenable, however (realism + locality of probabilities, if I understand correctly). I have shown in different places, e.g., in the enclosed paper published in the German journal *Philosophia Naturalis*, that probabilities reflecting some type of reality plus locality of probabilities leads to Bell inequality. Therefore the most general type of local realism that can be conceived is incompatible with QM. Notice that my assumption of local probabilities is exactly the quantum mechanical form of locality. You will of course think that this is impossible, but it is not so. The locality of probabilities is exactly the same as in QM in a general sense, but must be applied to subsets of the quantum sets, the existence of the subsets being a general consequence of my probabilistic local realism. Your reaction to my paper would be very interesting to read from me.

I am very interested in ϕ -factories and I am completing a paper that I will send you as soon as ready: the theory is complete, only the numerical calculations are missing.

It is, I believe, a very general study of two K^0 decays of ϕ 's with very mild and general additional assumptions. I am happy to learn from you that such experiment may be feasible. The problem is also to know which is the minimum and maximum K^0 proper time (t) that one can study in the laboratory: if t is too short the K^0 is inside the machine, if it is too long there are no K^0 's left because of decay. If you know of some study of this problem I would be very happy to have the information concerning it.

I will definitely read your paper in Schommers book. I had not understood its meaning before your letter.

Concerning the review paper, I am sorry if some inaccuracy (or some mistake) is contained in the part concerning elementary particles (Selleri to P.H. Eberhard, 6 December 1991-FSPA. My underline)

Some changes in perspective, which in Selleri's thoughts overcome the Ghirardi, Grassi, and Weber objection, were found in the already mentioned paper with Privitera, "Quantum Mechanics Versus Local realism for Neutral Kaon Pairs"⁷⁷⁸,

We formulate the theory of $K^0\bar{K}^0$ pairs by including variables which determine when the two kaons will undergo a simultaneous jump of strangeness.

⁷⁷⁸ Privitera P., Selleri F. (1992), "Quantum Mechanics Versus Local realism for Neutral Kaon Pairs", *Physics Letters B*, 296, pp. 261-272. (Privitera, Selleri 1992) in the following.

This approach is a direct consequence of the EPR reality criterion when locality is also taken into account. The probabilities of observing $K_{\text{bar}}^0 K_{\text{bar}}^0$ and $K_S K_{\text{bar}}^0$ pairs at different proper times are shown to disagree with quantum mechanical predictions up to 25%. The experimental study of such deviations could be easily performed in a Φ -factory (Privitera, Selleri 1992, pag. 261).

[...] the application of local realism to the physical situation [...] has brought us at least formally outside quantum theory: no quantum mechanical state vector exists [...] which can describe a kaon as having simultaneously well defined CP and S values. [...] the discrepancy is not only formal but substantial (Privitera, Selleri 1992, pag. 262).

[...] The local realistic theory of $K^0 K_{\text{bar}}^0$ pairs, which includes a deterministic treatment of the simultaneous jumps of strangeness of the two kaons, has been developed.

The quantum mechanical predictions for $K_{\text{bar}}^0 K_{\text{bar}}^0$ and $K_S K_{\text{bar}}^0$ pairs observation at different proper times have been shown to disagree with a limit derived from local realism up to a maximum of 13 to 25%.

The experimental test of the incompatibility of quantum theory and local realism in the $K^0 K_{\text{bar}}^0$ system finds its ideal place in the Frascati Φ factory, which will provide a copious production of neutral kaon pairs. The time evolution of $K_{\text{bar}}^0 K_{\text{bar}}^0$ should be easily measured identifying both K_{bar}^0 's decaying into $\pi^+ l^- \nu_{\text{bar } l}$. For the $K_S K_{\text{bar}}^0$ one can identify the K_S decay into a $\pi^+ \pi^-$ and the K_{bar}^0 decay into $\pi^+ l^- \nu_{\text{bar } l}$. The expected precision in the reconstruction of the path time of the K^0 decaying into two charged particles is $\approx 0.1 \tau_S$, which is more than enough to study the time evolution of the observation probabilities previously discussed (Privitera, Selleri 1992, pag. 271).

Concerning the above paper, an analysis is made again in (Selleri EPR for kaons at Dafne, 1994-FSPA):

In 1992 Privitera and Selleri (PS) developed the line of thought first presented in [Selleri on Kaons, 1983]. Neglecting CP violation, they started from the local realistic description of kaon pairs described by the state vector according to which the two particles necessarily undergo simultaneous jumps in strangeness. These jumps take place at random times in a statistical ensemble of pairs, but every jump must be predetermined at the time of production (time of Φ decay) of the kaons in order to account for the simultaneity within the local realistic approach. PS developed a rate-equation method for calculating the probabilities of the eight possible local realistic states of kaon pairs.

[...] The final part of the PS paper used the results found by HS [Home, Selleri 1991] for single kaons, and this implied an advantage and a shortcoming. The advantage was that the difference between opposite rates was fully recognized, thus overcoming the GW objection.

The shortcoming was however in the lack of generality of the HS model for single kaons. Introduced in the theory for correlated kaon pairs, the HS model could only lead to results of limited generality.

However the HS model was natural and elegant and the discrepancy with quantum mechanics found by PS was large (up to about 25%). The result indicated that such a discrepancy was likely to remain also if the most general theory for single kaons was used (Selleri EPR for kaons at Dafne, 1994-FSPA).

In an interview made in 1993 during a conference in Oviedo, Selleri declared that in 1995/96 DAΦNE will come into operation and he hoped their proposal [Privitera, Selleri 1992] could be implemented.

Going back to the endorsement that Selleri tried to do in order to make his proposal for an experiment to be accepted, in a letter to Prof. G. Maggi, of the Bari INFN section, Selleri submitted the endorsement by Zichichi and Giacomelli, a possible proof of a real interest in others scholars. He stated:

Dear Maggi,

I reply to your circular letter of 10 September. Actually, I have something new that may be of interest to the INFN. I have finished a long work concerning the Einstein-Podolsky-Rosen paradox for pairs of neutral kaons produced in the decay of vector “phi” mesons. The result is that there is a minimum discrepancy of 25% between any possible theory of local realism and quantum mechanics. The phi-factory in Frascati is the ideal machine to do this experiment.

The work is complete in all its parts, except writing: I am slowed down in writing by other commitments, for example the four international conferences in September in which I will attend. However, the work will soon see the light. The research was the subject of Sebastiano Cebianco’s degree thesis in Bologna: at the time of the thesis (end of June) there was a lot of interest from Zichichi (also from Giacomelli). The results were presented in a conference (Oviedo) and in a seminar (Belfast). My previous works on the same subject in 1983, 1991 (with D. Home) and 1992 (with P. Privitera) are overtaken by the latest results which are finally absolutely general, i.e. true for any theory of local realism⁷⁷⁹ (Selleri to G. Maggi, 16 September 1996-FSPA).

Another proof rests in a letter by Selleri to Beltrametti.

⁷⁷⁹ The original Italian text is in Addendum 3, p. 1039.

It is undated but maybe was written between September and November 1996, as from some evidences within the text, not necessary in the present analysis. Selleri confirmed the interest by Zichichi and Giacomelli:

[...] I have been working on the EPR paradox for kaon pairs since '83 and I finally arrived to the end with a general demonstration of the incompatibility between QM predictions and local realism. The third act of the story was Cobianco's thesis in Bologna, which aroused the interest of Giacomelli and Zichichi who wanted a copy. The second last but one was a seminar in the UK.

The last one is the work that I am finishing and that I will send to the *Physical Review*, something that I haven't done for twenty years. There is a 25-30% discrepancy between LR and QM for the joint detection of two $K_{0\text{bar}}$ at different times.

The generality of the new work is such that Ghirardi's criticisms, which are always a bit captious, are overcome as a whole. I am trying to spread the idea in Italy, because the experiment is only feasible at Frascati with the phi-factory. I will talk about it also here at the INFN in Bari on 7th November, and I will do most of the effort as soon as the preprint is technically ready: every analytical calculation is finished, only the smallest part of the numerical calculations is missing⁷⁸⁰ (Selleri to Beltrametti, 1996-FSPA. My underline).

The last work, the final one of which Selleri stated above is "Incompatibility between local realism and quantum mechanics for pairs of neutral kaons"⁷⁸¹.

A local realistic theory of single neutral kaons and of correlated neutral kaon pairs is formulated by deducing the most general consequences of three assumptions: (i) the Einstein, Podolsky and Rosen (EPR) reality criterion, (ii) locality, and (iii) No retroactive causality. Variables must be introduced for every kaon determining the stable CP value and the variable value of strangeness S. Instantaneous S jumps are shown to take place. If kaon pairs are produced in Φ meson decays, the local realistic probability of observing $K_{\text{bar}}^0 K_{\text{bar}}^0$ pairs at certain different proper times necessarily differs by 30% from the quantum mechanical predictions. The size of this difference justifies our systematic neglect of CP violation. A Φ factory is thus shown to provide a unique tool for the study of the EPR problem (Selleri kaons 1997, pag. 3493).

[...] The conclusion is that an experimental discrimination between local realism and quantum theory should be relatively easy at a Φ factory accelerator, and perhaps also with different techniques (Selleri kaons 1997, pag. 3494).

⁷⁸⁰ The original Italian text is in Addendum 3, p. 1040.

⁷⁸¹ Selleri F. (1997), "Incompatibility between local realism and quantum mechanics for pairs of neutral kaons", *Physical Review A*, 56 (5), pp. 3493-3506. (Selleri kaons 1997) in the following.

The importance of this paper rests in the generalization of four basic states for the single kaon physics, in the neglect of CP violation, with every kaon which maintains forever the same CP with which it was born, with the calculation of realistic probabilities for a peculiar mixture and for every of the four states, still generalizing them with the most general probability matrix, with the widening to the kaon pairs, studying the cases with the basic states, still ending up in an appearance of the four elementary states with the same weight of $\frac{1}{4}$.

The wide calculation made in the paper leads to the comparison between quantum mechanical prediction and the smallest possible probability of all local realistic theories of correlated neutral kaon pairs. So Selleri worked out the most general case. He stated that:

In this region quantum mechanics violates the local realistic limit by 28% on the average. We take this to be a sufficient indication of the physical interest of our results and of the concrete feasibility of an experiment choosing between quantum mechanics and local realism. The statement by Ghirardi and collaborators that Φ factory facilities are not useful for the experimental study of the EPR paradox is thus shown to be incorrect. A systematic numerical comparison between the quantum theoretical predictions and the upper and lower predictions of local realistic theories will be given in a forthcoming paper (Selleri kaons 1997, pag. 3504).

A description was also given by Michele Barone, who stated how in the process,

K_{0L} has a longer average life than K_{0S} . 1 out of 500 decays as if it were a K_S and doing so, it infringes the CP. Franco disregards this violation and assumes $K_L=K_S$ according to QM. And despite this, according to local theories, he finds $K_L > 30\% K_S$ (Interview with Michele Barone, Athens, 21 June 2019).

The importance of the above paper compared to (Privitera, Selleri 1992) also rests in two letters sent by Selleri to Dr. Amitava Datta in Kolkata; and to Ugo Amaldi Jr. at CERN, a venue in which another international project, CPLEAR, could reveal useful for Selleri's hopes:

In the last two years I have been working hard on this problem because the Frascati machine (DAΦNE) starts operating in less than a year from now. The main difference with respect the 1992 paper with Privitera (PS) is that in the conclusive part PS used the single-kaon model developed by Home and me (HS), which was perfect for what HS wanted to do (show that QM for a single kaon can be reproduced within the hidden variable approach), but was not general enough for the real needs of PS. This shortcoming of PS (like some other minor ones) is fully eliminated in the new preprint you are now receiving: In fact now I have developed the most general local realistic theory of single kaons, because it is needed in the theory of EPR pairs! You will find everything in my preprint.

Now I feel sure that one can say: all possible local realistic models for K^0 pairs disagree at the empirical level with the predictions of quantum theory. It is true that Bell's inequality is not violated in the case of K^0 pairs (due to the decreasing exponentials) - as Ghirardi *et al.* noticed - but local realism has many implications and Bell's inequality is only one of them. [...] It should be easy to extend these results to B^0 mesons (Selleri to Amitava Datta, 2 May 1997-FSPA. My underline).

Dear Ugo,

I am sending you a copy of my final work on the EPR paradox for neutral kaon pairs. This work overcomes some difficulties still present in the letter we published in 1992 with Privitera, the main one being the fact that in the end we used the insufficiently general Home and Selleri theory. Now, however, everything is all right. It would be nice if you were interested in doing this experiment on Frascati's machine: it is the only chance to really solve the EPR paradox. I do not say something like that lightly. **The people of Frascati do not seem to give a damn about these things anymore, after the conceptual devastation caused by the work of Srivastava and Widom, I am talking about Franzini at least**⁷⁸² (Selleri to U. Amaldi Jr., 5 May 1997-FSPA. My underline and **bold**).

The last statement provides the evidence of an interaction and several troubles in doing the experiment and in receiving time machine.

In his hectic research for a ϕ -factory accelerator, Selleri searched also in USSR: in a letter to V. Kadyshevsky⁷⁸³, he stressed the opportunity of a reciprocal interest collaboration in order to carry out the experiment, basically a request of help.

⁷⁸² The original Italian text is in Addendum 3, p. 1040.

⁷⁸³ Prof. Vladimir Kadyshevsky, at Laboratory of Theoretical Physics at Joint Institute for Nuclear Physics in Dubna, Moscow Region.

The relevant information is that Selleri had always thought only to Frascati and then, probably because of the troubles in obtaining time machine, he tried focusing his attention elsewhere:

Dear Vladimir,

Few days ago I learnt that in Novosibirsk there is a phi-factory accelerator. This can be of reciprocal interest because very recently I concluded a 14 years long research on the EPR paradox for $K^0 K_{\text{bar}}^0$ pairs. The result is a very large discrepancy (about 30%) between the quantum mechanical predictions and all possible predictions following from local realism for the joint probability of detecting two K_{bar}^0 's at different proper times. K_{bar}^0 's could be detected via semileptonic decays thanks to the $\Delta S = \Delta Q$ rule.

I have published previously results of this type, e.g., on *Phys. Letters B* (1992), but they were not general enough: only now can I claim that the discrepancy is **with all ever conceivable local realistic models**. This is the only way of solving the EPR paradox because the experimental efficiency for detecting pairs of neutral kaons is high, not far from 100%, while that for pairs of photons was only 4% in the best experiment. A low efficiency brings in the need of introducing additional assumptions that bias completely the choice between Q.M. and L.R. Until a few days ago I thought that my results could concretely interest only the Frascati phi-factory that will be in operation in one year or more, but a friend gave me this new and interesting indication. I hope not to abuse of your kindness if I ask you some questions:

1. Is the Novosibirsk phi-factory really in operation now?
2. Did they publish some results already? When? Where?
3. Who are the people I could try to contact for proposing my experiment?
4. Is anybody in Dubna collaborating with Novosibirsk?

If you are personally interested in the EPR matter I could send you copy of my paper. Thank you for your help. Friendly yours (Selleri to V. Kadyshevsky, 14 March 1997-FSPA. Selleri **bold**, my underline).

Selleri wrote also to Prof. G. Diambri Palazzi in order to attend to a conference in Rome, *Phenomenology of Unification from present to future*, held one month later. His talk was titled “Einstein-Podolsky-Rosen Paradox for the $\phi \rightarrow K^0 K_{\text{bar}}^0$ decay”

Dear Diambri,

The space to speak at your conference now would have been created: I leave by plane from Rome on the 24th morning, so, if it were possible, I would gladly speak on the 23rd.

It is easy to say that my eventual talk has nothing to do with “UNIFICATION” but I have always worked thinking about DAFNE and my results might interest someone. You see, of course. I enclose the title and abstract of the possible seminar⁷⁸⁴ (Selleri to Diambri-Palazzi, 24 March 1997-FSPA).

In the above letter it sounds clear how Selleri is thinking to a possible withdrawal as regards Frascati. It is unusual the fact that he was unsure whether or not to go to the conference, if you think about how he is struggling on every side, and even the tone is a bit loose.

Starting the 1998, Selleri began talking about the new venues in Japan and at Stanford, in addition to CERN and Frascati, even though the latter seemed to Selleri a lost battle.

In a letter to Jürgen Brandes, he emphasized his battle for a rationalistic relativistic physics:

In this moment I work hard at the EPR paradox for neutral kaon pairs. It is an area from which a final experimental resolution of the paradox can come, especially from the accelerators LEAR at CERN and DAFNE in Frascati, near Rome. A “final experimental resolution of the paradox” for me means a final breakdown of the infamous Copenhagen formulation of quantum theory.

It is the same philosophical battle that we are fighting for a rationalistic “relativistic” physics. (Selleri to J. Brandes, 14 January 1998-FSPA. My underline)

The lack of the “final breakdown” maybe represented one of, if not “the” reason for Selleri’s switch to FRT.

In the research programmes of his FQM group at Bari:

[...] the paradox of EPR is studied for pairs of neutral K mesons (kaons) produced either in the decay of the neutral vector meson phi, or in the antiproton annihilations at rest in hydrogen. Basic is (Selleri 1997, Incompatibility between LR and QM for pairs of neutral kaons). The formulation of LR already available in this research is much more general than any other one ever published on the same argument by different authors.

⁷⁸⁴ The original Italian text is in Addendum 3, p. 1040.

It contains only one “additional assumption” (very reasonable) that kaonic disintegrations are casual processes independent of all other events concerning the same particles, such as strangeness jumps. The proposer recently found a method allowing one to avoid even this last assumption and to formulate at empirical level the incompatibility between LR and QM (EPR paradox) in a way that, at this point, becomes absolutely general, deducing practical consequences purely from LR without additional assumptions of any type.

The algebraic (first phase) and numerical (second phase) development of this line of thought will be one of my research objects.

From the numerical point of view I will compare the LR predictions with the very recent experimental data of the international CPLEAR collaboration.

Concrete hopes for an imminent resolution of the EPR Paradox lie in the study of the pairs of neutral scalar mesons $K^0\bar{K}^0$ e $B^0\bar{B}^0$ produced with the newest accelerators from Frascati, Stanford and Japan (AGAB-Progetti di ricerca-Richieste finanziamenti ricerca scientifica dip di fisica armadio D settore 22 palchetto 4 busta 1).

Finally, he decided to write directly to Enzo Iarocci, the former new INFN president at LNF in Frascati, proposing him his papers, always in order to create interest for an experiment in Frascati:

Dear Iarocci, congratulations on your election as INFN president.

A long time ago I sent you my work on the foundations of quantum mechanics, now I take the opportunity to send you some recent articles, including one on the EPR paradox for neutral kaon pairs that could / should interest the new machine in Frascati⁷⁸⁵ (Selleri to Iarocci, 27 March 1998-FSPA).

In a paper written with his PhD graduate James Foadi, “Quantum Mechanics versus Local Realism and a Recent EPR Experiment on $K^0\bar{K}^0$ Pairs”⁷⁸⁶ Selleri stated how at CPLEAR an experiment on antiproton annihilations had been performed, and how a natural extension of this recent experiment could lead to a decisive step forward in order to reveal the 30% discrepancy between QM and LR.

⁷⁸⁵ “Caro Iarocci, congratulazioni per la tua elezione a presidente INFN. Tanto tempo fa ti mandavo i miei lavori sui fondamenti della meccanica quantistica, ora colgo l’occasione per inviarti alcuni articoli recenti, fra cui uno sul paradosso di EPR per coppie di kaoni neutri che potrebbe/dovrebbe interessare la nuova macchina di Frascati” (Selleri to Iarocci, 27 March 1998-FSPA).

⁷⁸⁶ Foadi R., Selleri F. (1999), “Quantum Mechanics versus Local Realism and a Recent EPR Experiment on $K^0\bar{K}^0$ Pairs”, *Physics Letters B*, 461 (1-2), pp. 123-130.

In a letter to the other supervisor of the Foadi thesis, Selleri underlined how the above paper sparks the interest of the experimental physicists of the CPLEAR at CERN:

The article [...] is finished. The following 10 pages are the preprint. It will be sent to *Physics Letters B* with a very good chance of being accepted: the days when research on the foundations was supposed to be forbidden are over, now we have access to the major journals. The CPLEAR researchers we are in contact with are very interested in the following⁷⁸⁷ (Selleri to Briatore, 11 December 1998-FSPA. My underline).

Selleri often shared his hopes to other scholars, as in the case of this letter to Prof. S. M. Roy, in India:

Dear Shasanka,

In the case of the EPR paradox I am placing great hopes in $K^0\bar{K}^0$ pairs from ϕ decays (DAΦNE expt in Frascati) and/or in antiproton annihilations at rest in hydrogen (CPLEAR coll. at CERN). Also the $B^0\bar{B}^0$ pairs produced in Stanford will be very interesting (Selleri to S.M. Roy, 6 January 1999-FSPA).

I propose again the letter to Gabathuler in its entirety, because Selleri revealed how Gabathuler measured the asymmetry parameter in the 1^{--} state and also cited for the first time other papers on similar subjects, confirming a wide interest in the topic:

Dear Erwin,

[...] The following 10 pages contain a paper (on the EPR paradox for neutral kaon pairs) submitted to *Physics Letters B*. They are the arrival point of a research that started in 1983. I am very satisfied about it, and I hope you will like it too.

In this paper we develop the most general predictions of local realism for $K^0\bar{K}^0$ pairs in the 1^{--} state for the asymmetry parameter which you measured. An important point of our theory is that we reproduce rigorously from the start the CP and S anticorrelations of the two kaons: they are non paradoxical (= compatible with local realism) and will probably be confirmed experimentally.

⁷⁸⁷ “L’articolo [...] è finito. Le 10 pagine seguenti sono il preprint. Sarà mandato al *Physics Letters B* con ottime probabilità che sia accettato: i tempi in cui le ricerche sui fondamenti erano tabù sono finiti, ora abbiamo accesso alle maggiori riviste. Gli sperimentali del CPLEAR con cui siamo in contatto sono molto interessati” (Selleri to Briatore, 11 December 1998-FSPA. My underline)

Other recent papers on similar subjects by Bramon & coll. and by Benatti & coll. do not contain this feature, i.e. they implicitly assume quantum mechanics wrong in this essential prediction (Selleri to Gabathuler, 11 January 1999-FSPA).

The completely general characterization of any possible local realistic theory of neutral kaon pairs is submitted in “Quantum mechanics versus local realism for neutral kaon pairs”⁷⁸⁸, written with Foadi. They again found marked disagreement with the standard quantum predictions.

At the beginning of 2000s, Selleri experienced a collaboration with Alexis Pompili on $B_d^0 \bar{B}_d^0$ pairs, proposing a possible experiment for Stanford and KEK in Japan. In the paper “On a possible EPR experiment with $B_d^0 \bar{B}_d^0$ pairs”⁷⁸⁹, they stated:

A very general local realistic theory of single B_d^0 mesons and correlated $B_d^0 \bar{B}_d^0$ pairs is formulated. If these pairs are produced in $Y(4S)$ decay, the local realistic asymmetry for observing pairs with like and unlike flavour at different proper times remarkably differs from the quantum mechanical prediction. Asymmetric B factories provide a powerful tool for the study of the EPR problem since the relative detection experiments can be shown to be capable of a time-dependent measurement precise enough to discriminate between local realism and quantum theory (Pompili, Selleri 2000, pag. 469).

They also submitted a paper, “Possible EPR Experiments with $K \bar{K}^0$ and $B_d^0 \bar{B}_d^0$ pairs” in the Proceedings of *Kaon 2001 International Conference on CP Violation*, at Pisa.

In this regard, Selleri wrote a letter to Lanceri, at the INFN in Trieste, on the above conference in Pisa and for seeking support for the experiment to be performed at BaBar⁷⁹⁰, at SLAC in Stanford, built to study B mesons produced by PEP-II storage ring:

⁷⁸⁸ Foadi R., Selleri F. (2000), “Quantum mechanics versus local realism for neutral kaon pairs”, *Physical Review A*, 61, pp. 446-452.

⁷⁸⁹ Pompili A., Selleri F. (2000), “On a possible EPR experiment with $B_d^0 \bar{B}_d^0$ pairs”, *European Journal of Physics C*, 14 (3), pp. 469-478. (Pompili, Selleri 2000) in the following.

⁷⁹⁰ BaBar: See <https://www.slac.stanford.edu/BFROOT/>. Accessed 24 September 2020.

Dear Lancerì,

After having interviewed Alexis, who suggested that I should contact you as the future coordinator for physics at BaBar, I would like to propose to you the very interesting opportunity to carry out a fundamental experiment at BaBar. It is the control of the predictions of quantum mechanics and the (incompatible) predictions of local realism.

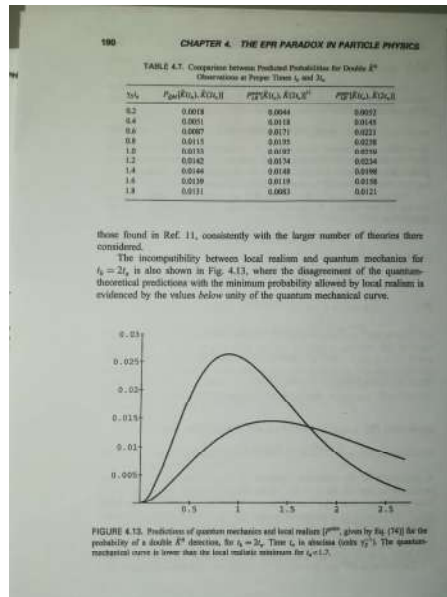
The experiment was proposed, as you may remember, in Alexis's article and published in the *European Physical Journal C*. I later presented the test at the Kaon 2001 in Pisa and at the 10th Lomonosov Conference in Moscow, and it received some interest. Now that BaBar has collected enough data and published the measure of Bd-mixing with dileptonic events, we think it is time to present our experiment to the BaBar community with greater emphasis. As a prelude I would like to debate with you to identify the best strategy that could lead BaBar within 12-18 months to publish the result of the proposed experiment. To begin with, Alexis suggested a theoretical seminar to be held during your next collaborative meeting to highlight the importance and relevance of the test that BaBar could carry out.

In the meantime, you could contact the heads of the B-mixing analysis group and/or the researchers who are already involved in B-mixing. It may then be considered to coagulate the strengths of some people or groups on this analysis.

Therefore, I invite you to think about a path that will lead, respecting the timescale of a complex experiment and in data collection, to coagulate within BaBar a group of physicists who will carry out this test. I am obviously very happy to offer my complete availability to this group⁷⁹¹ (Selleri to Lancerì, 2001-FSPA. My underline).

In (Afriat, Selleri 1999, Chapter 4, pp. 149-193) the incompatibility between QM and LR are shown in a table and in a figure. In the latter, the predictions of QM and LR for the probability of a double K_{0bar} detection are displayed:

⁷⁹¹ The original Italian text is in Addendum 3, p. 1041.



(Afriat, Selleri 1999, pag. 190)

So far I have provided some information on the laboratories where the experiments could be carried out, as well as Selleri's expectations for their implementation.

The main issue was that this kind of experiment were possible only in modern, big science-like laboratories, needed to be organized in a project, to crucially find the time machine in a jumble of other experiments, and so on. What happened, actually? Are the experiments indeed carried out or not? And if so, what results have they given?

Selleri focused his view at Finuda and Kloe projects at DAΦNE, in Frascati; at BaBar project at Stanford, with $B^0 \bar{B}^0_{\text{bar}}$; and at Belle experiment at KEK, at Tsukuba, in the Prefecture of Ibaraki, Japan, also on $B^0 \bar{B}^0_{\text{bar}}$.

With regard to the BaBar project, I found only the above letter to Lancieri and the paper written with Pompili on the opportunity to carry out an experiment at BaBar.

I can assert in conclusion that there has been no interaction or experiment, at least in the terms required by Selleri. This is one of the things to do in the future, another "what to do", to acquire further elements of the story.

For the other two possible venues, thanks mainly to Michele Barone and Vincenzo Patocchio, I found more detailed sources and information.

Concerning Frascati, the DAΦNE Physics Handbook⁷⁹² collected the work made by the DAΦNE Study Group, which on February 1992 submitted the outcomes in a conference, being Enzo Carocci the Director of LNF in Frascati. Looking at the Handbook, the DAΦNE accelerator was approved by INFN board in 1990 and consisted of a double ring e^+e^- collider with two intersecting regions and a project luminosity of $10^{33} \text{ cm}^{-2} \text{ s}^{-1}$. According to the editors (Luciano Maiani, Giulia Pancheri, and Nello Paver):

[...] the idea was to have a Report in which all relevant formulae, arguments, suggestions scattered in the literature had to be rederived from scratches and put in a form comprehensible and useful for the experimental groups which will work in Frascati (DAΦNE Physics Handbook, Foreword).

Several scholars from all over the world attended the DAΦNE Study Group. Among them, in respect of foregoing, people from Novosibirsk, Moscow, and Dubna, which maybe later tried to perform the same in Novosibirsk, where Selleri wrote in 1997. The topics addressed in the book ranged from CP, CPT physics to K decays, one photon processes, photon-photon processes, radiative ϕ -decays, spectroscopy issues above ϕ , low energy K-N interactions. And, related to my interest, to ϕ -decays and quantum mechanics. Giancarlo Ghirardi, together with his colleagues of Trieste University, Renata Grassi and Renzo Ragazzon, submitted in 1992 the paper “Can one test quantum mechanics at the ϕ -factory?”⁷⁹³. In the abstract and in the introduction, they stated:

The problem of possibility of testing Bell’s type inequalities in $\Phi \rightarrow K-K_{\text{bar}}$ is discussed. A comparison with EPR-Bohm type experiments for the singlet state and with Aspect’s experiments with optical photons is made. We show that, due to the specific features of the two meson systems, no violation of Bell’s type inequalities is implied by quantum mechanics.

⁷⁹² I warmly thank Vincenzo Paticchio for loaning me the DAΦNE Physics Handbook and the FINUDA technical report, on the FINUDA collaboration, being the latter published by LNF on 18 May 1995 and which was also attended by Vincenzo Paticchio himself.

⁷⁹³ Ghirardi G.C., Grassi R., Ragazzon R.(1992), “Can one test quantum mechanics at the ϕ -factory?”, *The DAΦNE Physics Handbook*, pp. 283-293. (Ghirardi, Grassi, Ragazzon 1992) in the following.

Finally, the deviations from quantum predictions which are implied by the assumption of “spontaneous factorization” are analyzed (Ghirardi, Grassi, Ragazzon 1992, pag. 283. My underline).

[...] we will analyze whether in the case of the $K\text{-}\bar{K}_{\text{bar}}$ mesons system produced in the decay of the vector meson Φ , $J^{PC} = 1^{- -}$, such inequalities can be used to test the predictions of quantum mechanics versus those of an arbitrary local realistic theory (Ghirardi, Grassi, Ragazzon 1992, pag. 284. My underline).

[...] if an appropriate number of semileptonic decays are collected in such a range, one could test the Furry’s hypothesis against quantum mechanics.

At any rate we think that Furry’s mechanism does not deserve too much attention since it meets serious difficulties in its theoretical description and, moreover, appears to have already been experimentally disproved [H. Albrecht et al. 1987] for analogous processes, typically B-meson decays (Ghirardi, Grassi, Ragazzon 1992, pag. 292).

They did not mention Selleri at all in their references, only (Aspect 1982), on his famous experiment (the most important one for testing QM against local realistic theories), Furry and Six. They analyzed only the “spontaneous factorization” hypothesis by Furry, which was found incorrect by Selleri himself. They underlined its difficulties and for them the matter is ended. There is no other theories or proposal to be taken into account.

And their conclusions seem clear: no space for an “arbitrary” local realistic theory! Their view, published in a book of “what to do” in the future at DAΦNE sounds conclusive against Selleri’s wishes.

Another link against the experiment comes from P.H. Eberhard and his paper⁷⁹⁴ at Lawrence Berkeley Laboratory (LBL), “Tests of Quantum Mechanics at a ϕ -factory”⁷⁹⁵.

As stated on the cover, expressly “prepared for the US Department of Energy” and “to be published as a chapter in *The DaPHine Physics Handbook*, L. Maiani, G. Pancheri, N. Paver, Eds, Vol. III, Servizio

⁷⁹⁴ My heartfelt thanks goes to Michele Barone for this paper and all the impressive documentation he gave me for this dissertation.

⁷⁹⁵ Written in August 1994. (Eberhard 1994) in the following.

Documentazione dei Laboratori Nazionali di Frascati, Frascati, Italy, 1994”⁷⁹⁶. Neither does he mention any Selleri work.

In the paper emerges how the violation are of the order a fraction of one percent, if estimates are made at the upper limit. But, even in the situation in which a great violation is found, as Selleri stated, Eberhard solves it using decoherence mechanisms. In the abstract, Eberhard stated that, even for great violation with the predictions of QM, decoherence explains the issue:

Unique tests of quantum mechanics, which can only be performed at a ϕ -factory, are proposed for Daφne.

Each of these tests consists of measuring the difference between the predicted and the actual amount of interference between two processes leading from a single pure initial state to a single pure final state of a kaon system.

Estimates are made of the upper limits that will be set for the amount of violation if the predictions of QM turn out to be correct. They are of the order a fraction of one percent.

For the case where, on the contrary, a significant violation is found, several decoherence mechanisms are considered (Eberhard 1994, Introduction. My underline).

Eberhard introduces the advantage and the great opportunity related to the ϕ -factory for testing QM:

Opportunities for testing quantum mechanics are unique at a ϕ -factory. Some of those that are suitable for Daφne are described here.

In the past, few checks of quantum mechanics have been performed in neutral kaon physics and none with a two-kaon system. At a ϕ -factory, several such tests are possible. The property tested is the predicted amount of interference between two different physical processes.

[...] The advantage of a ϕ -factory is the possibility to have initial kaon systems in quite pure states, [then] final decay modes that are theoretically equivalent to pure states, and practically no decoherence effect (Eberhard 1994, pag. 1).

⁷⁹⁶ Actually, it appeared on Vol. II, Chapter 2, “Topics in QM”, Par. 2.1, pp. 99-109. Anyway, I have used the original paper written at LBL, whose page numbering ranges from 1 to 12.

Eberhard quoted one of his papers, written in 1972 and unpublished, though, to recall how tests performed in the past using $K_L \rightarrow K_S$ regeneration have been proposed.

A test was made in 1976 by W.C. Carithers *et al*, with an amount of violation of the “decoherence parameter” equal to $3\% \pm 2\%$. Then he proposed to make it at DAΦNE, improving the accuracy, but with the conclusion that the violation has to be of 0.6%.

Other tests which he proposed “are designed to yield a low upper limit for [the decoherence parameter] if the predictions of quantum mechanics are 100% correct” (Eberhard 1994, pag. 1).

The seminal thing is that Eberhard states that the “decoherence parameter” measures the amount of violation of the theoretical prediction:

If a violation of quantum mechanics is ever revealed by one of the tests above, comparing the amount of violation found in each test will bring information about the decoherence mechanism responsible for the violation (Eberhard 1994, pag. 11).

Then Eberhard proposed several tests, as the measure of the number of pairs of K_L in the same event, and so forth.

Considering alternate theories, Eberhard made a clear cut, designing test to “detect effects described by alternate theories to quantum mechanics, more specifically by those which do not use hidden variables” (Eberhard 1994, pag. 8. My underline). In the conclusions, Eberhard argues that:

Significant tests of quantum mechanics that can be performed at DaΦne are proposed. They consist of measuring the amplitude of interference effects and comparing it to the predicted value. They may reach the relative accuracy of 10^{-3} .

[...] Tests of quantum mechanics in kaon physics were proposed already [in his unpublished paper of 1972]. One of the motivations was to look for possible spontaneous wave function collapses not involving an observer, because such effects would induce decoherence. That motivation is still valid.

[...] The latest theory assumes strangeness conservation in the decoherence mechanism and does not require “completeness”.

However, it is our opinion that it is better to keep as broad a perspective as possible and to perform all the tests proposed here, which test all the above theories, rather than only those that have the best chance to reveal a violation of QM in the context of the theory of the [following references]:

- J. Ellis, N.E. Mavropoulos, D.V. Nanopoulos, Phys. Lett. B 267 (1991) 465, Phys. Lett. B 293 (1992) 142, CERN-TH. 6897/93.
- P. Huet, M. Peskin, SLAC-PUB-6454 (1994), SLAC preprint, Stanford, CA, 94309.
- J. Ellis et al, Nucl. Phys. B 241 (1984) 395. (Eberhard 1994, pagg. 11-12. My underline).

In his conclusions, Eberhard prefers all experiments to be carried out, not just those that may reveal violations. And even if they were also done, they would only be the ones to be done in the same theories whose references he quoted above. So, in any case, no other experiment is recommended or foreseen, and neither seems to be given any chance to Selleri's idea, since he is not even mentioned.

Moreover, advising to do all possible tests is like saying not to do them, because the experiments go in the queue. It is one thing to do, as I can say, a test together with 100 others already foreseen in the accelerator. It is another thing to put all the tests instead, so the test we are interested in is not together with 100 others but with 1000 others, let's say, 99% of which are useless confirmations of what we already know. What you get is not to do the suspicious test, which will be in the queue with many other standard tests, which by the way will make you say that the QM confirmations are many.

To sum up, using decoherence mechanisms still bypassing the explanation made by Selleri and, *de facto*, making the Selleri's proposal of an experiment basically worthless. After these paper, Selleri's wish to have found a way to resolve the EPR paradox and even the disagreement between QM and LR seems to be over yet.

The article may have proved vital, also taking into account what was said earlier about Eberhard, who considered himself a local realist like Selleri. The attention will be focused only on CP and CPT violation at DAΦNE and seems to underestimate the application proposed by Selleri.

Michele Barone gave me a pivotal mail sent by Selleri to him in 2003, related to the NA48 experiment at CERN and to his result.

It is a clear explanation of why, when possible experiments have been made, they generally only studied the CP-violation, really useless for Selleri:

From: Franco Selleri

To: Michele Barone

subject: EPR and NA48 experiment outcomes

[...] I deliberately ignore the violation of CP which gives effect to one in ten thousand.

Thinking only of the difference between local realism and QM, I find that:

1. LR and QM coincide for the properties of single kaons;
2. LR and QM differ by 30-50% for $K-K_{\text{bar}}$ correlations.

The experiments [done] on CP are type 1 and nothing can be said about the QM/LR comparison.

The data should be analyzed in a non-correlated way, if possible, if not make a new experiment.

Even for type 2, experiments there will be type 2 violations but the order of magnitude 10^{-4} (1 out of 10^4) makes it impossible to erase the 30-50% differences between QM and RL. All right? Franco⁷⁹⁷ (Selleri to M. Barone, 11 February 2003-MBPA).

This further clarifies what Selleri wanted from the experiment, with the clear differences between individual and correlated kaons. An experiment that, in practice, has not been done.

Indeed the NA48⁷⁹⁸ experiment “is a CERN experiment [...] which is taking data to study direct CP violation in the neutral kaon system” (<http://na48.web.cern.ch/NA48/>). On this website, the official page, is stated: “Welcome to the CP violation experiment”.

⁷⁹⁷ Da: Franco Selleri

A: Michele Barone

Oggetto: EPR e risultati dell'NA48

[...] Io ignoro volutamente la violazione di CP che dà effetti di uno su diecimila. Pensando solo alla differenza fra realismo locale e MQ trovo che:

1. RL e MQ coincidono per le proprietà dei kaoni singoli;

2. RL e MQ divergono del 30-50% per le correlazioni K-Kbar.

Gli esperimenti [fatti] su CP sono del tipo 1 e nulla possono dire sul confronto MQ/RL. Bisognerebbe analizzare i dati in modo non correlato, se fosse possibile, se no fare un nuovo esperimento. Anche per esperimenti del tipo 2 ci saranno violazioni di tipo 2 ma l'ordine di grandezza 10^{-4} (1 su 10^4) rende impossibile che cancellino le differenze del 30-50% fra MQ e RL. Chiaro? Franco. (Selleri to Barone, 11 february 2003-MBPA).

⁷⁹⁸ See https://en.wikipedia.org/wiki/NA48_experiment; <http://na48.web.cern.ch/NA48/>. Both accessed 24 September 2020.

According to NA48 experiment:

The discovery of the phenomenon of direct CP violation, one of the most important experimental results obtained at CERN, was announced by the collaboration in 1999. The publication of the final result was made in 2001. In addition the experiment made a contribution to studies of rare decays of neutral kaons.

The following stage of the experiment (NA48/1) was carried out in 2002 and was devoted to high precision study of rare decays of neutral kaons and hyperons. The next stage (NA48/2) was carried out in 2003-2004 and was dedicated to a large programme of studies of properties of charged kaons, including the search of direct CP violation, studies of rare decays of the charged kaon, and low-energy QCD using final state rescattering. The successor of NA48 is the NA62 experiment, which started data collection in 2015 and is dedicated to further studies of rare decays of the charged kaon

(https://en.wikipedia.org/wiki/NA48_experiment).

Regarding the FINUDA project at Frascati, a great involvement for the hypernuclear physics, due to the opportunity to carry out nuclear physics experiments in a collider and not in a fixed target machine, Vincenzo Paticchio told me during our interview that:

Franco mentioned something to me informally. And at that time I talked about it with the person in charge of the experiment, Tullio Bressani. But the thing as it began died in the bud. It expired because there was already a very dense schedule, and we were ancillary to the most important experiment, that of KLOE, deputy of the elementary particles. The machine time was almost entirely devoted to KLOE (Interview with Vincenzo Paticchio, Bari 29 May 2019).

Therefore, Selleri's specific approach to FINUDA is only informal, at the hypothesis level.

Actually, there were many experiments already expressed and structured, even on the DAΦNE Physics Handbook I mentioned above, which were not done.

In MBPA at Athens I found this fax sent on 29 October 1993 by INFN-LNF to Group III-Nuclear physics of INFN Bari, containing a seminar announcement:

Looking into the future with Finuda at DAΦNE- Purpose: quantum mechanics of time→the future of causality. With the attendance of:

Widom A.-Boston

Srivastava Y.-Perugia

Sassaroli E.-Perugia,

and the reference at the paper of the former two scholars, that is, Srivastava Y., Widom A. (1993), “Interaction between past and future events in $\Phi \rightarrow KK$ decays”, *Physics Letters B*, 314, pp. 315-319.

This seminar, together with the Srivastava-Widom paper, could be the one of which Selleri stated (see page 590 above) as a “conceptual devastation” for his hopes to perform an experiment in Frascati.

Thanks to Michele Barone I had also the opportunity to read papers and outcomes related to the Belle⁷⁹⁹ experiment at KEK⁸⁰⁰ in Tsukuba, Japan. He submitted to me a seminar made in November 2003 at CERN⁸⁰¹; and a paper released on 2 March 2007⁸⁰².

In the former, the author Apollo Go, from Belle collaboration, stated that:

According to QM, a pair of particles created in any interaction exhibits EPR type of non-local correlation.

[...] Bell Inequality [...] has since been tested with polarization in photons and spin in atoms but never with particle-antiparticle in elementary massive particles. In particle physics, both $\Phi \rightarrow K^0 \bar{K}^0$ and $Y(4S) \rightarrow B^0 \bar{B}^0$ exhibit the same EPR non-local correlation. In this talk, I will discuss [...] the possibility of testing Bell Inequality. Finally, the preliminary result of violation of Bell Inequality using semileptonic B^0 decays at BELLE experiment in KEK will be presented (Testing Bell inequality with B-mesons at BELLE experiment-Apollo Go).

⁷⁹⁹ <https://belle.kek.jp/>. There is also a Belle II, <https://www.belle2.org/>. I will speak of the latter in the following. Both sites accessed 24 September 2020.

⁸⁰⁰ KEK is the Japanese acronym for The High Energy Accelerator Research Organization-*Kō Enerugi Kasokuki Kenkyū Kikō*. Makoto Kobayashi, emeritus professor at KEK, is known globally for his work on CP-violation, and was awarded the 2008 Nobel Prize in Physics. See. <https://en.wikipedia.org/wiki/KEK>; <https://www2.kek.jp/accl/eng/>; <https://www.kek.jp/en/>. All accessed 24 September 2020.

⁸⁰¹ CERN EP Seminar- 17/11/2003-Testing Bell inequality with B-mesons at BELLE experiment by Apollo Go (National Central University, Taiwan).

At the website <https://indico.cern.ch/event/415662/attachments/851545/1187669/apollo.pdf>, there are the slides of his talk. Accessed 24 September 2020. He also published “Observation of Bell Inequality violation in B mesons” on arXiv. [arXiv:quant-ph/0310192v1](https://arxiv.org/abs/quant-ph/0310192v1). Apollo Go-arXiv in the following. Accessed 24 September 2020.

⁸⁰² /03/2007 KEK-High Energy Accelerator Research Organization-Youhei Morita, KEK Public Relations Office, “Belle confirms quantum entanglement at 10 Billion electron volts”.

Entanglement in particle/antiparticle Hilbert space exists for $\Upsilon(4S) \rightarrow B^0 \bar{B}^0$.

- With correction to the intrinsic loss (decays), one can form the Bell-CHSH Inequality.
- Experiment was carried out by looking at semileptonic decays for the B^0 s at BELLE experiment in KEK, Japan
- A violation of Bell Inequality of $>3\sigma$ is observed: $S(\Delta t=2\text{ps}) = 2.725 \pm 0.167_{\text{stat}} \pm 0.092_{\text{syst}}$

First ever Bell Inequality measurement with particle/antiparticle quantum number.

- The result is consistent with QM prediction (with detector resolution, efficiency, and background folded in)

(<https://indico.cern.ch/event/415662/attachments/851545/1187669/apollo.pdf>. *Italics in the text).*

In his paper on arXiv (see footnote 801), Apollo Go also argued that:

A pair of $B^0 \bar{B}^0$ mesons from $\Upsilon(4S)$ decay exhibit EPR type non-local particle-antiparticle (flavor) correlation. It is possible to write down Bell Inequality (in the CHSH form: $S \leq 2$) to test the non-locality assumption of EPR. Using semileptonic B^0 decays of $\Upsilon(4S)$ at Belle experiment, a clear violation of Bell Inequality in particle-antiparticle correlation is observed: $S = 2.725 \pm 0.167_{\text{stat}} \pm 0.092_{\text{syst}}$.

[...] Bell Inequality is a very powerful test on local hidden variable theories. It has never been tested in particle-antiparticle correlation using massive elementary particles. We measure the Bell-CHSH inequality using semileptonic B^0 decays. A violation greater than 3σ is observed. MC simulations confirm that the result is consistent with QM predictions (Apollo Go-arXiv).

Even though performed in a CHSH additional assumption framework, the “First ever Bell Inequality measurement with particle/antiparticle quantum number” found an agreement with QM.

The second paper, a press release, actually, is “Belle confirms quantum entanglement at 10 Billion electron volts”⁸⁰³, written by the Belle group in 2007. It is maybe one of the first work that I found in which is specifically stated a recall to a Pompili-Selleri model, seen as an alternative to QM. The authors of the Belle group stress that the results are consistent with QM:

Previous experimental tests of quantum mechanics have been carried out with optical energy photons, systems of atoms and kaons (particle containing strange quarks).

⁸⁰³ (Belle 2007) in the following. The press release is no more available on web.

Using pairs of B mesons (particles containing b quarks) the Belle experiment at KEK in Tsukuba, Japan, has submitted a paper to *Physical Review Letters* [...], which reports tests of quantum mechanics at the 10 Billion electron volt energy scale, which is significantly beyond the energy scale of previous tests.

Belle tests quantum mechanics by measuring the time dependence of decays of B mesons that are produced from the decays of the Upsilon(4S) resonance at the KEKB electron-positron collider. The decay products are reconstructed in the Belle detector.

By closely examining the time dependence of pairs of B meson decays, the Belle experimenters conclusively rule out two alternatives to quantum mechanics; spontaneous disentanglement in which the two B mesons evolve independently and another alternative called the Pompili-Selleri model.

The Belle results are consistent with the prediction of quantum mechanics, where each pair of B-mesons behaves as a single “entangled” object. The data have now been placed on public record, and can be used to test any other alternative models that are devised by theorists in the future (Belle 2007. My underline).

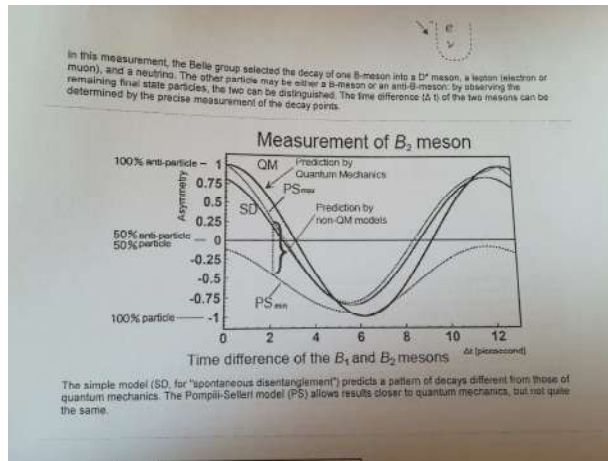
Then they explain the framework of the experiment, giving at the end of (Belle 2007) two figures with the outcomes:

A B-meson and an anti-B-meson change into each other. The probability of observing one type of meson oscillates as a function of the time. The phenomenon is called $B\bar{B}$ oscillation. At the KEK B-factory, a B-meson and an anti-B-meson are generated as a pair. In a simple model of this process, each meson then oscillates independently between B and anti-B. If this were true, a distinctive pattern of results would be seen: sometimes B and B, sometimes B and anti-B and so on.

But, according to quantum mechanics, the two mesons are “entangled” to form a single object. The first meson is seen as a B 50% of the time and as an anti-B 50% of the time.

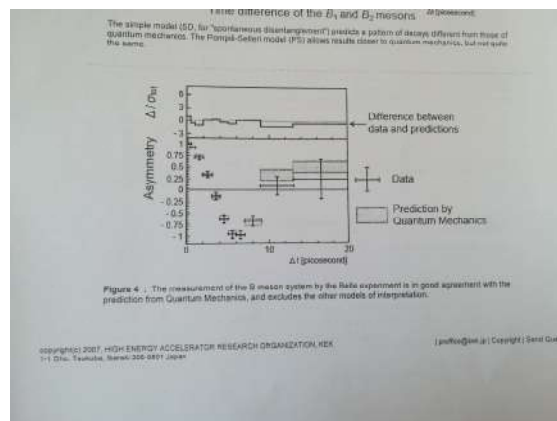
The second meson is in the opposite state, and then oscillates between B and anti-B as time passes. The patterns of results (B and B, B and anti-B, ...) is different from the simple model. Another model (the Pompili-Selleri model) avoids entanglement and can produce results close to the quantum mechanical prediction, but is different in detail.

In this measurement the Belle group selected the decay of one B-meson into a D^* meson, a lepton (electron or muon) and a neutrino. The other particle may be either a B-meson or a anti-B-meson: by observing the remaining final state particles, the two can be distinguished. The time difference (Δt) of the two mesons can be determined by the precise measurement of the decay points (Belle 2007. My underline).



The simple model (SD for “spontaneous disentanglement”) predicts a pattern of decays different from those of QM. The Pompili-Selleri model (PS) allows results closer to quantum mechanics, but not quite the same.

The measurement of the B meson system by the Belle experiment is in good agreement with the prediction of QM and excludes the other models of interpretation.



The above works and outcomes marked a watershed, because now there was the no longer theoretical but experimental effectiveness, which sought precisely the possible violations of Bell’s inequalities. Perhaps the approach of Selleri was not used, but they are certainly a different approach, in my opinion, from the view seen for Frascati.

Concerning Belle, Barone sent me a mail which could be considered as a possible legacy of Selleri’s work, if it should happen. He wrote me on the chance to made another experiment at Belle, and to try finding a space to make a Selleri-like proposal:

[...] At CERN I followed the kick off meeting of the BELLE II collaboration. I asked the spokesperson to resume the experiment on hidden variables done by BELLE. The new collaboration was not aware of the experiment done in the past, they will inquire and decide. With the new modifications to the machine the brightness will increase by a factor of 10, producing much more K. [...] I talked to Prof. F.Forti in Pisa about it, you should get his OK if you want to mention it in your thesis.⁸⁰⁴ (M. Barone communication to the present author, 19 July 2019).

I very recently wrote to Prof. Forti who very politely answered me real soon giving some information:

[...] Both Babar and Belle experiments studied the problems related to entanglement in quantum systems, linked to hidden variables. Belle made an EPR measurement (<https://inspirehep.net/literature/745605>), while Babar has measured the asymmetry of T (<https://inspirehep.net/literature/1209711>) strongly linked to entanglement (<https://inspirehep.net/literature/1091168>).

As far as Belle II is concerned, it will certainly extend these measures and probably make others, but to give significant results it will need full statistics, which will only be available in a few years (probably at least 5). Also for this reason I have to say that at this moment there is not a very strong push in the collaboration to develop these analyses, although I am sure there will be.

Please let me know if this information is enough or you would like more details. Best regards⁸⁰⁵ (Prof. F. Forti to the present author, 21 September 2020. My underline).

⁸⁰⁴ “[...] Al CERN ho seguito il kick off meeting della collaborazione BELLE II. Ho chiesto al porta parola di rifare l’esperimento sulle variabili nascoste fatto da BELLE. La nuova collaborazione non era al corrente dell’esperimento fatto nel passato, si informeranno e decideranno. Con le nuove modifiche alla macchina la luminosità aumenterà di un fattore 10, producendo molti più K. [...] Io ne ho parlato al prof. F.Forti di Pisa. Tu dovresti avere il suo ok se vuoi citarlo nella tua tesi” (M. Barone communication to the present author, 19 July 2019).

⁸⁰⁵ “[...] Sia l’esperimento Babar sia l’esperimento Belle hanno studiato i problemi legati all’entanglement nei sistemi quantistici, legati alle variabili nascoste. Belle ha fatto una misura di EPR (<https://inspirehep.net/literature/745605>), mentre Babar ha misura la asimmetria di T (<https://inspirehep.net/literature/1209711>) fortemente legata all’entanglement (<https://inspirehep.net/literature/1091168>). All sites accessed 24 Septemebr 2020.

Per quanto riguarda Belle II, sicuramente estenderà queste misure e ne farà probabilmente altre, ma per dare risultati significativi avrà bisogno della piena statistica, che sarà disponibile solo tra alcuni anni (probabilmente almeno 5). Anche per questo motivo devo dire che in questo momento non c’è una spinta molto forte nella collaborazione per sviluppare queste analisi, anche se sono sicuro che ci saranno. Mi faccia sapere se queste informazioni le bastano o vorrebbe maggiori dettagli. Cordiali saluti” (Prof. F. Forti to the present author, 21 September 2020. My underline).

So, there is some tight, little room to think for the experiment to be carried out. And, mainly, the issue of hidden variables in EPR experiments will probably still be the subject of current debates.

To end this section, I mentioned here below three evidences acquired through interviews, the latter of which is made by Freire Jr.

The first evidence rests in an interview to Michele Barone released in almost two steps. He stated giving clearly answers which constitute relevant cues on trends involved in the choice of the experiments to be carried out as well as on Selleri's attitude against the odds:

AUTHOR:

In Frascati, Franco made several proposals for an experiment with kaons to discern between QM and LR

BARONE:

Yes, in fact I know it well! I was there!

AUTHOR:

Why was not it done?

BARONE:

He did not get performed because Biaggini at Frascati had written an article in which he wrote that he did not believe in this thing. They wanted to make the machine run for other events. There were other experimental groups to be privileged.

AUTHOR:

And in fact I think Selleri proposed it in Japan.

BARONE.

It's not clear whether Franco proposed it or whether they left independently in Japan. The fact is that they sent the data to Franco who analyzed them on the decay of KL and KS. **And the result of the experiment was negative.** Rubbia, when I went to him, had done an article on the decay of KL and KS. I have just Franco's comments on this article.

I knew those who in Frascati were in favour of the experiment and they told me that the proposal had not passed. A question of groups and experiments to be privileged. I do not know how much Franco insisted. Also because he had this slightly snobbish attitude. He always thought that he had the most pleasant ideas of all and that it was the others who had to pray to him. If they once said no to him, he would tell them to go to hell (Interview with Michele Barone, Bitonto (BA), 1 June 2019. My underline and bold).

In the second step, Barone talks about Eberhard and the importance of his paper. Anyway, even in the following there are several precious hints, both technical and political:

[...] What are the experiments that could be done? Here is an article made by Eberhard in which he explains what could be done in Frascati about FQM. He says, in the conclusions, that two types of tests could be done. A test that measures the average life of particles, a test that goes through CP validation. And another test that could be done on the basis of hidden variables. This last one was proposed by Franco. In these calculations Eberhard says that it makes more sense in this machine to test CP violation than to test hidden variables. Because the amount of data that would have accumulated for the first case was much higher than for the second case (useful in general and also as a significant statistic).

And I think that this was the reason that then convinced people of Frascati not to do the experiment proposed by Franco. CP violation was more important for physicists than the revelation of the presence of hidden variables.

In the Frascati research group they thought it was no longer appropriate to do the experiment. The others did not see the problem of hidden variables. They thought, in Eberhard's words, that there is a decoherence effect and therefore it is not worth doing Franco's experiment. Decoherence destroys everything and you can't observe anything! Franco was also in contact with Lee-Franzini [or D. Franzini?], who was the spokespecial. And Franzini was pushing to do the experiment. But then he did not check anything anymore (Interview with Michele Barone, Athens, 21 June 2019. My underline and **bold**).

The second proof comes from Augusto Garuccio, in our interview. He analyzes the reason for Selleri's shift to FRT and then stresses the technical difficulties in kaon experiments:

By that time [2000] Franco had cooled down on the locality and focused on the problem of relativity.

I did not follow attentively how Franco detached himself from this problem. My impression is that he, having finished the initial momentum, since there was this series of experiments that in a certain sense showed in a more and more massive way that the results of the QM were correct, thought that this space was now closed. He had thrown himself on kaons, on which I did not follow him because I did not know the subject well. So he worked a lot with others. At that time there was great interest in the topic and the opportunity to do this.

It seemed to me that there were inherent difficulties and therefore there was a risk that everything on paper that looked beautiful, elegant, clean, in practice required additional hypotheses even greater than those present in optics. Now a PDC apparatus can be bought not online but in almost all physics laboratories. And so technology has helped us a lot, with medium power lasers, with crystals that cost a maximum of 1000 euros and a few counters you can simulate this experiment well. You do not do it at the limit of the loophole but you do it. All the problems remain inside and yet the great moment of the great hope of having the anomalous experiment has passed. I guess it was not done.

AUTHOR:

In Frascati there was no machine time.

GARUCCIO:

If there had been this experiment of optics with an uncertain result the climate would have changed, because then that would have become the crucial experiment. But in a period in which I do not say every month, but every 3-6 months an article was published with violation of the Bell's inequalities of 5, 7, 11 standard deviation, in a certain sense, having to have a list of experiments, I can also understand that such an experiment goes in second order. Uncertainty becomes important but even if you do it is no longer crucial, you are the 30th in a certain series. However, it has to be said that entangled states are not yet well understood by people. Now you understand the importance. Nothing was known then (Interview with Augusto Garuccio, Bari 1 July 2019. My underline and **bold**).

The third and last evidence is in the interview made by Freire Jr. with Selleri himself. Once again, it emerges the importance of experimental physics in Selleri's attitude:

SELLERI:

[...] the experiments were for me always very important; I always kept my eyes on experimental science because otherwise doing pure theory is nonsense. I mean you can create anything, you can invent a thousand theories but if you don't have the control of experiments how do you know that what you are doing has a chance to be correct? So you are right to say that interest for experiments is a constant of my way of thinking. In the beginning I thought that it was a constant for everybody. Later I realized that it was not so.

[...] recently I published papers on particle physics for the EPR paradox. A system of two correlated neutral kaons can be very useful for checking the Einstein-Podolsky-Rosen paradox and Bell's theorem, because high-energy detectors have a very high efficiency, so there is not the problem of two types of inequalities.

In particle physics you can really choose between local realism and quantum mechanics. In principle. In practice it is very difficult to convince people because there is a strong pressure coming from other more traditional problems. To convince those people to do an important experiment on the EPR particles is not easy. Also because they find some theoreticians telling them that the EPR paradox is already solved in favour of Bohr. So it is a waste of time they say, wrongly.

FREIRE:

The first time you have suggested these experiments was —

SELLERI:

With kaon pairs it was in '83.

So people know that I do not believe that experiments of Bells theorem are final and that newer experiments are needed — for example the experiments about which I gave you a couple of papers yesterday, with K^0 , K_{bar}^0 pairs which allow one to avoid the additional assumption.

So for those particles the strong inequality and the weak inequality - perhaps they are not identical but they are very near for kaon pairs, so it is easy to discriminate quantum mechanics really. My feeling is that (talking on the average of course every man lives the situation in a different way) on the average people do not want to discover the truth about Bell's theorem. I think that is the average case. Although maybe this average does not correspond to any single human being, as it is very difficult to imagine a physicist who thinks, "Probably Bell's theorem shows that quantum mechanics is wrong, but this I will cover up." This is clearly not the right psychological way. But finally it comes out to the same (AIP 2003. My underline).

They are prominent, paramount proofs of Selleri's approach to the matter. Selleri, at a specific question on the reaction of scientific community, so stated:

[...] I was invited to a conference in Rome. In the year 2000 there was an international conference at the University of Rome devoted to such matters. More recently I was invited in Pisa in the year 2001 at the conference Kaon 2001. That is the title of the conference, Kaon 2001, and I presented this idea to the particle physicists who were there and so on.

I presented my ideas to the particle physicists and they were interested and there was positive reaction at that moment, but I do not know if any real plan for experiments is going on to test this. I still hope that eventually it will be done.

It so happens that the best place in the world to do such an experiment would be Italy, Frascati, because we have a phi factory accelerator called Daphne.

[...] Other places where my experiments could be carried out are Stanford and an accelerator in Japan, but they are not kaon producers, but they rather produce B mesons. B mesons are more difficult to analyze because they have a higher mass, many decays and it is more complicated. The best would be to do it really at this lower energy accelerator. As you see thousand and twenty MeV is just above the threshold for a two kaon production. **But I am not willing to disturb people too much. I publish, I send papers, I go to conferences to present my ideas, I talk to single people when they come, but after that I am not the type of person who runs after people pushing them with too much pressure. It is up to them to understand that there is a problem and that they could do the experiment** (AIP 2003. My underline and **bold**).

The above three interviews show how Selleri's attitude was related to a certain fairness together with a haughtiness, according to which his concepts are always clear and people have to understand that it is the best thing to do and have to start doing things concretely by their own.

Looking at Selleri's words (which in 2003 talked about the opportunity of experiments to be performed in a mood quite absent, still ignoring whether some experiment is to be carried out or not⁸⁰⁶) could reveal how far he felt the FQM research in the moment of the interview, being immersed in the new topic of FRT.

In the foregoing three interviews are also highlighted the seeds of his passage to Foundations of Relativity Theory (FRT), an issue I will analyze in the following section.

2.10-THE SECOND TURNING POINT. POSSIBLE REASONS FOR SELLERI'S MOVE TO FOUNDATIONS OF RELATIVITY THEORY

I have just shown some reasons that may have prompted Selleri to switch to FRT studies. Garuccio's words in the last section give a good idea.

The transposition of Aspect's experiments as conclusive and definitive against the LR, the failure of the experiments on neutral kaons, either not done or with negative results, the widespread attention to his work but without a concrete impact on the fundamental decisions at the level of major

⁸⁰⁶ Of course is only my perception, quite enhanced, actually, by his words to Freire and by the statements by Garuccio which refers to Selleri stay at Baltimore during the 2000s.

institutions (laboratories, etc.), the lack of a school organised by Selleri himself, which could also produce scientists who could in the future occupy decision-making positions, why not. Only many sympathetic scholars, who also continued to research Selleri's proposals but acted as individual islands with little or no connections. And whose efforts were stuck with their disappearance or possible incorporation.

As Selleri himself stated to Freire Jr. on the possible development of a school:

[...] About my collaborators, it is not a very easy story to tell because they had to change activity to find jobs. It was not possible to keep them. For a long time I thought this was pure repression.

Then I came to realize better that it was not only that, but it was also the fact that I was not powerful enough. Everybody tries to grab as much as possible for himself. If your hands are not long enough you get nothing. So my pupils had to find different jobs.

[...] doing foundations of quantum mechanics did not allow them to get a job. So it is difficult to say that I made a school. There are a lot of disconnected people doing different activities. We are still in contact, we are friends, we talk and so on, but everybody is doing his thing (AIP 2003. My underline).

And Barone emphasized more:

I said to him one day: "Franco, you have to think about the afterwards. You have to create a school. Because this is the only way your cultural heritage can remain". And he, he did not tell me clearly, he told me that he was not capable of being the director of a group and that he preferred to ally himself individually with each person. He did not want to create a Selleri school, but he allied himself, depending on the purpose of his research, with the people he thought would be good enough to serve it better. And who were scattered all over the world (Interview with Michele Barone, Bitonto (BA), 1 June 2019. my underline).

Although, as I have just said and I will try to explain in the next paragraph on Selleri's legacy, there are several scholars who publish on the issues addressed by Selleri, quoting him and analyzing his hypotheses.

And maybe even at the level of experiments something could change, if we think about Belle II (provided that there is an acceptance of the proposal).

Concerning the failure of his requests for an experiment with kaons, Selleri took stock of his struggle against the Copenhagen interpretation of quantum theory, still revealing the need for a change due to a stalemate situation:

So now I understand that in order to find an inconsistency in the theory it is not enough to find it in one branch, you have to find it in all of them. Really quantum mechanics from the epistemological point of view is not a well defined theory. It is rather a set of more or less parallel theories. In practice this situation is not a weakness of the theory, it is a strength. Of course it is a disaster scientifically to have such ambiguities in a fundamental theory, but people do not care so that the ambiguity becomes practically a strength, as it is very difficult to kill all the interpretations together. Only the EPR paradox can do that. It's a decision that could be made with a suitable experiment. But then people don't do the experiment and that's the end of the story (AIP 2003).

The occurrence of all these arguments probably pushed Selleri that it was not possible to change the perspective towards the LR, that the spaces were closed. His attitude to face in a global way the great problems of the 20th century in physics, together with his philosophical and even political vision, pushed him to analyze the other great theory of the century, the theory of relativity.

In Freire's interview, which in any case is in retrospective and therefore contains a vision already linked to a thorough analysis of his developments in FRT, Selleri claimed that:

You see, my research in the foundations of quantum mechanics has the following strong points, the way I see it. First, the proposal of experiments to detect the objective reality of the probability waves. Second, showing that the inequalities violated by experiments are very different from the original Bell's inequalities in many ways, so that they do not constitute a violation of local realism.

And third, proposing a set of new experiments with K mesons which could really allow one to discriminate between the Copenhagen approach and local realism. That is what I know. But this is not a new theory. It is only a set of ideas. But in relativity it is different. With relativity I could go much deeper and I have basically a new theory that can replace special relativity (AIP 2003).

However, the complete transition to FRT was very long, with several works both in FQM and FRT, lasting more than 12 years, as can be seen from the primary bibliography, organized in a chronological sense. In this regard, I remember that the transition between PP and FQM lasted, as I showed, about 3 years.

There are a lot of letters sent by Selleri to several scholars in which he told about his research. And in all of them, he attached comments on FQM papers, specifically on kaons, and papers on FRT.

I submit here only a few of them, going between 1996 and 2000, sent to Miguel Ferrero, Gerald Holton, and Dipankar Home:

Dear Miguel, [...] this time I will arrive in Oviedo with the written contribution ready for the proceedings. There are two possible speeches that I can make:

1. EPR paradox for $K^0\bar{K}^0$ pairs. This is the continuation of my speech at the first conference in Oviedo. It is about particle physics, a bit technical.
2. Noninvariant one-way velocity of light. This is a speech more immediately loaded with history and philosophy. I have found a serious alternative to special relativity which in my opinion is better than the original.

The choice between the two possibilities is up to you. I would prefer the second argument because it is comprehensible to everyone, and I know from experience that it may interest the audience. However, Oviedo's lecture is a conference on quantum mechanics and one could also say that relativity has nothing to do with it. Please decide which of the two speeches I will make and tell me as soon as possible⁸⁰⁷ (Selleri to Miguel Ferrero, 12 January 1996-FSPA).

Dear Professor Holton,

Recently I read two of your books in their Italian translations and found that your work is very important and that I fully agree with your thesis that the so called "external" history is essential for a correct understanding of the nature of physics.

I am a theoretical physicist who devoted a lot of energy to a critical study of the foundations of modern physics and came to know about your ideas initially from Mr. Tommaso Ciccarone's 1995 thesis of which I was somehow a referee. Perhaps you might be interested in some of my papers. With this hope I am sending you the following:

⁸⁰⁷ The original Italian text is in Addendum 3, p. 1041.

1. Complementarity and/or quantum theory, which is critical of Bohr's epistemological position and in favour of Einstein's views. The historical point of view is briefly represented in section 6.
2. Fundamental problems of quantum physics, which is a contribution to a written debate and has the advantage of being very short.
3. Noninvariant one-way velocity of light which opens with a detailed criticism of the relativity principle (Sec. 1-5) and concludes that the transformations between inertial systems that should be adopted are those given in Eq.s (18)-(20)-(22), called "inertial transformations".
4. The relativity principle and the nature of time is an historical review of the subject ending with some original ideas in the last section.
5. There is a serious logical problem in the special theory of relativity (not for publication in this form, but quite firm in the conclusion) which presents in 2 pages what to me appears to be the main contradiction within SRT.

If you found time to go through some of this material I would be very interested in knowing your reaction. With the hope of not abusing of your kindness I send you best regards [...] (Selleri to Gerald Holton, 3 March 1997-FSPA).

Dear Dipankar,

I am now sending to you 4 papers via airmail.

Two concern the EPR paradox in particle physics for $K^0\bar{K}^0$ pairs and for $B^0\bar{B}^0$ pairs. You can see from the figures that the discrepancies between LR and QM are very large in these reactions. An experiment becomes thus fully decisive for the survival of QM. Hoping that there is no experimental cheating, we should see the resolution of the paradox pretty soon. Machines that can allow these experiments exist in Stanford, in Japan (KEK) and in Frascati.

The other two papers deal with relativistic physics. They are reviews of the work I have done on this field. Perhaps they are similar to other papers you saw in the past, but they also contain new material, e.g. the treatment of radar ranging, of aberration and of Jupiter's satellites occultations. The very firm conclusion is that $e_1=0$: the right theory is not SRT, but a different one in which space and time do not mix (absolute synchronisation). I recently lectured on this matter in USA with very remarkable success.

Your comments on my results are very welcome (Selleri to Dipankar Home, 30 March 2000-FSPA).

I did an inspection of the list of Selleri's publications related to the foundations of quantum mechanics and the foundations of relativity theory, even introducing a statistical distribution of his papers in these two fields.

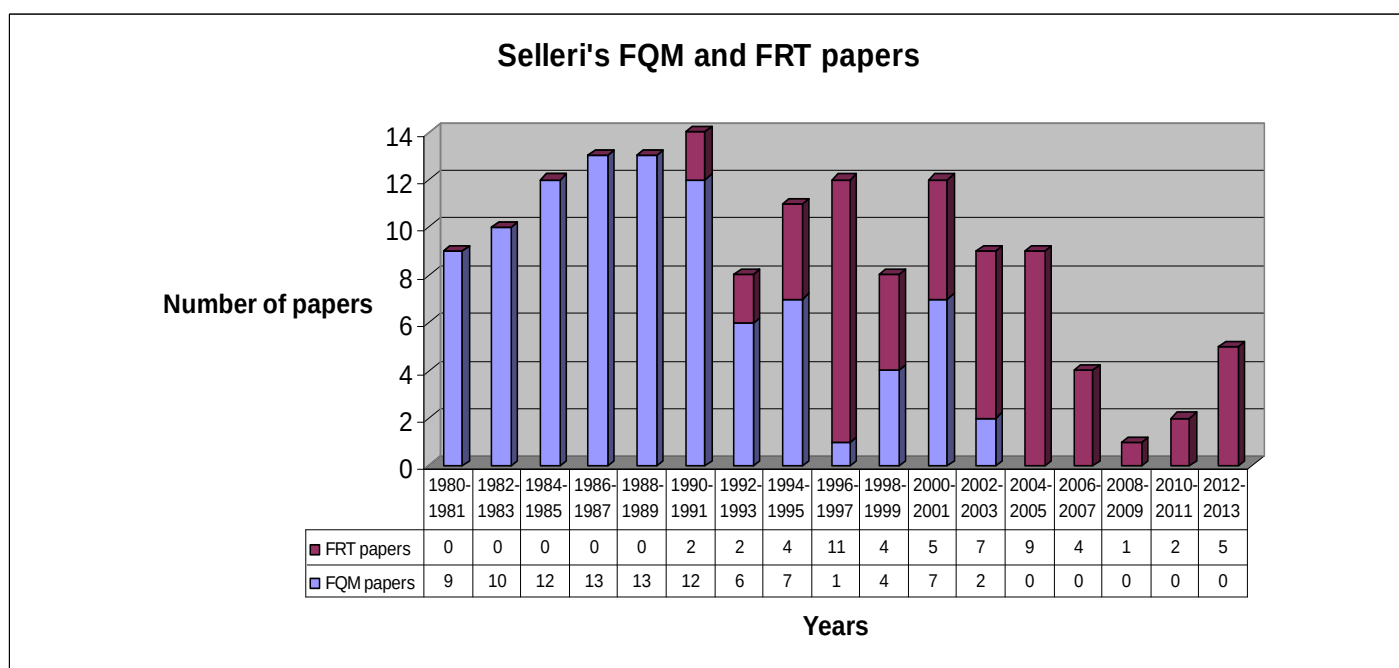
This research suggests a blurred transition area. I have taken into account only the English papers published in the 1980s, 1990s and 2000s.

In the 1980s Selleri published only in FQM, with an average of 5.7 papers per year. In the 1990s the situation changed, though. In FQM this figure drops at least to half: Selleri published an average of 3.0 papers per year, while in FRT he published 2.3 papers per year. In the four years going from 2000 to 2003, he published 2.25 papers per year in FQM, while he published 3 papers per year in FRT.

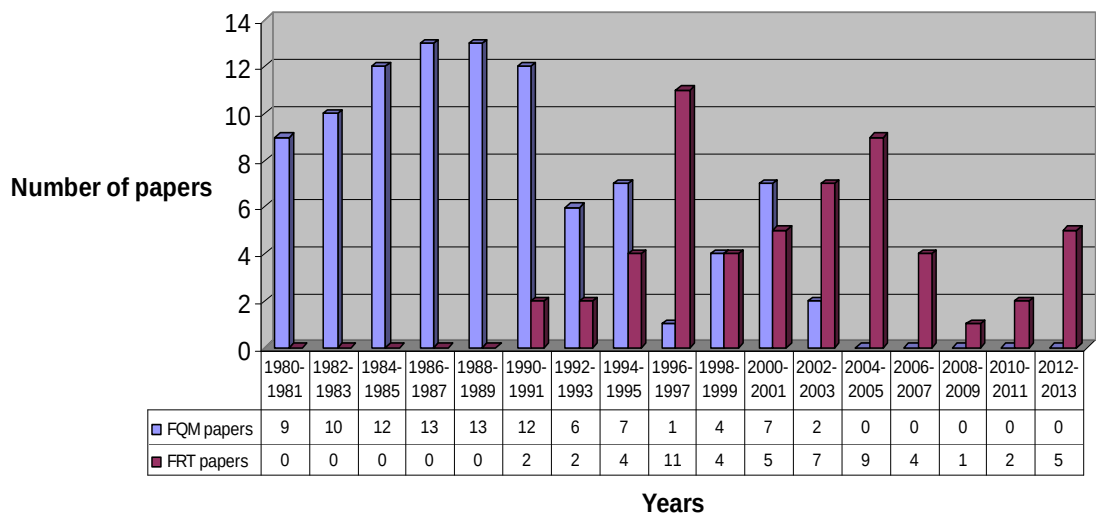
The switch occurred permanently only in 2004. The last published paper in FQM was indeed a 2003 one. In the remaining 9 years of the 2000s, Selleri published 21 papers in FRT, 2.3 papers per year.

Below I submitted two charts both showing (using two different charts for the same data) how the passage between FQM and FRT was long and shaded. For several years Selleri published in both fields.

Sometimes he published nothing; and he resumed his activity in FQM after a period of inactivity, in conjunction with his research on kaons. Starting the 1980 to 2013, Selleri published 96 English papers in FQM and 56 in FRT. I put biennals in the x-axis from 1980 to 2013; and FQM and FRT number of papers in the y-axis:



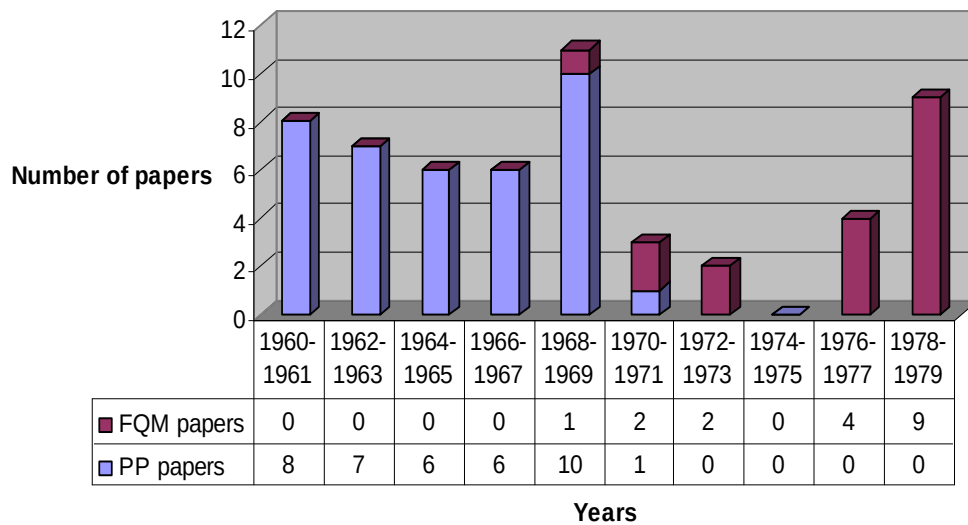
Selleri's FQM and FRT papers

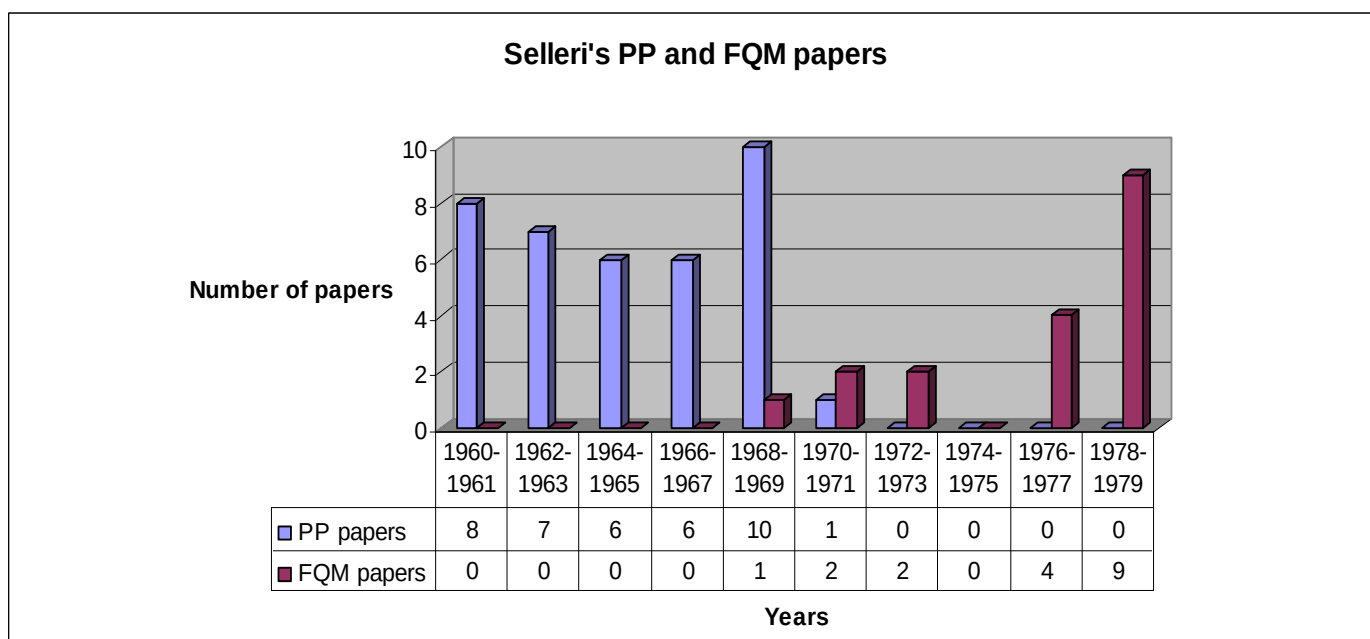


To compare with the above passage, I also added two charts showing his passage from PP to FQM. Here the shift sounds sharper. In only 3-4 years, Selleri started publishing only in FQM.

Starting the 1960 to the 1979, Selleri released 38 English papaper in PP and 18 in FQM. I put biennals in the x-axis from 1960 to 1979; and PP and FQM number of papers in the y-axis:

Selleri's PP and FQM papers





The general decline in the total number of publications during the years 2006-2013 can be explained by the presence of a great number of papers in FRT which were not published. Selleri stated that he found a lot of difficulties in order to release his last book in *Relativity* in 2011, attributing such difficulties to an obstacle to the circulation of his ideas:

[...] My book is published by a publisher in Milan. It should be published in March in Italian, but the English edition is on the high seas.

For the first time in my little story, I have severe difficulties in publishing a book. I attribute it to the concreteness of the proposed alternative: someone must have realized that now it is getting serious! (Selleri to Rocco Macrì (maybe), 2011-FSPA)⁸⁰⁸

Selleri's interest in FRT had a first input in 1983, at the conference in Bari:

[...] by the way, [Popper] was also critical of special relativity [...] and I think that every time he was critical he was right - which is not like saying that his philosophy was all correct, I don't know, I'm not philosopher. Sometimes I find it complicated. Instead as a critic of the foundations of modern physics he is a genius in my opinion.

⁸⁰⁸ “[...] Il mio libro lo pubblica un editore milanese. Dovrebbe uscire a marzo in italiano, ma l’edizione inglese è in alto mare. Per la prima volta nella mia piccola storia incontro severe difficoltà a pubblicare un libro. Lo attribuisco alla concretezza dell’alternativa proposta: qualcuno si deve essere accorto che ora si fa sul serio!” (Selleri to Rocco Macrì (maybe), 2011-FSPA).

He has done something fantastic - because he always could take the point and see exactly what was the difficulty. Eventually he told me, “Franco, what do you think of the theory of special relativity?” and I gave a vague answer because I did not have clear ideas (the episode was before I started doing research on the foundations of relativity) and he was very unhappy with me because of that (AIP 2003. My underline).

However, his first official papers were released only in 1990.

Selleri's statements on his switch from FQM to FRT, as submitted in his books or in Freire's interview are mainly related to the need of finding a logical place for a medium in which the real empty waves could propagate themselves. Namely, the ether. But an ether quite different by the 19th century as well as the ether rejected by Einstein in 1905 with the SRT. So, to protect local realism he will try to search a medium for empty waves, switching him towards the FRT.

In Freire's interview, Selleri argued of real waves:

[...] the idea of real waves is difficult to imagine for many physicists because a wave in general is not a thing in itself, but a property of a medium.

[...] So the idea of real waves really requires a medium. And in recent years I switched to the foundations of relativity to see if a logical space existed for such a medium (AIP 2003. My underline).

Some of Barone's statements confirm the above motivation, adding the successful outcome of Aspect's experiments as a way to change; and still introducing the influence on Selleri research on FRT:

[...] Franco's belief in wave-particle duality pushed him to find a means through which the wave could propagate, so he focused his attention on relativity (Obituary-CERN Courier-March 2014).

When the Aspect experiment came out, Franco told me for the first time that he wanted to move on to relativity, to deepen the aspects of relativity. He was convinced that what the QM called vacuum was not such, but a state of energy [zero point energy?] that boils continuously and from which virtual particles emerge. Then the theory that was going was the QED and the theory of strong interactions had not yet been done. He identified the vacuum with the ether. It was before 1986.

He told me this when he left the Department of physics in Bari and got into his car, a Fiat 500. When he told me I did not understand him.

But he was influenced mainly by an article written by an Australian physicist, a neo-lorentzian, (Prokhovnik). They exchanged faxes, which I have, and which faded. So he identified the vacuum with the ether and started to study in depth the non-Lorentzian theories (Interview with Michele Barone, Bitonto (BA), 1 June 2019).

In a letter to Olivier Costa de Beauregard, Selleri, expressing his appreciation for the new field of FRT, marked the differences between his early research in FQM:

Dear Olivier,

[...] thanks for your interest in my papers on the physics of space and time. As you can imagine this research in the foundations of relativistic physics is a fascinating and exciting activity, more so than in quantum physics where the truth is more hidden and more difficult to discover (Selleri to O. Costa de Beauregard, 17 December 2003-FSPA. My underline).

All in all, the reasons above expressed have definitely influenced, starting the 1990, Selleri's beginning in researching and publishing in the field of FRT, although he continued in researching in both fields for several years. Nevertheless, I will deal with this issue in Chapter 3, fully devoted to FRT.

2.11-ANY LEGACY IN FOUNDATIONS OF QUANTUM MECHANICS, THOUGH?

A QUICK SUMMARIZATION AND BEYOND

Selleri was certainly the one who, by launching the Varenna 1970 proposal (together with Castagnoli), contributed to the growth and development of FQM studies, as recognized by several scholars (see section 2.6.3). And with the organisation of numerous national and international conferences, he has strengthened the synergies between experts, helping to spread this field of research, which has now become a mainstream one, and has also interwoven many relationships, collaborations and meetings.

He has created Group IV in FQM at the University of Bari, which has attracted a number of other colleagues and has made numerous publications and collaborations around the world. In addition, he has always left research freedom to the various members.

As recognized by Freire: “Selleri fed the rush for better experiments and brought the quantum controversy to a wider audience” (Freire 2014, pag. 342),

Selleri has also had a certain influence on several scientists, often directing them towards a choice linked to FQM and has also tried to spread their ideas (see for instance Ghirardi, Kostro, Arp, van Flandern and so on). For instance, Selleri stimulated Ghirardi to start working on FQM as a professional activity; and he was even an indirect influence to achieve the development of the future GRW model.

Josè Ramalho Croca, in the acknowledgements to his 2002 book *Towards a nonlinear quantum physics* gives a heartfelt thanks to Selleri:

I would like to thank Professor Franco Selleri for his friendship and constant support in the vicissitudes of the hard battle to recover causality.

No less significant was his constant assistance in practical academic matters since anyone who seeks for the truth has to face not only the inherent difficulties of scientific research but also the strong prejudices of the established scientific community.

Now as ever, anyone who dares to have opinions in science and seeks truly innovative approaches to understand Nature questioning the sacrosanct established ideas is to be stopped at any cost (*Towards a nonlinear quantum physics*, p. vii).

Selleri was also very willing to reply to many letters and emails from ordinary people, high school teachers, etc., sending them copies of his papers, articles, and books. And he revealed great patience and will to disseminate science. Obviously he was also ready to crush some crazy ideas, but always from a constructive point of view and trying to explain the matter.

Watching the countless letters and e-mails in FSPA I found people (which I also known personally) influenced and even impressed by Franco's popular work, so much so that they devoted themselves to the research with great dedication. And which received a lot of suggestions for reflection that then they submitted in feedback to Selleri.

Who wanted a judgement to be comforted. That were happy if they had an answer. That gave gratitude for what they have learned from reading Selleri's paper. I met them too, here in Bari. There were Italianists to whom Franco's book "changed their lives". And scientists also from abroad who wanted to come and work with.

Selleri also received statements of extreme appraisal from teachers in the high school for very interesting seminars. Some scholars in Universities changed their research program after reading Selleri's papers. He directed several of them in research paths that will then follow for the rest of their lives. Vincenzo Patricchio pointed out that in Bari:

Franco has been a point of singularity in this department. Honestly he has by no means been well appreciated and has not been valued for how he could be valued. On the other hand, this department has not left its mark on the history of scientific research and I do not know if it will make it. Franco directed me to the group of low-energy nuclear physics that still does current, valid research in fundamental research today. Physics has gone all the way to the high energies.

Where, however, few have take stock of the situation. In nuclear physics, on the other hand, at low energy there is a more 360 degree vision, we work, we can still say something not trivial (Interview with Vincenzo Patricchio, Bari, 29 May 2019).

As already told, according to Michele Barone, Selleri, as a director of INFN section in Bari, helped a lot of students receiving leaves during the military service, writing the need to have them for research, seminars, workshops etc. the students also received a great esteem by the military superiors.

And always Barone, who spent his professional career mainly at CERN, claimed that: "Franco told me about these grants at CERN. At the time, nobody wanted to go abroad because they all wanted to find a place at the

Institute of Physics in Bari. Franco told me to participate in the CERN scholarships. And so, by participating, I worked and collaborated there for most of my life” (Interview with Michele Barone, Bitonto (BA), 1 June 2019).

From a strictly physical point of view, Selleri has carried out research on the founding themes of QM, such as the EPR paradox, Bell’s inequalities, and the wave-particle dualism, always proposing experiments that could shed light on the disagreements between the different QM interpretations, even if being a theoretical physicist has always required the need to seek support from experimental groups.

With regard to the effectiveness of his scientific results, there are now several papers discussing the so-called “Pompili-Selleri model”, an alternative vision to QM that requires the presence of hidden variables in an experiment to be done in the ϕ -factory for the production of neutral Kaons and B bosons, in order to discriminate between QM and LR.

And an entire book (Nutricati 1998), two issues of Foundations of Physics in 2004 (34 (11-12), see bibliography), and a conference in Cesena for his 75th birthday in 2011, have been dedicated to Selleri’s activity. He has been a member of prestigious institutions around the world, such as the New York Academy of Sciences and the Fondation Louis de Broglie.

Selleri’s participation in a huge number of conferences on FQM, always as an invited speaker (See Addendum 2) is an evidence of an activity of spreading of the fundamental ideas of his scientific work and of wide interest in them.

Among the various examples, there is a request of the London School of Economics (LSE), to attend their activities for the period September-December 2003, even lecturing several seminars on the Foundations of Modern Physics. Selleri chose kaons and FRT. In the invitation letter of the 8 May 2002, the Centre for Philosophy of the Natural and Social Sciences of LSE wrote: “We are writing to you as an outstanding scholar who might like to spend time at our centre, when next on research leave” (Eleonora

Montuschi, LSE Deputy Director to Selleri, 8 May 2002-AGAB). On 3 March 2003, the director of the Department of Physics of Bari wrote to the Dean of Bari University in order to a Financial contribution to be given to Selleri for his stay at LSE with an unanimous favourable opinion from the Physics Department Council of 28 February 2003 “underlining the importance of the invitation addressed to his colleague by such a prestigious institution”⁸⁰⁹ (Marangelli to Bari University Dean, 3 March 2003-AGAB).

Some Selleri’s books have sold many copies, think of the twenty thousand ones of *Che cos’è l’energia?* in the first six years (then, there are no information). Or the success of *Die Debatte um den Quantentheorie* and its 7 versions, especially the English one, *Quantum Paradoxes and Physical Reality*, which received a lot of remarkable reviews. Selleri has also always made a great communication campaign to spread his works, with letters, e-mails, and books’ copies sent.

His review papers in FQM have always shown a deep research aimed at providing the most complete state-of-the-art, always in his motivation to make science clear and accessible to all. Motivation closely linked to his political beliefs, as shown many times.

And even science, as seen in Selleri’s choice built in a 3D-space, can receive new energy by clarity. In his interview to Freire, Selleri stated:

I always thought [...] that it is our duty to build a science that can be communicated to everybody. And at those times I was thinking in terms of the working class, the working people. That is to say, if the only way to understand what I’m doing is to study differential equations or Hilbert space, there is too high a threshold. If instead I build physics in three-dimensional space and in time according to the rules of causality then I can communicate my results (AIP 2003).

Michele Barone stated that Selleri has been a great popularizer! Even though Selleri’s wife, Angela Pizzi argued that he preferred being lecture more than seminars.

⁸⁰⁹ “sottolineando l’importanza dell’invito rivolto al collega da una istituzione così importante” (B. Marangelli to Bari University Dean, 3 March 2003-AGAB).

Anyway, according to Barone:

Franco knew how to explain in simple, plain words. And for this reason everyone followed him, everyone understood. And he was always very successful. You know, physicists are not good speakers. If they are theoretical, they use a lot of equations, if they are experimental, you cannot understand what they say. But he explained it very well, there was a debate in every place, even the most remote from the subject.

In popularization and universal knowledge I think that Selleri took from his thesis supervisor, Puppi. He was a great public speaker. And I think that Puppi himself gave the imprint to Franco.

Selleri made and organized big conferences in Italy and internationally. With the party, I think when Luigi Berlinguer was director of the university section, they began to bring the debate on the two cultures to the Unity Feasts (Interview with Michele Barone, Bitonto (BA), 2 June 2019).

The value of the history and philosophy of science have certainly been crucial to his activity, always at the centre of his attention and the subject of numerous articles (See Addendum 1). Think, incidentally, of the 1993 article “La scienza senza storia non ha più storia” (Science without history no longer has history)⁸¹⁰.

In a letter to Pasquale Tucci, former President of SISFA, Selleri stated:

it was a pleasure to participate in the work of your 16th Congress. It is true that this is a unique reality and that Italian physics could take pride in it at an international level, as well as impulses for renewal. Physics unaware of its true history is a poor thing, almost a caricature of itself⁸¹¹ (Selleri to P. Tucci, 4 June 1996-FSPA. My underline).

A recognition comes from Lanfranco Belloni in the pages of the Italian newspaper “Corriere della Sera”, in the issue of 12 December 1993, who talk about Selleri as an “internationally acknowledged authority [in the foundations of modern physics].

⁸¹⁰ Selleri F. (1993), “La scienza senza storia non ha più storia”, *Gazzetta del Mezzogiorno*, 4 Dicembre 1993.

⁸¹¹ “è stato un piacere partecipare ai lavori del vostro XVI Congresso. E’ ben vero che si tratta di una realtà unica e che la fisica italiana ne potrebbe trarre vanto a livello internazionale, oltre che impulsi di rinnovamento. La fisica ignara della sua vera storia è una povera cosa, quasi una caricatura di se stessa” (Selleri to P. Tucci, 4 June 1996-FSPA. My underline).

His aim is to stimulate a philosophical debate in an environment (that of physicists) traditionally reluctant to any suggestion from the other side of culture”⁸¹² (FSPA).

The huge correspondence held by Selleri with numerous scholars and Nobel Prize all over the world, is another confirmation of his active role in research, capable of creating various debates, even today.

In the following I will report a series of excerpts related to what I found in my research, related to various contributions from scholars, interviews and current affairs articles that show how research is being carried out on topics proposed or studied also by Selleri in the field of FQM.

The contribution by Gino Tarozzi⁸¹³ has already been reported at pages 441-442 in this dissertation, together with Tarozzi’s recalling of Jammer endorsements on Selleri’s research.

Another contribution, again in (Tassani 2013), comes from Fabio Minazzi⁸¹⁴:

A [...] complex, rich and articulated vision of scientific knowledge constitutes, moreover, a critical-epistemological and scientific awareness that has always accompanied the multiple examinations that Selleri, over the course of several decades, has constantly devoted to the critical investigation of various open problems, directly connected above all with the analysis of the foundations of modern physics.

[...] Selleri denounces the dogmatic one-sidedness of two approaches such as those of scientism and historicism: “It is necessary to escape the false dilemma scientism/historicism and accept the fact that a scientific theory can work very well even if it is heavily conditioned by philosophical choices of the most varied kinds. It is necessary to consider the different theories for what they really are, outside all schematism. In order to do this, an indispensable tool is a fairly deep knowledge of the history of the discipline to be critically studied and its real connections with the more general cultural and social processes”⁸¹⁵.

⁸¹² “esperto riconosciuto a livello internazionale [nei fondamenti della fisica moderna]. Il suo intento è stimolare un dibattito filosofico in un ambiente (quello dei fisici) tradizionalmente refrattario a ogni suggestione proveniente dall’altra sponda della cultura” (FSPA).

⁸¹³ Gino Tarozzi (1954-): full professor of Logic and Philosophy of Science at Urbino University. He collaborated a lot with Selleri, still publishing remarkable papers of FQM. See:

<https://www.uniurb.it/trasparenza/amministrazione-trasparente/organizzazione/gino-tarozzi>,
<https://www.uniurb.it/persona/gino-tarozzi>. Both accessed 24 September 2020.

⁸¹⁴ Fabio Minazzi (1955-): Full professor in Theoretical philosophy at University of Insubria. See: https://www.uninsubria.it/sites/default/files/Curriculum_0.pdf. Accessed 24 September 2020.

⁸¹⁵ Selleri citations quoted in (Minazzi 2013) belong to (Selleri Paradossi 2001),

[...] Selleri also speaks, explicitly and programmatically, of the “ineliminable role of the history of science, no longer only as a cultural curiosity, but also as an instrument of critical understanding for every researcher in his discipline”. But, mind you, for Selleri this openness to the historical dimension of science must always and necessarily also involve philosophical reflection because “by linking science to philosophy we can try to fully recover the historical dimension of scientific thought and to understand the nature of the troubles that have given rise to the various scientific conceptions”⁸¹⁶ (Minazzi 2013⁸¹⁷, pagg. 182-184).

Concerning the role of history and philosophy in Selleri’s thoughts, Pompilio Nutricati, the author of the book dedicated mainly to Selleri’s work, stated that⁸¹⁸:

For most of his life as a scholar, Selleri focused his studies on the clarification of the scientific and philosophical aspects of quantum mechanics (Nutricati 1998, pag 123).

[For Selleri] the scientist [...] must not have the presumption of being detached from the philosophical, social-historical and cultural spheres, at the moment in which he approaches science. The philosophy with which Selleri faces scientific problems plays a double role: on the one hand, it tries to recover that historical perspective of scientific thought which, distancing itself from historicism (understood as the analysis of those cultural contents which, historically conditioning scientific activity, still turn it into “cultural fashion”) allows a greater understanding of objective cognitive conquests and of the different ways of approaching physical reality; on the other hand, the philosophical attitude is recovered in its being a critical-constructive activity of the scientific-rational product of contemporary physics.

In Selleri’s epistemological approach the two aspects intertwine coherently with his adherence to scientific realism, which is inseparably conditioned by the principles of causality and comprehensibility (Nutricati 1998, pag 124).

[...] if the understanding of physical phenomena implies the need for objectivity, it is impossible not to take into account the arbitrary and subjective elements inherent in the creative act. The scientific theory thus turns out to be a very complex construction that synthesizes and organizes empirical evidence and data, together with cultural and philosophical tendencies.

⁸¹⁶ The Italian original text is in Addendum 3, p. 1042.

⁸¹⁷ Minazzi F. (2013): “La questione epistemologica del realismo nel programma di ricerca di Franco Selleri” in Tassani I. (ed), *Oltre la fisica normale-interpretazioni alternative e teorie non standard nella fisica moderna per il 75-esimo compleanno di Franco Selleri*, Isonomia Epistemologica, 3, pp. (181-208).

⁸¹⁸ The Italian original text is in Addendum 3, pp. 1042-1043.

[...] What Selleri believes is needed is an ecology of science, i.e. a theoretical-conceptual ecology that allows to eliminate that dogmatic and authoritarian structure, and at the same time highlights the fact that in science not all ideas are equal but there are positive and negative ideas for progress.

The position he embodies is, then, that of the neorealist who fights against the irrationalistic positions of those who support the completeness of quantum theory.

The scientific and philosophical touchstone in favour of realism is the paradox of EPR which, in Selleri's opinion, in time will become a decisive element for the victory or defeat of the realist conception (Nutricati 1998, pag. 125. My underline).

Selleri fought against the so-called Blind Dogmatism, which:

precludes not only any kind of philosophical debate, which should always be alive in the field of research, but tends to suffocate precisely that research which goes "against the tide" and which, as has very often happened in the history of our civilization, has given strength to the development of knowledge of reality and the relationship between it and man (Nutricati 1998, pag 173).

The quotes from Max Jammer to early Selleri's contribution in FQM have already been reported⁸¹⁹ and also recalled by (Tarozzi 2013) at page 444.

Another endorsement comes from a paper of Olival Freire Jr. (Freire 2009)⁸²⁰, in which he stated that:

Selleri elaborated a unique approach to foundations, combining his mistrust of the quantum formalism with an agenda to appeal for more and more experiments on these issues.

Indeed, not only did he disagree with the complementarity interpretation for philosophical reasons but he also thought that Hilbert space as a mathematical structure for quantum theory would eventually be considered erroneous and be replaced by a description in the normal space-time frame. He was among the first to suggest experiments to test de Broglie's wave plus particle picture, to expose loopholes in most of the first Bell's theorem experiments, and to suggest testing Bell's inequalities in particle physics.

⁸¹⁹ See pages 214; 439-440; 533; and 551 in this dissertation.

⁸²⁰ Freire O. (2009), "Quantum dissidents: Research on the foundations of quantum theory circa 1970", *Studies in History and Philosophy of Modern Physics*, 40, pp. 280–289. (Freire 2009) in the following.

Experimental work to date has frustrated his expectations as it has confirmed quantum mechanics predictions. However, his role as a kind of critical consciousness of experiments on Bell's theorem has probably been responsible for his high regard in this field. Furthermore, Selleri has mixed his defense of a realistic approach to quantum mechanics with what he considers wider social responsibilities (Freire 2009, pag. 286).

[...] Turning his research to foundational issues did not cause any major damage to Selleri's professional career. Although his full professorship was postponed for 10 years, until 1980, explained by him as a result of his switch, he has never encountered major professional obstacles to his field of research: "I have been treated fairly. I have not been discriminated for the activity I did". Leaving a major center to work in a new center, Bari, with younger physicists also eased potential obstacles, as he acknowledged: "In Bologna it would have been more difficult".

In hindsight, Selleri also considers that the Italian environment also contributed to the new stage of his career, "anyway, I have the feeling that Italy is more tolerant than other countries to the foundations of quantum mechanics," a feature he attributes to a factor not yet studied by historians: Enrico Fermi's criticisms of quantum mechanics.

However, in the early 1970s he did not think of Italy in the same way. For him, "for some time Paris was looking as the most interesting place on earth for my type of research, because de Broglie was alive. There was the de Broglie Foundation, there was Vigier, there was d'Espagnat. There were young leftists like Paty, Levy-Leblond interested in fundamental questions, so it seemed like a paradise". Such a conducive climate did not last. "I have seen with time the paradise melting away completely, slowly, because eventually de Broglie died, the de Broglie Foundation after his death followed a path of low profile and it was not anymore a real defense of de Broglie's ideas, d'Espagnat changed completely his philosophy, and Vigier was very difficult to agree with from the beginning, because he considered himself a nonlocal realist, a position whose motivation for me is still very difficult to understand.

And then the young leftists converted to the orthodox line of thought as well, so with time nothing was left, and Paris disappeared from my horizon". The turn of events in Paris was for him indicative of a negative feature of science at the time.

He recalls that among the founding fathers of quantum mechanics there were two conflicting camps of equal sizes and it became "99 to 1" in favor of the complementarity view among those active in research. He suspects that dogmatism was the cause of the change. "How was it possible, through repression and control of positions and publications? And then also a lot of dogma exists. People do not dare to oppose important ideas" (Freire 2009, pag. 287).

I have tried, within the limits linked to the development of the thesis, to search the web for some evidence of the current relevance of his scientific message. The most relevant thing has been to find papers on some issues, more than see if they agree or not with Selleri's view. Obviously, my research has certainly been far from exhaustive.

Concerning the local realism and the struggle between local and non-local realism, I found two papers: The first one is a collaboration paper (with 7 scholars, including Zeilinger and Zukowski), written in 1997 and still published in *Nature*, "An experimental test of non-local realism"⁸²¹, basically affirms that it is not enough to leave the locality, some properties of realism must also be abandoned:

Most working scientists hold fast to the concept of 'realism'—a viewpoint according to which an external reality exists independent of observation. But quantum physics has shattered some of our cornerstone beliefs. According to Bell's theorem, any theory that is based on the joint assumption of realism and locality (meaning that local events cannot be affected by actions in space-like separated regions) is at variance with certain quantum predictions. Experiments with entangled pairs of particles have amply confirmed these quantum predictions, thus rendering local realistic theories untenable. Maintaining realism as a fundamental concept would therefore necessitate the introduction of 'spooky' actions that defy locality.

Here we show by both theory and experiment that a broad and rather reasonable class of such non-local realistic theories is incompatible with experimentally observable quantum correlations. In the experiment, we measure previously untested correlations between two entangled photons, and show that these correlations violate an inequality proposed by Leggett for non-local realistic theories. Our result suggests that giving up the concept of locality is not sufficient to be consistent with quantum experiments, unless certain intuitive features of realism are abandoned (Groeblicher et. al 2007, pag. 871. My underline).

The paper received even a comment by Stephen Parrot⁸²², which intertwined with the authors. Among several objections, he stated that:

⁸²¹ Groeblicher S., Paterek T., Kaltenbaek R., Brukner C., Zukowski M., Aspelmeyer M., Zeilinger A. (2007), "An experimental test of non-local realism", *Nature*, 446, pp. 871-875. (Groeblicher et. al 2007) in the following.

⁸²² Parrot S. (2007), "Comments on "An experimental test of non-local realism" by Groeblicher S., Paterek T., Kaltenbaek R., Brukner C., Zukowski M., Aspelmeyer M., Zeilinger A, *Nature*, 446 (2007), pp. 871-875 "[arXiv:0707.3296v2](https://arxiv.org/abs/0707.3296v2). Accessed 24 September 2020. (Parrot 2007) in the following.

I found this paper very interesting, in fact so interesting that I was motivated to think carefully about its assumptions and to check some tedious mathematics. I noticed what looked like a serious error in the paper's proof of its key inequality (Parrot 2007 pag. 1).

The paper does not completely explain its statistical methods (Parrot 2007, pag. 7).

As a mathematician who is largely self-taught in physics, I am unsure of the correspondence between the physical measurements described in the paper and the mathematics of the Poincaré sphere. Is this well-established physics, or is it a kind of guess, based on mathematical analogies between complex polarization vectors in classical electrodynamics and the two-dimensional complex state space describing quantum-mechanical photons? I am uneasy about the paper's justification for its assumption that the average over a great circle on the Poincaré sphere can be confidently replaced by an evaluation of the single correlation $C(\sim a, \sim b)$ for $\sim a, \sim b$ on the circle. The paper justifies this assumption as follows:

“So far, no experimental evidence against the rotational invariance of the singlet state exists. We therefore replace the rotation averaged correlation functions in inequality with their values measured for one pair of settings (in the given plane).”

It seems dangerous to assume that something is true on the sole grounds that no one has proved it false. That risks overlooking potentially important new physics. (Parrot 2007, pag. 9. My underline)

The second paper was published on Nature by Howard Wiseman⁸²³. He deals with “a fundamental scientific assumption called local realism” and how some experiments have (finally) prevented it. The title, “Death by experiment for local realism” is unambiguous. In the abstract Wiseman states:

A fundamental scientific assumption called local realism conflicts with certain predictions of quantum mechanics. Those predictions have now been verified, with none of the loopholes that have compromised earlier tests.

The world is made up of real stuff, existing in space and changing only through local interactions - this local-realism hypothesis is about the most intuitive scientific postulate imaginable. But quantum mechanics implies that it is false, as has been known for more than 50 years.

⁸²³ Wiseman H. (2015): “Death by experiment for local realism”, *Nature*, 526, pp.649-650. (Wiseman 2015) in the following.

However, brilliantly successful though quantum mechanics has been, it is still only a theory, and no definitive experiment has disproved the local-realism hypothesis - until now. [...] Hensen *et al.* (it is a collaboration with 19 scholars)⁸²⁴ report the first violation of a constraint called a Bell inequality, under conditions that prevent alternative explanations of the experimental data. Their findings therefore rigorously reject local realism, for the first time. (Wiseman 2015, pag. 649).

Hensen and co-workers' experiment was made possible only by combining state-of-the-art quantum technologies - it was performed in the Netherlands, but used diamond substrates prepared in the United Kingdom and fast random-number generators developed in Spain. Maintaining optimal operation of all the devices during the experiments was extremely challenging, and the rate of events [...] was only about one per hour. As a consequence, only 245 such events were recorded, and the statistical uncertainty in the reported Bell-inequality violation is comparatively large. Nevertheless, from a careful analysis of the entire data set [...], Hensen *et al.* reject the local-realism null hypothesis at a confidence level conventionally considered to be statistically significant. It is to be hoped that more data will be generated soon (Wiseman 2015, pag. 650).

Looking at these final statements, with a certain arbitrary rejecting, even with a poor statistics, maybe the death of local realism is not so clear.

The foregoing papers are clearly confirmations of QM, but it is significant how QM and LR are currently compared and that maybe it is even an open matter.

However, it sounds curious how, after 8 years between the two papers, the untenable local realistic theories, definitely defeated, were still the main argument of papers published on Nature, maybe the most relevant journal in the world.

Anyway, the research lines on the interpretational issues Selleri worked on have survived him and are little fields of live research with their value to the future development of physics still subject to controversial assessments.

It remains to be seen what will be the developments related to the basic activity of kaons, and if it will be possible to perform the experiment requested by Selleri.

⁸²⁴ Hensen, B. et al.(2015), "Loophole-free Bell inequality violation using electron spins separated by 1.3 kilometres", *Nature* 526, pp. 682–686.

Indeed, some opportunities may possibly open up at Belle II at KEK in Japan. However, although his theories in the field of FQM have remained linked to a minority of physicists around the world, and have certainly not been successful, they have nevertheless enriched and varied the debate, as should always be the case in science. The critical analysis with which Selleri has contributed to the FQM debate has paradoxically given an impulse to the search for a greater understanding of the QM, not leaving it associated with a unique and monolithic vision.

For all that has been said, his role in FQM has certainly been recognizable and not to be underestimated.

CHAPTER 3

SELLERI AND THE

FOUNDATIONS OF RELATIVITY

THEORY (1990-2013)

“Science commits suicide when it adopts a creed”.

Thomas Henry Huxley (1825-1895)⁸²⁵

3.1-FOREWORD

The Special Relativity Theory and the General Relativity Theory (further refereed to as SRT and GRT, respectively) are two other cornerstones of 20th century physics. Being apparently the outcome of Albert Einstein’s ingenuity, they owe much, as happens with any physics theory, to the work of several other scientists, such as Larmor, Lorentz, Fitzgerald, and Poincaré for SRT; Ricci-Curbastro and Levi-Civita for GRT, to name but a few. The latter developed absolute differential calculus, which made it possible to “translate into precise formulas the most complex geometrical questions concerning varieties re-emerging at any number of dimensions. It will be widely used by Einstein in the development of GRT”⁸²⁶. Compared to QM, the two theories, based on very strong postulates, exhibit some elements of contiguity and separation. The marginalisation of superluminal signals from the SRT has always fed the alternative of local theories to hidden variables in quantum theory, unlike the intrinsic nonlocality of QM. On the other hand, the acceptance of the two theories, especially of the SRT, closely resembles the long-standing QM diatribes. The SRT revealed Einstein’s positivistic component *à la Mach*, with the elimination of absolute space and time as well as of the ether. The GRT, on the other hand, practically represented a correction and an integration of the SRT, with a certain re-introduction of the ether, albeit with a different significance.

Indeed, starting from the 1920’s, the ether returned to Einstein’s ideas and, thanks to the work of Ludwik Kostro (supported and widely disseminated by

⁸²⁵ Speech at the Museum, South Kensington, on unveiling of a statue of Charles Darwin. Quoted in Herbert Spencer, 'The Factors of Organic Evolution', *The Nineteenth Century* (April/May 1886), 19, 770: https://todayinsci.com/H/Huxley_Thomas/HuxleyThomas-Quotations.htm. Accessed 24 September 2020. For Thomas Henry Huxley see also: <https://www.britannica.com/biography/Thomas-Henry-Huxley>. Accessed 24 September 2020.

⁸²⁶ “tradurre in formule precise le più complesse questioni geometriche riguardanti varietà riemanniane a un numero qualunque di dimensioni. Esso verrà largamente utilizzato da Einstein nello sviluppo della TRG”. Geymonat L. (1971), *Storia del pensiero filosofico e scientifico-Volume V-Dall'Ottocento al Novecento*, Milano: Aldo Garzanti Editore, Pag 67.

Selleri), who had the opportunity to analyse the files of the German scholar, it was possible to analyse Einstein's different ideas on the ether itself.

In the previous chapter we have seen the motivations behind Selleri's passage to the FRT⁸²⁷, i.e., the outcome of Bell's experiments of the inequalities; the negative results of the proposed experiments with the kaons in the ϕ -factory, or their lack; the need, in the study of the EW, to search for a means in which they should propagate, thus joining the concept of ether.

Selleri, who always had Einstein among the inspiring models of his research, always for the sake of scientific truth, did not hesitate to criticize him for his positivistic visions and for the enormous problems inherent in the running of the SRT.

Basically, Selleri stated that Einstein, in his attempt to save the principle of relativity and the ether, has not solved the quantum wave problem: a medium for which a state of movement cannot even be conceived can certainly not support the propagation of waves.

Selleri's whole activity concerned the SRT itself, with a minimum attention to the GRT, which, from personal confidences I received from him, was in any case in his sights for a future investigation.

After recalling the concepts of the SRT, I will show how it has always been the subject of much criticism; how Italy, for example, has been a country particularly at odds with the dictates of the SRT and how there has been a continuous movement of strong opposition to it, think of Quirino Majorana, for instance. An opposition, as I shall try to show, sometimes even full of preconceptions.

Moreover, I will introduce Selleri early commitment in FRT, the influence he received and his early papers. He analyzed from the philosophical and sociological point of view, as his use, the problems inherent in the SRT and developed, even passing through even blind paths which did not lead to much, an alternative to the Lorentz transformations (LT), which he christened

⁸²⁷ Even if, as we have seen, Selleri's first articles on FRT date back to 1990, his activity has been spread over FRT and FQM, with EPR studies applied to the case of kaons.

“inertial transformations” (IT). He then developed an alternative theory (unlike the FQM, as it declared to Freire), called “Weak relativity” (further referred to as WR), in which he shown, with various *Gedankenexperimente* and also with concrete cases (think to the Effect Sagnac), the greater simplicity of his theory in the explanation of the phenomena, often treated in artificial way from the RT. In 2011 he managed, with many troubles, to get a book on WR released in Italian with a small publishing house, whereas the English edition was the subject of huge troubles for the publication, being edited posthumously in 2015 for a Greek publishing house that now is in serious economic difficulties. About the above mentioned problems, I show here an email (with unknown date) sent by Selleri to Tobias Schwaibold, at that time the contact person for the Springer publishing house:

[...] unpublished book: WEAK RELATIVITY, which I submitted for publication to Springer something like 3 - 4 years ago. The contact was with a not very kind lady who probably kept it in a drawer for one year before refusing it. Question: is it possible that Springer reconsiders the negative decision, for example by sending the book to a new referee? Probably you can still find the text of WEAK RELATIVITY in the Springer archives. Otherwise, if you want, I will send a new copy. I consider this book to be very important, as it extends my critical description of modern physics from quantum theory to relativity (Selleri to T. Schwaibold, undated, FSPA. My underline).

Moreover, there was also the illegal distribution of the files of his book prior to its publication, made in the USA by people in which Selleri had placed his trust. The chapters and the plot were passed on to someone who then put Weak Relativity’s book online, albeit in a preliminary version.

The death of Selleri in 2013 blocked his research on SRT. Moreover, since his involvement in the FRT was very recent, the historical researchs sound not so exhaustive, though. For this reason, I believe that my analysis can contribute to bring some insights into the debate.

However, it should be pointed out that there has been a debate at the same time during the publications (few, compared to what was produced) of Selleri in FRT, with the participation of several scholars all over the world; as well as with the establishment of several international conferences on FRT, which have seen Selleri always present, very often as organizer of the same.

3.2-ELEMENTS OF RELATIVITY THEORY

The worldwide bibliography on the matter is extremely vast⁸²⁸, which is why I will limit myself to very general references, remarkably incomplete and really far from being exhaustive at all. During Selleri's process for the development of his theory, I will even show some few mathematical details inherent LT and so forth.

The 1905, the admirable year ("Annus mirabilis") of Einstein, saw the publication, among the four pioneering works, of *Zur Elektrodynamik bewegter Körper* (*On the Electrodynamics of moving bodies*)⁸²⁹. Ugo Giacomini, in his essay "Einstein", in *Storia del pensiero filosofico e scientifico*⁸³⁰, introduced the seminal 1905 paper written by Albert Einstein⁸³¹:

[In 1905, within the paper *Zur Elektrodynamik bewegter Körper*] Einstein, starting from electrodynamic considerations, criticizes the concept of absolute rest not only in the field of mechanics, but also in electrodynamics. The new conjecture in which absolute rest is no longer spoken is called by him the "principle of relativity" and it is introduced together with another hypothesis, that light propagates in vacuum with a speed independent of the speed of the emission source. This paper then examines the concept of simultaneity, of which an operationistic definition is proposed, and clarifies the results of the application of the principle of relativity (Giacomini 1971, pag. 852).

⁸²⁸ The present author's master degree, on the Principle of Relativity, presents a 20-page bibliography on the specific subject, which in turn is far from exhaustive: Romano L. (2004): *The principle of relativity*, Master degree, University of Bari.

⁸²⁹ Einstein A. (1905): "Zur Elektrodynamik bewegter Körper", *Annalen der Physik*. 17, pagg. 891-921. He received 5486 citations, according to Google Scholar, accessed 24 September 2020. The English version had 1376 citations. Maybe the number of citations is not the best way to calculate the true appreciation and the pivotal far-reaching value of the paper.

⁸³⁰ Geymonat L. (1971), "Storia del pensiero filosofico e scientifico. Volume V. Dall'Ottocento al Novecento", Milano: Aldo Garzanti Editore, pp. 851-889. The Giacomini essay is represented as (Giacomini 1971) in the following.

⁸³¹ All Giacomini's original texts in Italian are in Addendum 3, pp. 1044-1045.

[...] The memory [of Einstein in 1905] is divided into two sections: kinematics and electrodynamics. The kinematics section in turn consists of two parts which are well divided: in the first one the definition of contemporaneity can be included; in the second one rests the formal development of a theory of the transformation of the coordinates (Giacomini 1971, pag. 862).

[...] There is therefore a relativity in the definition of simultaneity, due to the finite speed with which any signal is transmitted, relativity that is also transmitted in the relativity of lengths.

[...] the second section of the first memory already introduces a concept that Einstein will develop in depth and generalize in general relativity: there are physical laws [...] that are invariant relativistically, i.e. they have the same shape in all systems in uniform relative motion [Inertial reference systems-IRS] (Giacomini 1971, pagg. 863-864).

[...] beyond the experimental value [...] a very large method value can be seen. Einstein, still in the first section of the first memory, showed how, starting from the definitions of space contraction and time dilation, a relativistic kinematics can be constructed ex novo. Here then appear the definitions of relativistic velocity, the explanation of the Doppler effect and, above all, the relativistic definition of the momentum and dynamics of the single particles; that is to say, as we have said, a whole new mechanics is born (Giacomini 1971, pagg. 866-867).

Giacomini analyzed even the fatherhood of the SRT, underlining the relevant contribution by Hendrik Antoon Lorentz⁸³² and Jules Henri Poincaré⁸³³:

According to [...] Edmund Whittaker, historian of modern science, the discovery of special relativity should be attributed more to Lorentz and Poincaré than to Einstein, although he expressed the theory concisely and presented it clearly in his experimental results. Percy W. Bridgman also underlined Lorentz's contribution to the birth of the theory of special relativity, since he had shown that the notion of absolute rest in space, i.e. the existence of a privileged reference system, was without foundation.

Is Whittaker's thesis historically and critically correct? Is it true that Poincaré and Lorentz discovered the key results of the special theory before Einstein? We have to ask ourselves, first of all, if Einstein [was] familiar with their works [...].

⁸³² Hendrik Antoon Lorentz (1853-1928): <https://www.nobelprize.org/prizes/physics/1902/lorentz/biographical/>
<https://www.nobelprize.org/prizes/physics/1902/lorentz/facts/>
<https://www.britannica.com/biography/Hendrik-Antoon-Lorentz>. All accessed 24 September 2020.

⁸³³ Jules Henri Poincaré (1912): <https://plato.stanford.edu/entries/poincare/>
<https://www.britannica.com/biography/Henri-Poincare>. Both accessed 24 September 2020.

Beyond that we must ask ourselves if he really expressed the same points of view as the two scientists who would have preceded him.

It is true that as early as 1902 Poincaré had pointed out the impossibility of making a movement appear relative to the ether.

In [*Science and Hypothesis*] he had assumed the relativity of the motions of two systems that are in uniform relative motion with respect to each other, denying the contemporaneity of two events that take place in different places. However, he always remained in the field of hypotheses, assuming that a new mechanics could be born only after certain experiences. It seems that Einstein actually read *Science and Hypothesis* before publishing his 1905 memoir and that from that book he took the name “relativity” to indicate his theory.

Lorentz had proposed his equations to explain the outcome of Michelson-Morley’s experiment, and had managed to predict from them that the increase in the speed of an electron causes an increase in its mass.

Einstein knew Lorentz’s memory and extended the result obtained by him to all bodies, removing from the equations the preeminent character of hypothesis of circumstance, in order to give them an operative meaning.

While Lorentz recognized that the physical meaning of his formulas had been clarified by Einstein and Minkowsky, Whittaker wanted to attribute the discovery of relativity not to Einstein but to Lorentz.

On the other hand, although Einstein claimed not to have studied Poincaré’s work in depth and to have known only Lorentz’s work on electromagnetic theory, it must be kept in mind that already in 1904 Lorentz had dealt with the principle of invariance, drawing it from Poincaré.

All these facts make it legitimate to sustain that Einstein was in a certain sense the author of themes and suggestions already existing in the physics of his time, as long as we do not forget that he is still the creator of a new mechanics. In a few moments in the history of physics it is more evident that great ideas are the result of a certain scientific atmosphere rather than of brilliant and isolated intuitions. We can conclude that without Poincaré’s anticipations the theory of relativity would have found more difficult to impose itself on the philosophical level, without Lorentz’s work it would perhaps have been impossible to give mathematical form to operational facts. However, nothing is taken away from Einstein’s ingenuity by the observation that his work has crowned a set of criticisms of classical mechanics, rather than opening them up (Giacomini 1971, pagg. 874-876).

In this regard, in my master degree⁸³⁴ (see footnote 831 for the reference) I analysed the relationship between Lorentz, Poincaré and Einstein, also on the matter of their different contributions to the theory of special relativity, in order to show how science outcomes are the result of several contributions, even passing through failures or shortcomings.

Firstly, the Lorentz Transformations were the product of the work of Voigt and Larmor. Abraham Pais reveals that the Irish physicist Joseph Larmor, in 1898, found the correct form of the LT and demonstrated that the contraction of Lorentz and Fitzgerald can be deduced from them. Lorentz introduced the local time, which changes for different reference systems that are in relative motion. It was the first case of different times in each reference system, although with a diverse underlying idea from that of Einstein. In Lorentz's mind there was no will to question an absolute, independent and always valid time; local time was certainly not a real time but a heuristic working hypothesis, a useful auxiliary variable that allowed him to write in the same form the electromagnetic field equations for two systems in relative motion.

Some years later, Lorentz will give priority to Einstein's work, saying that RT was obtained only thanks to the German scholar, independently from previous theories, and that Einstein would have conceived it even without the work of his predecessors. Instead, as Einstein said several times, if it had not been for Lorentz he would never have been able to discover the SRT.

Lorentz's relativistic methodology resides precisely in his study of the electrodynamics of bodies in motion, a subject that was central to him and from which Einstein also began. With the law he found for electrons, the increase in mass as speed increases; and his will to generalize it, Lorentz reached the most important result on which the whole relativistic revolution rests. The law of variation of mass lead, in fact, to the decisive role of relativistic dynamics which is resolved in the kinematic relations of metrics. But Lorentz did not grasp the space-time aspect, not even in principle.

⁸³⁴ Here I did not submit anY excerpt, just resume my outcomes.

He no longer followed Newton's approach and did not accept the idea of an absolute space. However he preserved an absolute time, true, distinct from local time. Later, he wrote to Einstein that this was his mistake, actually.

It is worthy to underline how Lorentz realized that the GRT allowed the hypothesis of a stationary ether. Einstein, initially against this idea, as this would violate the principle of relativity, later came to the conclusion that the GRT space-time had real physical properties and therefore a new ether could be introduced. The power of Lorentz's theory, although completed by Poincaré, is also visible in the innumerable amount of current work that tries to combine it with the GRT, to adapt it to modern results, but also to explain its better role with respect to the SRT.

Perhaps even more than Lorentz, Jules Henri Poincaré embodied the spirit of SRT. If the former manifested a knowledge of theory, presented one that will prove to be an alternative that still holds comparison, and stimulated Einstein's work, Poincaré was the first to introduce the principle of relativity in 1904, with the precise definition of the impossibility of observing absolute motion in optical phenomena, extending it to electromagnetism, and applying it to gravity.

Poincaré partially criticized the ether and strongly criticized both absolute space and time. He continued the Lorentzian opening to the invariance in form, or covariance, of the physical laws, which occurs in the non-homogeneous Lorentz group, today called Poincaré's group, introduced for LT. This leads to a relativistic theory that is not attributed to Lorentz precisely in the name of the lack of a group concept for LT. In fact, even Lorentz's corrections to LT, in 1904, were not the correct ones for the lack of the factor $\gamma = 1/\sqrt{1-(v/c)^2}$ to numerator in the expression of time. Poincaré corrected them the following year, thus allowing the invariance of Maxwell's equations, which is not present in Lorentz's expression, and creating a group structure for LT.

Lorentz introduced the transformations when dealing with two reference systems $S (x,y,z)$ and $S' (x',y',z')$ which move one relative uniformly with respect to the other by sliding their axes in parallel:

$$\begin{cases} x' = x - vt \\ y' = y \\ z' = z \\ t' = t - vx / c^2 \end{cases}$$

Poincaré, however, made the above correction $\gamma = 1/ \sqrt{1-v^2/c^2}$ and wrote what are now recognized as LT:

$$\begin{cases} x' = \gamma (x - vt) \\ y' = y \\ z' = z \\ t' = \gamma (t - vx / c^2) \end{cases}$$

Moreover, in a letter to Lorentz between 1904 and 1905, Poincaré also wrote the formula of relativistic addition of speeds.

Poincaré introduced the imaginary time coordinate, anticipating Minkowski's work, and saw the local time with a physical meaning, and no longer as Lorentz's simple mathematical aid. He also expressed the doubt about the objective meaning of simultaneity, opening in practice the foundations of the whole SRT, in its acceptance of the two postulates and in the elimination of the concepts of absolute space and time and the ether. The electromagnetic energy was seen by Poincaré in 1900 as a fictitious fluid having inertial mass $m = E / c^2$, a possible advance of perhaps Einstein's most famous equation.

Whether the ether exists or not, is stripped of its role of absolute Reference System (RS): a RS at rest in a possible ether is only an Inertial Reference System (IRS) like the others. The ether RS becomes one among infinite IRS, and among them no experiment can select it. Everyone can believe that his own IRS is at rest in the ether, and each IRS can be the ether RS.

Poincaré acknowledged Lorentz's pioneering role in all his work on the principle of relativity. He often mentioned Lorentz's "brilliant invention" of the local time. Among the various theories on the ether, he said that Lorentz's was "the most satisfying among those available".

Einstein never mentioned Poincaré, who perhaps made a contribution greater than Lorentz. It is practically certain that he was aware of Poincaré's speech in 1900 and that he had read his 1898 observation on the non-intuitive nature of the equality of two time intervals.

At Bern, in the *Akademie Olympia (Olympia Academy)*⁸³⁵, a circle which Maurice Solovine suggested to Einstein and Conrad Habicht in order to read the works of great authors and debate them, they regularly spent their evenings reading and discussing philosophical subjects, especially Hume, together with Poincaré and Mach. Einstein recognized how these three authors had a certain influence on his education.

But there is more. The 1900 Poincaré book, *Science and Hypothesis*⁸³⁶, is the one which Einstein and his friends read in Bern. In a detailed list left by Solovine on the books read by the Academy, only Poincaré's book is mentioned in detail, together with the comment that the book made a deep impression on them and kept them breathless for weeks on end!

Einstein introduced two postulates in order to generalize the classical mechanics, based on the Galilean principle of relativity and Newton's three principles, to the new evidences of the electromagnetic theory that conflicted with the notions known until that moment.

The first postulate, the Principle of Relativity (further referred to as PR) states that the laws of mechanics, electromagnetism and optics are the same in all IRS. In Einstein's own words⁸³⁷:

⁸³⁵Akademie Olympia:

http://www.einstein-website.com/z_biography/olympia-e.html; <https://einsteinpapers.press.princeton.edu/vol5-doc/57>. Both accessed 24 September 2020.

⁸³⁶Poincaré H. (1902), *La Science et l'Hypothese*, Paris: Flammarion.

⁸³⁷Einstein A. (1916): "Die Grundlagen der allgemeine Relativitätstheorie (The Foundation of the Generalised Theory of Relativity)", *Annalen der Physik*, 49, pp. 769-822. (Einstein 1916) in the following.

The special relativity theory rests on the following postulate which also holds valid for the Galileo-Newtonian mechanics.

If a co-ordinate system K be so chosen that when referred to it the physical laws hold in their simplest forms, *these* laws would be also valid when referred to another system of co-ordinates K' which is subjected to an uniform translational motion relative to K . We call this postulate “The Special Relativity Principle”. By the word special, it is signified that the principle is limited to the case, when K' has *uniform translatory motion* with reference to K , but the equivalence of K and K' does not extend to the case of *non-uniform* motion of K' relative to K . (Einstein 1916, pag. 770. *Italics* in the original text)

The second postulate, the principle of constancy of the speed of light (which can also be derived from the PR) states that light propagates in vacuum at constant speed c , regardless of the state of motion of the source or the observer.

The initial departure from the Galilean definition, made by Poincaré, acquires a generality with Einstein. But the latter introduced the principle of relativity also from the evaluation of the fact that in nature there is the non-existence of instantaneous interactions. From this, follows an interval of time for the interactions, and therefore a speed of propagation, which can have a maximum value. Compared to Galileo’s principle of relativity, the propagation speed of interactions is no longer considered infinite.

Then Einstein devoted himself in the development of the theory, a new kinematics and dynamics applied to moving bodies with speeds close to the light velocity.

Summarizing the basic role of Lorentz and Poincaré in the birth of RT, from their work emerges the introduction of LT, the definition of the principle of relativity, of relative simultaneity, of the fourth imaginary coordinate of time, of the electron and of the theory based on ether and contraction. Even today, the cornerstones of the so-called Lorentzian theory of relativity, which is revealing itself as an alternative to SRT, deserves a mention. As often recalled by the same Selleri, there are at least eleven independent experiments, and none of them allows to distinguish between Einstein and Lorentz, nor certainly in favour of the former.

They give results that can be explained by both theories. But three of them, namely those of Sagnac, Michelson-Gale and Ives-Stilwell show results in contradiction with Einstein because they imply a privileged RS and contain aspects that make interpretation in this perspective much easier. These are the fields in which Selleri will work, still favouring Lorentz's theory with his acceptance of ether, the remarkable concept which Selleri chose as the medium of propagation of the real waves of QM.

An ether certainly different from the 19th century and 1905 conceptions, though. Really close to the one rediscovered by Einstein as physical space-time whose local state is described by the $g_{\mu\nu}$ components of the metric tensor g of the GRT.

3.3-A LONG-STANDING OPPOSITION

Einstein was awarded the Nobel Prize in 1921 for his contribution to quantum theory (although much hindered, at least in the early years, by the scientific community), neither for SRT nor GRT. Very few people understood RT. The two theories always suffered a strong opposition until today, widespread over the world and sometimes really violent. In the 1930s, the RT was strongly challenged in Germany by the Nazi Party and by several intellectuals as "Hebrew science", to be boycotted and eliminated only for this reason. Over time, RT has become a consolidated theory and the dynamics present in Thomas S. Kuhn's key book, *The Structure of Scientific Revolutions*⁸³⁸, with textbooks endorsing the dominant view of theory, in this case RT, has even revived themselves.

I found several papers published until these years, which testify the bugs presented by RT as well as the reasons for which there is a wide opposition against the theory. As I did for the whole thesis, here too the choice is only personal and by no means exhaustive. A lot of diverse approaches are indeed introduced, and sometimes the boundary between scientists and crackpots sounds very blurry. In MBPA I found a controversial paper, "95 years of

⁸³⁸ Kuhn T.S. (1962), *The Structure of Scientific Revolutions*, Chicago: University of Chicago Press.

criticism of the Special Theory of Relativity (1908-2003). The G.O. Mueller research project”, edited by G.O. Mueller and Karl Kneckebrödt, impossible to find online. They stated how there are 3789 publications criticizing the theory and they decided distributing this documentation to libraries, to the printed media and to eminent representatives of public opinion, addressing open letters to the members of the German federal parliament (Bundestag) and to journalists of several German newspapers.

[...] About the year 1914, SRT had already been refuted directly by several experiments and indirectly by the absence of experimental confirmation. The Michelson-Morley experiment and its repetitions have had positive results, in complete contrast to the relativist's propaganda until today of an alleged null-result: these experiments have found velocities of the earth of about 6 km/s (1887), 10 km/s (1902), 7,5 km/s (1904), 8,7 km/s (1905). In 1913 Sagnac with his rotating interferometer also found moving fringes, the rate of motion of the fringes depending on the rate of rotation of his instrument. On the other hand there were no experimental confirmations for the pretended length contraction and time dilatation. This desperate experimental state of affairs before WWI has never been recognized by the relativist textbooks. As a systematic overview we have collected 130 errors arranged into 21 thematic groups.

Another chapter gave for the first time a rough chronology of the development of the criticism. Finally the authors submitted several social pressure and enforcement which, in their belief, have been practised by the followers of RT:

- the relativists try to suppress any critical books and papers by preventing their publication
- if critical papers or books have already been published, their authors are calumniated by the relativists
- the relativists prevent a reception of published critical papers or books by banning them from their bibliographic references and not answering them publicly but covering them with silence
- the relativists persecute all persons critical of SR and exclude them from all jobs in academic physics and in school teaching
- the relativists calumniate all critics of SR as antisemites, nazi, Stalinist and the like
- with the suppression of any criticism since 1922 in Germany the general public is deceived about the real state of the theory

- the relativists propagate the theory of SR in all circles of society far from natural sciences without informing them about the existing criticism
- the relativists misuse their influence and the tricks of pedagogy in teaching for the indoctrination of the young public about the great achievements of relativity
- the relativists misuse the suggestive power of the audiovisual media presenting their pretended effects as real and without informing about criticism.

Another paper which introduced several experiments to disprove the two postulates of SRT came by Roberto A. Monti⁸³⁹ and his “Theory of relativity. A critical analysis”⁸⁴⁰. According to him, “Einstein’s theory of relativity is shown to be a physical theory of limited experimental validity. 12 different experiments seem to disprove its two postulates” (Monti 1996, pag. 1). Monti analyzed the two postulates of SRT and the group of experiments related to them.

In the conclusions related to the first and the second postulate of SRT, he summarizes that, concerning the first postulate:

[...] Three experimental tests seem to be in contradiction to the first postulate of Einsteinian relativity:

- (1) The experiment made by Trowbridge, which proves that the dielectric rigidity of the ether is not infinite (vacuum decay effect).
- (2) The reality of Olbers effect, which is a paradox no more, and the existence of the background radiation corresponding to the electric conductivity of the ether.
- (3) Comparison between relativistic linear law and logarithmic law (Monti 1996, pag. 6).

Among the several experiments, Monti introduces even the Morley-Miller ones held in 1902-1905 and the Miller experiment performed in 1921-1925:

Morley and Miller pointed out that the result of the Michelson-Morley experiment did not have the anticipated magnitude, but the indicated effect was not zero.

⁸³⁹ Roberto Monti (1945-2014). See: [https://www.macrolibrarsi.it/autori/_roberto-a-monti.php#:~:text=Roberto%20Monti%20\(1945%20%E2%80%93202014\),Chimica%20dell'Universit%C3%A0%20di%20Bologna](https://www.macrolibrarsi.it/autori/_roberto-a-monti.php#:~:text=Roberto%20Monti%20(1945%20%E2%80%93202014),Chimica%20dell'Universit%C3%A0%20di%20Bologna). Accessed 24 September 2020.

⁸⁴⁰ Monti R. (1996), “Theory of relativity. A critical analysis”, *Physics Essays*, 9 (2), pagg. 238-260. (Monti 1996) in the following. The only document found reports a numbering which goes from page 1 to page 39. I will refer to this latter in my citations.

Moreover, Michelson and Morley made only one series of observations, in July 1887, and never repeated the ether drift experiment at any other time, notwithstanding many printed statements to the contrary.

[...] The observations showed a *very definite positive effect* slightly larger than that previously obtained, but still too small to be reconciled with the expectation. The velocity of relative motion of the Earth and ether obtained from the observations made in 1905 is 8.7 ± 0.6 km/s [...].

Since the Theory of Relativity postulates an exact null effect from the ether drift experiment which had never been obtained in fact, the writer [Miller] felt impelled to repeat the experiment in order to secure a definitive result (Miller D.C., *Rev. Mod. Phys.*, 5, 203, 1933, cit in Monti 1996, pag. 21. *Italics* in the original text).

From 1921 to 1925 Miller had the opportunity to repeat the Michelson-Morley experiment at Mount Wilson. The result was the following:

These observations all show a positive periodic displacement of the interference fringes, as of an ether drift, of the same magnitude, about 10 ± 0.33 km/s, as had been obtained in previous trials [...]. Under the conditions of actual observations, the periodic displacements could not possibly be produced by temperature effects [...]. These experiments had given conclusive evidence of a real effect which was systematic but which was small in magnitude and was unexplainable as to his azimuth [...]. The average of the curve on sidereal times, showed conclusively that the observed effect is a cosmical phenomenon (Miller D.C., *Rev. Mod. Phys.*, 5, 203, 1933, cit in Monti 1996, pagg. 25-26).

Concerning the second postulates, Monti evidences how:

Einstein's second relativity postulate seems to be disproved by the following experimental tests:

- (1) Measurability of kinematic and electromagnetic velocities of light. Romer and Bradley could measure the one-way speed of light (c_0) just because $c_0 + v \neq c_0 - v \neq c_0$. New electromagnetic measurements of the speed of light can underline the groundlessness of the second postulate.
- (2) "Equal arms" interferometric experiments, which prove that the kinematic speed of light is not a constant (Michelson-Morley, Morley-Miller, Michelson-Pease-Pearson).
- (3) Michelson-Sagnac effect (the optical gyroscope).
- (4) The Kennedy-Thorndike experiment (the interferometer "with unequal arms").

- (5) The Ives-Stilwell experiment (existence and measurement of longitudinal and “transverse” Doppler effects).
- (6) Marinov’s “coupled shutters” experiment.
- (7) Silvertooth experiment.
- (8) Radar observations of Venus.
- (9) Detection by electromagnetic means of the Earth’s motion through the background radiation (“Nevertheless it moves”) (Monti 1996, pagg. 35-36).

In the general conclusions, Monti stated how SRT exceeds the mere empirical sphere, being an academic subject, too significant for a lot of interests:

Einsteinian relativity seems to be a physical theory of limited experimental validity on the basis of at least 12 different experimental tests, which seem to disprove its two postulates.

Further tests that may disprove the theory of relativity are conceivable (new electromagnetic measurements of the speed of light, a modified Kennedy-Thorndike experiment).

The difficulty in dealing with the scientific matter of the “coming of relativity” is not due to a lack of scientific argumentation or experiment. These, in my opinion, already indicate that the theory is in trouble. The real difficulty seems to be that relativity is not a scientific question, but an academic subject. Many scientists work in research programs concerned with relativistic astronomy and astrophysics, relativistic cosmology, relativistic gravitational antennas, relativistic scientific and popular literature. And many scientists work in elementary particle physics to study the 10^{-37} s after an event (the big bang) that might never have occurred. Against this background the most sound scientific argumentations do not have much of an impact. But notwithstanding the present difficulties the scientific should prevail (Monti 1996, pag. 36. My underline).

Selleri had a correspondence with Monti, and this paper was a subsequent stimulus for Selleri’s work. I shall introduce the first and main influences which Selleri received in FRT in the next section.

The Italian contribution to RT is well reported and introduced in a remarkable paper, titled⁸⁴¹ “I fisici italiani e la Relatività: le ragioni di una lunga opposizione (1906-1927)”⁸⁴², in which the authors deeply explain the framework of the opposition to SRT and GRT in Italy, where experimental physicists and astronomers were the strong opponents. The poorness of mathematical skills of the experimental physicists and the astronomers at that time, with their need of sort of a tutorship by mathematicians; the lackness of theoretical physicists and the country’s scientific and cultural backwardness in those years contributed to making Italy the country that perhaps opposed SRT and GRT for the longest time, despite several confirmations to the theory, not least the famous total sun eclipse in 1919.

The authors’ approach investigates the dynamics and the reasons behind the strong Italian opposition to RT:

The task of this article is to reconstruct in detail the specific dynamics in which this hostility is articulated and to highlight the reasons for this long opposition by analysing scientific and epistemological preconceptions, took the form of a tenacious support of the electromagnetic reductionism with a consequent defence of the ether and of the privileged references, or of the Newtonian theory of gravitation and of a real allergy to the highly mathematical physical theories that was rooted in a widespread ignorance of formalisms that needed to be controlled in order to penetrate its physical meaning (De Maria, Maltese 1997, pag. 2).

At the beginning, RT had some troubles in being accepted, even if some mathematicians showed a better attitude in favour of SRT, compared to physicists and astronomers:

The SRT had a slow, tortuous, difficult acceptance in Europe and the United States, with the only exception of Germany where, already in 1907-9 a small but prestigious group of theoretical physicists (Planck and others) began to understand its novelty and scope and openly sided in its favour.

⁸⁴¹ All the original Italian text contained in these excerpts are in Addendum 3, pp. 1045-1048.

⁸⁴² De Maria M., Maltese G. (1997), “I fisici italiani e la Relatività: le ragioni di una lunga opposizione (1906-1927)”, *Quaderni di Storia della Fisica*, 1, pp. 245-273. (De Maria, Maltese 1997) in the following. Even in this case I did not find online the paper. I used the copy received by Michele Barone, which has a diverse numerical index, going from page 1 to page 39. I referred to this latter index.

Elsewhere (France, England, United States), the resistance is much stronger and is led by experimental physicists. In this context, Italy assumes the role of “lower extreme” on the scale of acceptance of SRT first and then of GRT. In our country, in fact, resistance to SRT is initially strong both among mathematicians and physicists and astronomers; in the latter two categories, the opposition to relativity is particularly tenacious and persistent. It is also true that some important mathematicians such as Guido Castelnuovo and Tullio Levi-Civita began to show an attitude of willingness in 1911 and that Levi-Civita [...] following the correspondence with Einstein in 1915 developed great interest in GRT. From 1916 he began to produce his famous contributions in the field and from 1919, with a small group of colleagues and collaborators [...] he started a propaganda campaign in favour of relativity to break the encirclement, overcome distrust, and open cracks in the wall of hostility both among his mathematical colleagues and, above all, among physicists. The fact is, however, that Levi-Civita and his collaborators were perfectly aware, even in 1919 (the year of the eclipse from which the confirmation of the GRT and the “canonization” of Einstein took their cue), that they were moving in an intellectually hostile environment and that they constituted a minority in that environment⁸⁴³.

Physicists (starting with the dean of the community, Augusto Righi) and observational astronomers remained stubbornly opposed to relativity and, from 1912 until the mid-1920s, the former transformed their initial passive distrust into an active descent into the field to attack some pillars of the SRT on a theoretical level and to propose experiments to confirm the validity of their attacks. The first actors of this offensive were Michele La Rosa, Quirino Majorana, Augusto Righi (De Maria, Maltese 1997, pagg. 1-2. My underline).

Two reasons for the physicists’ opposition rest in:

Loyalty to the ether and opposition to the introduction of “metaphysical” hypotheses in physics [which] characterize the attitude of distrust, if not hostility, shown by experimental physicists in those years (De Maria, Maltese 1997, pag. 5).

⁸⁴³ From the original footnote n. 2, pagg. 1-2:

[...] Tullio Levi-Civita, specifically, represented for Italian experimental physicists, starting with their Dean Augusto Righi, a point of reference for the understanding of theoretical physics [...]. Levi-Civita’s lecture in Rome on 8 March 1919 (“How could a conservative reach the threshold of the new mechanics”) was the culmination of the propaganda activity carried out for relativity, it was translated into several languages and had great diffusion. In Italy, however, it did NOT succeed in increasing the number of supporters of relativity among physicists, even if it contributed to focus more attention on the subject and to provoke an intense debate. (“Tullio Levi-Civita, in particolare, rappresentò per i fisici sperimentali italiani, a cominciare dal loro decano Augusto Righi, un punto di riferimento per la comprensione della fisica teorica... la conferenza di Roma dell’8 marzo 1919 di Levi-Civita (*Come potrebbe un conservatore giungere alla soglia della nuova meccanica*) fu il culmine dell’attività propagandistica svolta per la relatività, fu tradotta in più lingue ed ebbe grandissima diffusione. In Italia essa NON riuscì tuttavia ad aumentare, tra i fisici, il numero dei sostenitori della relatività, anche se contribuì a focalizzare una maggior attenzione sull’argomento e a suscitare un intenso dibattito”).

De Maria and Maltese also analyzed the contribution of the quarrel between Einstein and Abraham for the matter, being the latter the intermediary between Levi-Civita and Einstein:

[...] The controversy unleashed by Abraham [against Einstein following his scalar theory on gravitation, the first step towards GRT] was attended with great interest by Italian physicists, who saw him as a sort of paladin of the ether, as well as a defender of classical mechanics and electromagnetism. [...] The great credit that Abraham's assertions had in Italy is based on the fact that he was, by Einstein's own admission, "the one who showed the best understanding (of relativity)". Abraham was also the intermediary through which Levi-Civita entered into correspondence with Einstein.

The correspondence between the two scholars developed, intense, in the spring of 1915, and Levi-Civita contributed to convince Einstein to abandon his previous theory of limited covariance and to arrive in November 1915 at the correct formulation of the equations of the gravitational field, generally covariant.

Following [correspondence] with Einstein, Levi-Civita, having stopped the caution that had marked his first interventions, became an enthusiastic supporter of Einstein's theories and engaged in intense and creative work on GRT, publishing fifteen works between 1917 and 1920. From the war years onwards Levi-Civita represented the point of reference for those in Italy who wanted to be interested in relativity (De Maria, Maltese 1997, pagg. 6-7).

Among the Italian physicists who challenged SRT were Michele La Rosa and Quirino Majorana, who especially challenged the second postulate, that of the constancy of the speed of light which "cannot claim [...] any logical evidence nor any experimental evidence". Majorana over the years acquired an increasingly marked anti-relativistic attitude, but paradoxically his experiments were cited as a confirmation of the second postulate of SRT. Between 1918 and 1920 even the most important experimental physicist, Augusto Righi attacked RT:

[He showed] mistrust of this "metaphysical" and mathematically too complex theory. Righi believed that relativity was at most a useful mathematical tool of investigation, but he hoped that soon the state of progress of knowledge would become such that it could do without its "metaphysical" implications (De Maria, Maltese 1997, pag. 19).

Astronomers turned out to be violently anti-relativist. According to the authors: “the lack of knowledge of physics among many of the Italian astronomers of that time had greatly amplified the enthusiasm to see a physicist take the field alongside them against relativity” (De Maria, Maltese 1997, pg. 19).

If we consider the fact that the physicists themselves had limited preparation, the picture is far from promising for a thoughtful and profound criticism of RT. The authors underlined also the astronomers’ approach and motivation on their rejection of RT, still revealing how far behind they were compared to the international scene:

The attitude of Italian astronomers on relativity deserves a brief reflection. Their anti-relativism is vehement and finds its raison d’être in a decisive defense of the Newtonian paradigm (Euclidean space and the law of universal gravitation).

They prefer to justify the “anomalies” of this paradigm with modifications to Newton’s law or assuming the existence of ad hoc masses rather than admitting the GRT. In the 10’s such hypotheses had been put forward in the international community of astronomers but had soon been marginalized in the face of the success of GRT. In Italy they were still in vogue in the 1920s, thus demonstrating the character of the rearguard battle fought by their supporters.

The Fifth International Philosophy Congress, held in Naples in 1924, hosted an entire section on relativity, presided over by Marcolongo who delivered an opening speech with the significant title: “between relativists and anti-relativists”, from which it is easy to deduce that, 5 years after the eclipse from which the first great experimental confirmation of the GRT had sprung and 19 years after the [SRT], the clash between enemies and supporters of relativity in Italy was far from over (De Maria, Maltese 1997, pagg. 31-32. My underline).

In conclusion, they stated the three scientific reason behind the hostility:

[...] hostility towards Einstein’s theories took root [in Italy] and grew on three cornerstones. The first is the defence of the ether as a privileged reference and a vehicle for the transmission of optical and electromagnetic phenomena; the second, closely connected to the first, was the reluctance to abandon Euclidean geometry as a grid for absolute space and the visualization of phenomena; [the third] was the fidelity towards Newtonian universal gravitation (De Maria, Maltese 1997, pag. 33).

[...] As we said at the beginning, Italy was not the only nation to show this opposition, whose vehemence and duration, however, place our country at the bottom of a hypothetical scale of acceptance of relativity (De Maria, Maltese 1997, pag. 34).

Another paper which deeply investigates the Italian situation, still describing the contribution of Righi, La Rosa, Majorana is Silvio Bergia's, "Il contributo italiano alla relatività"⁸⁴⁴.

Bergia emphasizes how Righi and Majorana, despite their philosophical and psychological reasons, contributed to the debate between the ballistic theory by the Swiss physicist W. Ritz and the SRT. He also showed why Italian physicists opposed to SRT⁸⁴⁵:

A methodological breakthrough, the replacement of an inductive methodology with a hypothetical-deductive one, would have proved fruitful. But it was totally alien to the mentality, to the epistemological creed of most European physicists and to the almost totality of Italians. It is necessary, in this regard, a clarification: we are not here including in the category physicists-mathematicians. And this on a sociological basis: mathematical physics was dealt with essentially, if not exclusively, by mathematicians. [...] A large part of the community of Italian physicists was therefore prejudicially hostile to Einstein's theory. But while most of them limited themselves to an *a priori* negative judgement, there were those who, precisely because of their basic attitude [...] were led to formulate criticisms which proved constructive and which would have set some development in motion; and, in some cases, to conduct experiments which, conceived to refute Einstein's hypotheses, in fact provided a corroboration of them (Bergia 2005, pagg. 263-264).

Then Bergia deeply describes the role and the work of Levi-Civita and Ricci-Curbastro, and even evidences how Einstein recognized their role in the mathematical tools for GRT:

Einstein was on several occasions explicit in recognizing the fundamental role played by the work of Italian mathematical physicists, Levi Civita in particular, as well as Ricci, in the construction of his theory.

⁸⁴⁴ Bergia S. (2005), "Il contributo italiano alla relatività", *Bollettino dell'Unione Matematica Italiana*, Serie 8, Vol. 8A-La Matematica nella Società e nella Cultura, 2, pagg. 261–287. (Bergia 2005) in the following. See also: http://www.bdim.eu/item?id=BUMI_2005_8_8A_2_261_0. Accessed 24 September 2020.

⁸⁴⁵ The original Italian text contained in these excerpts is in Addendum 3, p. 1048.

In the article of 1916 in which he presented his construction in an organic way, he wrote: *“The mathematical apparatus useful for the general relativity theory, lay already complete in the “Absolute Differential Calculus”, which were based on the researches of GAUSS, RIEMANN and CHRISTOFFEL on the non-Euclidean manifold, and which have been shaped into a system by RICCI and LEVI-CIVITA, and already applied to the problems of theoretical physics”* (Einstein statement in Einstein A. (1916): “Die Grundlagen der allgemeine Relativitätstheorie (The Foundation of the Generalised Theory of Relativity)“, *Annalen der Physik*, 49, p. 769, cited in (Bergia 2005), footnote n. 20, pag. 275. *Italics* in the original text).

Among the scholars who contributed to the development of RT in Italy, Bergia quotes Federigo Enriques:

Federigo Enriques deserves special mention. [...] Member of the Academy of Sciences of the Institute of Bologna since 1902, he promoted the appointment of Einstein as a corresponding member of the Academy. Einstein was to give three lectures there on 22, 24 and 26 October 1921. Enriques himself presented the speaker on the occasion of the first of them. The event caused a remarkable echo, and contributed to the diffusion of relativistic themes in the Italian cultural environment (Bergia 2005, pagg. 278).

An excellent contribution is given by Ugo Giacomini, in his essay “Esame delle discussioni filosofico-scientifiche sulla teoria della relatività”⁸⁴⁶. A in-depth analysis of the various criticism and interpretations of RT emerges, as I have shown for QM. This is because a well-established theory with several confirmations as RT went to touch some basic concepts such as space and time, which are founding themes in Western philosophy and the subject of a neverending debate. According to Giacomini, the criticisms of RT can be grouped in two stages⁸⁴⁷:

[...] the first, which runs from 1920 to 1930 or so, is the one in which the fundamental neo-Kantian, Machian and empiriocriticalist criticisms appear, as well as those of Whitehead, Schlick and Bergson.

⁸⁴⁶ Giacomini U. (1972), “Esame delle discussioni filosofico-scientifiche sulla teoria della relatività”, in Geymonat L., *Storia del pensiero filosofico e scientifico. Vol 6. Il Novecento*, Milano: Garzanti, pagg. 439-468. (Giacomini 1972) in the following.

⁸⁴⁷ The original text in Italian of all these excerpts by Giacomini is in Addendum 3, pp. 1049-1050.

The second moment, which can be traced back to 1950, is the one in which the criticism of Bridgman's operationism and neo-positivist tendencies develop. Even if they take place in this period, i.e. after 1930, a separate place deserves the criticism of the Russian Marxists, because what happened in Russia between 1930 and 1940 has no relation whatsoever to the studies of scientific and philosophical criticism in Western Europe of that period. The interest of Soviet philosophers is to confront Einstein's theories, starting from the assumptions of dialectical materialism, and this provokes a heated debate among the various tendencies of Soviet Marxism which has no counterpart in Western epistemology (Giacomini 1972, pag. 439).

Then Giacomini describes the two kind of criticisms. The first one wants to make Einstein's work a part of the philosophical currents supported by the individual scholar; whereas the second one dwells instead on the limits of theory:

[The criticisms of the first group] are all attempts to relate Einstein's thought to the problems and perspectives of a particular philosophy, and try to underline how the theory of relativity has confirmed both kantism and the philosophy of evolution, and so on.

[...] after 1930, when [...] criticism of dialectical materialism and the founders of quantum mechanics developed, we see that it is no longer a question of imposing certain philosophical categories to explain the meaning of relativity, but rather of showing its limits and defects from various points of view, in particular from the cognitive value of theories. In other words, they are mainly methodological criticisms. After all, while in 1920-30 an attempt was made to philosophically explain the theory of relativity, after 1930 criticism was aimed at evaluating the cognitive scope of this theory. Then in 1949 [...] the criticisms move to particular points, [...] deepening of specific themes, which are no longer explanations of the global meaning of the theory of relativity, but criticisms of its gnoseological value. Finally, the studies that appeared around 1960 almost all deal with a single theme, which, moreover, is the most important in Einstein's thought, that of the relations between geometry and physics.

A brief analysis of the criticisms of the theory of relativity reveals the permanent opposition between operational and ontological interpretation of the theory, the recurrent attempt to make space-time pass through either as a mere construction of the knowing subject or as a substance within which events take place (Giacomini 1972, pagg. 439-440).

[...] In the face of the theory of relativity, the various conventionalisms that had emerged from the crisis of positivism were insufficient, which could not admit that Einstein's theory had a real scope for the transformation of concepts.

The two theses of total ontologism and total conventionalism came out jokes in the course of the debate, perhaps because they lacked the tools to understand the historical origin of Einstein's ideas and their possibility of realization. In fact, in Einstein's thought there is a whole tradition, born in 19th century Germany, of the geometrization of physics, which dates back to Riemann and Helmholtz. [...] It does not pose the problem of the difference between ontologism and conventionalism because it believes that it identifies the greatest possible objectivity of physics with the clearest and most heuristically perfect geometrical form.

[Spiritualistic criticism in turn fails] from the methodological point of view because it insists too much on the free creativity of the human mind. The most penetrating criticisms are instead those that distinguish between the theory of general and special relativity by saving the latter and not committing themselves to the cognitive value of the former. [...] Equally interesting are the criticisms that start from the centre of relativity, i.e. from the relationship between physics and geometry [...] to make it clear that the important thing about the theory of relativity is the direction of research, the type of investigation indicated, i.e. the scientific tradition present in it (Giacomini 1972, pagg. 440-441).

Giacomini illustrates the several interpretations of RT, still grouping them in: Conventionalist, Machian, Kantian, spiritualistic and neo-positivist interpretation in the period 1918-1930; Criticisms of quantum activists and operationists between 1930 and 1940; and Criticism of the theory of relativity within the Soviet dialectical materialism in 1930s.

Of course, the far-reaching analysis made by Giacomini is updated to 1972. Today, new interpretations arise. Anyway, often fierce criticism is always present.

Umberto Bartocci, at the University of Perugia in the 1990s, on the sidelines of a 1999 conference on Galileo, launched a heated debate, namely, an “inquiry about a possible falsification of special relativity” among all dissident physicists in the relativistic field, asking everyone the best mathematical and general argument against RT. In practice, according to Bartocci, the defenders of RT misinterpreted its concepts and anyway, as James Paul Wesley reports in a letter to Bartocci himself, the SRT “presents obvious horrendous mathematical contradictions and errors as pointed out by thousands of individuals since 1905 onwards”.

Again in MBPA I found a file with all the debate via email, very much discussed and with very dissimilar stances. It is very interesting but it is 48 pages long and would take us a long way.

I thought a lot about including it in Addendum 3 but then I preferred not to do it (even if there are some reference to Selleri), also because in the debate some people express a desire not to bring these discussions in a public chat. Anyway in it there is an answer to Selleri, which talks about theories equivalent to SRT, by George Galeczki. The answer that Selleri receives is “Dear Franco, there cannot be ‘theories equivalent to SRT’ since SRT is wrong. There is a profound, physical difference between theories with and without a preferred reference frame!”.

As I have shown in this paragraph, the opposition to RT continues to be strong, for the most varied reasons.

In the next paragraph, I will introduce how Selleri started to dedicate himself to FRT in 1990. I will show how his approach has moved from an in-depth analysis of the weaknesses of the SRT. Which, without detracting from the enormous power inherent in his predictions, shows, since Einstein’s own integration with the GRT, that he has some weaknesses. A really diverse attitude with respect to other many opponents, sometime quite irrational.

Selleri always recognized the strength of the key theories of the 20th century, that is, QM and RT. But his belief in a simpler truth, together with some bugs inserted in the above theories, has ever pushed him to search for an alternative.

3.4-SELLERI’S MOTIVATION, INFLUENCES, AND EARLY APPROACHES: THE 1990 PAPERS

Almost 80 years after the hostilities and the resistance that we have seen, born practically at the same time as the birth of RT itself, Selleri entered the world of FRT, continuing an opposition to SRT, albeit from a different point of view with respect to some scholars I introduced in the previous section, that is, as a theoretical physicist and with a very pondered analysis. I established the motivations behind Selleri’s choice to dedicate himself to RT.

Among them, his fundamental choice of the wave-particle duality which implies the need to find a means for the propagation of the real empty waves, recognizing it in an ether quite similar to that one reintroduced by Einstein in the 1920s.

The early Selleri's activity in FRT covered the years 1990-1993. Starting the 1996, he experienced another phase. As I will shown later, the first two papers in FRT were released in 1990. After two years without publishing anything in FRT, Selleri covered the next three years with two papers per year. So, everything seems to start with the 1990 articles.

Moreover, I have already shown in the previous chapter (see page 544) that the first contact between Selleri and relativity comes in May 1983, at the beginning of the Bari conference, when Popper asked him what Selleri thought about it, a point which finds Selleri disconcerted, creating a certain disappointment on Popper's side.

After this event, there is a handwritten letter from Selleri dated June 1985, in which he asks for dispensation from the graduation choir board meetings in order to study SRT. Selleri wrote:

Dear Natali, during the whole month of July I will be [...] in Nice, Brussels, Amsterdam to study the teaching of special relativity with an EEC fund.⁸⁴⁸ (Selleri to S. Natali, 27 June 1985, AGAB).

Below I am going to introduce five letters dated between 1988 and 1991 in which SRT is already in the current writings by Selleri. I thought they could be interesting for an accurate dating of the beginning of the start of Selleri's research in FRT.

In the first letter, sent to Roberto Monti, Selleri, among several comments, underlines his thoughts on the principle of relativity and affirms how a paper in FRT was not in construction, was not planned.

⁸⁴⁸ "Caro Natali, durante l'intero mese di Luglio sarò [...] a Nizza, Bruxelles, Amsterdam per studiare l'insegnamento della relatività speciale con un fondo CEE" (Selleri to S. Natali, 27 June 1985, AGAB).

Then he points out how dissidents relativists are still growing, still revealing a knowledge of the background related to FRT:

[...] There are things that one writes to a person, but that one would not want to see spread to the entire Italian people. Example? I have told you not to believe in the principle of special relativity, but I would not tell the same thing (which is true) to a person who would immediately judge me badly (e.g. Amaldi), if not after having articulated it well in a scientific article. This article I do not know when (nor for sure if) I will do it, having for the moment other priorities.

[...] On the whole, the army of relativists that is forming in dissent with Einstein is impressive: Aspden, Silvertooth, Monti, Essen, Marinov, Wesley, etc. It may be that a tide is mounting that will lead to great things. There you are the representative of Italian physics. It is a good physics, equal to the best in the world and perhaps, culturally speaking, the absolute best⁸⁴⁹ (Selleri to Roberto Monti, 20 April 1988-FSPA. My underline and bold).

Then two letters between Selleri and Duffy follow. Michael Duffy has been the main promoter of the *Physical Interpretations of Relativity Theory* (PIRT) conferences⁸⁵⁰ at the Imperial College in London, starting the 1988, which currently are organized in Bauman University at Moscow. Only one year after the first edition of the conference, duffy invited Selleri in the next 1990 PIRT. It could be a confirmation of a certain general popularity of Selleri in the field of physics. PIRT is a conference centered on relativity. Could it be a clue that even Selleri was already seen as a reference on the FRT topic, so much so that Duffy was asked him who to invite? Notwithstanding being PIRT a relativity conference, in 1990, according to the letter, Selleri will deal with EPR. And, in addition, he also asks some information on relativity experimental works made by Silvertooth (his interest for experiments is still always strong and preminent). Hence, it looks like Selleri is leaning towards the FRT. But...

In the proceedings of the same 1990 PIRT he will publish his first paper in FRT! So, between 1988 and 1990 the switch is still under construction!

⁸⁴⁹ The original Italian text is in Addendum 3, p. 1050.

⁸⁵⁰ PIRT: See: <http://www.pirt.info/?lang=eng>. Accessed 24 September 2020.

At the beginning, Selleri discloses his willingness to talk about EPR and explain his opinion on Dirac's ether, still evidencing it as a good tool for relativity but misleading for quantum theory:

Thanks for sending me the proceedings of your first conference on relativity (1988) [...] you are very kind to ask for my opinion about what to do next [...] **I will be happy to take part in the London 1990 conference and that I could try to explain why the EPR paradox is important for all physicists**, especially those, like yourself, who have a rationalistic outlook, as I see from your paper. It is the very locality of the physical laws that is at stake, in all reasonable definition of locality, probably some fundamental change is needed in quantum theory, this is my opinion.

My opinion about Dirac's aether is that it is extremely important for relativity, but misleading for quantum theory, and that people working on Dirac's aether there are almost certainly on the wrong track.

Of course, I do like the idea, philosophically speaking, but not always a nice idea is also a true one. In quantum phenomena energy-momentum conservation has been checked to hold in individual interactions with striking accuracy. Therefore there is no place for locally determined Brownian motion.

Some people work on nonlocal Brownian motion, so that two distant particles in a scattering process should essentially perform the same Brownian motion in all detail! I really dislike this idea. Moreover, if one of the two particles is detected the other one should immediately stop and forever all interaction of stochastic nature with the aether. This is far too much for me to take. Conclusion: **pay attention to the Dirac aether in quantum theory, you risk to embark a lot of junk** (Selleri to Duffy, 14 March 1989-FSPA. Selleri underline, my bold).

Then, Selleri asks information about the outcome of Silvertooth experiment on SRT, still revealing his commitment in the field or RT already here in the early 1989. And maybe since 1988, looking at the first letter to Monti, in which he dealt with the principle of relativity:

[...] Second, I am very interested in special relativity and I would welcome the possibility to understand if you have generally accepted conclusions about experiments such as that of Silvertooth. Are they valid? What is their meaning? Will this be discussed in London or the problem is considered obsolete? (Selleri to Duffy, 14 March 1989-FSPA. My underline).

Finally, Selleri shows a significant detail, by communicating his commitment with the work of Halton “Chip” Arp⁸⁵¹:

[...] Third, I am translating into Italian the book by Halton Arp and I am very positively impressed [...]. In my opinion he is giving a new wonderful cosmology and proofs that the “big bang” needs a general revision (Selleri to Duffy, 14 March 1989-FSPA. Selleri underline, my bold).

Duffy answered very soon with lots of technical detail concerning Silvertooth experiment. The wrong solution of the ether drift in Silvertooth’s view is explained:

[...] a paper dealing with the epr paradox would make an excellent contribution [...]. I was interested to read your warning about the usefulness of the Dirac aether concept and the danger in trying to use it in quantum theory. [...] you mentioned Silvertooth and his experiments in your letter. As a matter of fact the Silvertooth experiments were discussed, the authority being Chalmers W. Sherwin during the meeting of 1988 in San Diego. Dr. Sherwin reported that the effect detected by Silvertooth which was interpreted as some “ether drift” effect was in fact due to an electrical effect arising from the method of coupling the experimental apparatus to the instruments. The effect disappeared when the coupling was changed from an AC coupling to a DC coupling. We were given to understand that Silvertooth acknowledges that the supposed “ether drift” which his apparatus detected was a spurious effect, arising from this source (M. Duffy to Selleri, 20 March 1989-FSPA. My underline).

Another document comes from the letter sent by Selleri to Giancarlo Cavalleri, which had written a paper together with C. Bernasconi⁸⁵², not among his most cited ones.

⁸⁵¹ Halton Christian “Chip” Arp (1927-2013): He was known for his 1966 Atlas of Peculiar Galaxies, which (it was later theorized) catalogues many examples of interacting and merging galaxies, though Arp disputed the idea, claiming apparent associations were prime examples of ejections. Arp was also known as a critic of the Big Bang theory and for advocating a non-standard cosmology incorporating intrinsic redshift. See https://en.wikipedia.org/wiki/Halton_Arp; <https://www.haltonarp.com/articles>; <https://www.nytimes.com/2014/01/07/science/space/halton-c-arp-astronomer-who-challenged-big-bang-theory-dies-at-86.html>; <https://skyandtelescope.org/astronomy-news/cosmologist-halton-arp-19272013/>. All accessed 24 September 2020. It sounds curious how many sites are currently dedicated to him, which experienced, according to Selleri, a strong disagreement and hostility for his ideas during his life.

⁸⁵² Cavalleri G., Bernasconi C. (1989), “Invariance of Light Speed and non Conservation of Simultaneity of Separate Events in Prerelativistic Physics and Viceversa in Special Relativity”, *Nuovo Cimento B*, 104, pp. 545-561.

The letter was written between the first two papers published between September and November 1990 by Selleri in FRT. Within the paper, the authors dealt with the simultaneity, another issue which Selleri challenges in his activity in FRT, together with lorentzian ether. Selleri asks for some elucidation, still showing he has maybe already challenge the issue:

Your article with Bernasconi is very beautiful and shiny, it shows that you are a thinking head. [...] there is only one thing I do not understand and I would ask you to explain it to me. [...] . You derive the LT starting from the contraction of the lengths and the expansion of the time intervals. Otherwise the bases are classic, for example you combine the speeds like this: $c-v$. I have no difficulty in tuning in with you, your philosophy suits me very well. But the point is that after getting the LT, going on in the usual way that you know well, you find that the speeds combine instead [differently]⁸⁵³ (Selleri to G. Cavalleri, 16 october 1990-FSPA. Selleri underline).

In a letter to Blandino, Selleri is ready to widely explain the matter, still confirming his knowledge of the paper by Bell, another influence he received, together with Builder, Prokhovnik, the same Cavalleri and Bernasconi, as I am going to explain soon. Selleri explains how RT looks like a way to protect locality against the refusal by QM. In the last statements, clearly explaining his preference to Lorentz RT, he seems not completely ready to dedicate himself entirely to FRT, still choosing an EPR solution related to the overcoming of QM:

[...] these exceptions make the conclusion of a “normal” validity of the locality not entirely convincing. However, these exceptions are blocked by RT: each signal takes a finite time to propagate between two points A and B, and those in B cannot know what happened in A before that time has passed. The QM, on the other hand, requires instantaneous propagation of influences between A and B by reducing the wave packet. And mind you, this is also required by the theory of quantized fields in its relativistic version! Which proves that this theory is in reality not very relativistic and that it is a logical mess. However, the point remains this: Bell’s inequality, which can be deduced from realism + special relativity, is in contrast to QM.

⁸⁵³ The original Italian text is in Addendum 3, p. 1050.

This contrast is at an experimental level and constitutes a very serious crisis in contemporary physics. In essence it is necessary to choose between RT and QM.

A way out invoked by Bell himself is that of a return to Lorentz's "relativity", with his absolute good time, with the lengths that really contract, with the ether and "perhaps" (he believed it, I believe it little) with the opportunity of exceeding the speed of light with respect to a system in motion.

Mind you, I like Lorentz [much more than] Einstein, philosophically speaking, but I believe little in this solution of the EPR paradox. My favourite solution is the dismantling of quantum theory⁸⁵⁴ (Selleri to Blandino, 3 April 1991-FSPA-My underline).

In the interview with Barone, he confirmed me that Selleri's influences stand in the papers by H.A. Lorentz, Builder, Prokhovnik, Cavalleri and Bernasconi, and Bell.

In the programme of "Frontiers of fundamental physics"⁸⁵⁵, a conference held in Olympia in 1993 (and co-organized by Selleri), among the various issues, the host had to debated on the Lorentz interpretation of RT as well as the need of a wave-propagating medium, that is, the ether. In the programme, surely arranged also by Selleri, is stated:

[...] Lorentzian relativity: in spite of the obvious validity of Michelson-type experiments people have gone too far in denying the existence of a wave-propagating medium (ether). The possibility to introduce again a preferred frame will be discussed in the spirit of Lorentz, Builder, Prokhovnik, Bell and other physicists dualism (MBPA-Olympia 1993).

By the way, Selleri made his first public talk on FRT at the above conference. The remarkable work by H.A. Lorentz is well known all over the world⁸⁵⁶.

⁸⁵⁴ The original Italian text is in Addendum 3, pp. 1050-1051.

⁸⁵⁵ C52. 1993, 27-30 September, Olympia, Greece

FRONTIERS OF FUNDAMENTAL PHYSICS

Org. by: NRC Demokritos (Athens) and Physics Dept. of Bari University

⁸⁵⁶ Lorentz H. A. (1895), *Versuch einer Theorie der elektrischen und optischen Erscheinungen in bewegten Körpern*, Leyden: E.J. Brill/Collected Papers 5, pp.1-137.

Lorentz H. A. (1904), "Electromagnetic phenomena in a system moving with any velocity less than that of light", *Proc. R. Acad. Amsterdam*, 6, pp. 809-; Collected Papers 5, pp. 172-197.

Lorentz H. A. (1909), *The theory of electrons and its applications to the phenomena of light and radiant heat*, Lipsia: Teubner.

Lorentz H.A., Einstein A., Minkowski H., Weil H. (1923), *The Principle of Relativity*, Methuen, London/New York: Dover Publications, Inc.

Selleri himself recommends to Barone the papers on relativity he read, i.e., (Builder 1958); (Prokhovnik 1967); (Post 1967); (Bell 1976); (Prokhovnik 1979); and (Cavalleri, Bernasconi 1989)⁸⁵⁷.

(Builder 1958) is the first paper which influenced Selleri's approach: in the abstract, Builder undelines clearly his thought:

The relative retardation of clocks, predicted by the restricted theory of relativity, demands our recognition of the causal significance of absolute velocities. This demand is also implied by the relativistic equations of electrodynamics and even by the formulation of the restricted theory itself. The observable effects of absolute accelerations and of absolute velocities must be ascribed to interaction of bodies and physical systems with some absolute inertial system. We have no alternative but to identify this absolute system with the universe.

Thus, in the context of physics, absolute motion must be understood to mean motion relative to the universe, and any wider or more abstract interpretation of the "absolute" must be denied.

Interaction of bodies and physical systems with the universe cannot be described in terms of Mach's hypothesis, since this is untenable. There is therefore no alternative to the ether hypothesis. It is shown that this is compatible with the restricted theory of relativity and even provides a tenable basis, when taken together with the principle of relativity, for that theory. It is shown that the hypothesis provides a satisfactory and sufficient causal explanation of the predicted relative retardation of clocks, and attention is drawn to its striking pedagogical and heuristic advantages (Builder 1958, pag. 279).

Builder accepts the ether hypothesis, still seeing a natching with SRT. However, in his conclusion, Builder strongly discourages the SRT approach, which is an useless, complex explanation of phenomena, which can be easily outlined with absolute concepts and with the ether:

⁸⁵⁷ Builder G. (1958), "Ether and relativity", *Australian Journal of Physics*, 11 (3), pp. 279-297;
Prokhovnik S.J., (1967), *The Logic of Special Relativity*, Cambridge: Cambridge at the University Press;
Post E.J. (1967), "Sagnac effect", *Reviews of Modern Physics*, 39 (2), pp. 475-493;
Bell J.S. (1976), "How to Teach Special Relativity", *Progress in Scientific Culture* I (2), today available only in
Bell J. S. (1987), *Speakable and unspeakable in quantum mechanics*, Cambridge: Cambridge University Press, Chapter 9, pp. 67-80;
Prokhovnik S.J., (1979), "An introduction to the neo-lorentzian relativity of Builder", *Speculations in Science and Technology*, 2 (3), pp. 225-242;
Cavalleri G., Bernasconi C. (1989), "Invariance of light speed and nonconservation of simultaneity of separate events in prerelativistic physics and vice versa in special relativity", *Il Nuovo Cimento B*, 104 (5), pp. 545-561.

The permissibility of retaining the concepts of absolute space, of absolute motion, and of the ether, and the fact that we can assign to these concepts definite and clear meanings compatible with the restricted theory of relativity, has striking pedagogical and heuristic advantages. The conceptual difficulties associated with the restricted theory [of relativity] all arise out of the denial that these absolute concepts are permissible, and out of the consequent attempts to avoid them in the presentation of the theory. It is frequently maintained that the theory has forced us to discard entirely the old-fashioned commonsense notions of time and space; but nothing comprehensible or definable has been offered in their place. Moreover, **any questions** as to what causes the relativity of simultaneity, the measured constancy of the velocity of light in all inertial reference systems, or the reciprocity of relativistic variations of length, of mass, and of clock rates, are evaded by vague references to the principle of relativity, to the four-dimensional character of space-time, and so on. On the other hand, **the presentation of the restricted theory in terms of the absolute concepts (following generally the lines of its development by Poincare and Lorentz) involves no conceptual difficulties.**

The relativity of simultaneity, the reciprocity of relativistic variations, and the constancy of the measured velocity of light, then all appear simply as comprehensible effects of the motions, relative to the ether, of the bodies observed and of the measuring instruments used. The only other important factor that has to be taken into account is the impossibility of making true measurements of light velocity over a one-way path, so that we are restricted to measurements which give only an average value of the velocity of light transmitted over a go-and-return path. On this basis the presentation involves no more than a few simple calculations, and can be made easily intelligible and comprehensible to junior university students. **The heuristic value of this approach is also noteworthy. It reduces many questions, which would otherwise lead to discursive and inconclusive arguments, to a form in which a simple and conclusive answer can be given.**

For example, the relative retardation of clocks predicted by the restricted theory becomes a simple and intelligible consequence of the motion of the clocks relative to the ether. Similarly, we are enabled to answer intelligibly what are otherwise very difficult and contentious questions, namely: Are the observed relativistic variations of length, of mass, and of clock rates, real?

[...] How can “reality” be reconciled with their reciprocal character? The answer is, of course, simply that these effects are really-observable, but that what corresponds to them in nature are real effects caused by the motions, both of the observed bodies and of the observing instruments, relative to the ether. It is worth remarking that the pedagogical and heuristic advantages of this approach depend only on the tenability of the ether hypothesis and on the admissibility of the absolute concepts, i.e. on their compatibility with the restricted theory and with the general body of physical knowledge. These advantages would remain even if there were also available an alternative and equally tenable set of hypotheses and concepts.

It has not been practicable to give detailed references to the work of the many previous authors like Wiechert, Lodge, Ives, who have put forward important arguments in favour of the absolute concepts. Still less would it have been practicable to refer to, or to answer, the innumerable ebullient attacks made on the ether hypothesis in the last 50 years. I have, in fact, tried to confine myself to material relevant to the discussion of what seems to me to be a new, and perhaps decisive, consideration in favour of the absolute concepts, i.e. the recognition (Dingle 1957) that the relative retardation of clocks, predicted by the restricted theory, is an absolute effect which demands our recognition of the causal significance of absolute velocities (Builder 1958, pagg. 296-297. My underline and **bold**).

Builder approach introduces the one-way velocity of light measurement, a causal approach to absolute velocities. He points out that the ether hypothesis is the only possible one, and he rejected the Mach approach, being it untenable. Selleri will be still very close to Builder way of thinking. Selleri researched on all topic I bold in the Builder statement, i.e., the relativity of simultaneity, the measured constancy of the velocity of light in all inertial reference systems, the reciprocity of relativistic variations of length, of mass, and of clock rates, and the one-way velocity. And Selleri will always favour the Lorentz approach. I think this is definitely the first and main step in Selleri's path, sort of guideline which, together with the following others papers, more focused on specific problems, paved his walk on his activity in FRT.

In the abstract of (Post 1967) is stated the description of Sagnac interferometer, which could give new energy to the 1913 Sagnac effect for the absolute rotation, namely for the existence of a privileged reference system. Post made also a review of the experiments and the older effects. The optical path obtained with the interferometer is compared to the electromagnetic description:

A revived interest in the Sagnac effect has recently resulted from the development of the self-oscillating laser version of the original Sagnac interferometer. The Sagnac interferometer or ring laser is an example of an electromagnetic sensor of absolute rotation, so historical and theoretical background information is useful in evaluating the possibilities of electromagnetic sensing of absolute rotation.

A critical literature study of the many experimental ramifications and the older kinematical theory of the effect is presented. This geometric optical theory is then complemented and compared with more recent work that is based on a physical optical analysis using a complete electromagnetic description of the phenomenon (Post 1967, pag. 475).

I would like to recall that Selleri will work a lot on the Sagnac effect, one of his *experimentum crucis* in FRT in order to discriminate between SRT and his new theory.

Another influence came from (Cavalleri, Bernasconi 1989), in which the authors claimed that:

Light speed invariance and nonconservation of simultaneity of separate events are often considered as peculiar properties of Einstein's special relativity (SR). On the contrary it is possible to formulate SR so that simultaneity be conserved and light speed be no longer invariant. *Vice versa* it is possible to formulate prerelativistic, or Galilean physics (GP), so that simultaneity be no longer conserved and light speed be invariant. Simultaneity conservation (or not) and light speed invariance in both GP and SR depend on the arbitrary way in which the clocks of reference systems are synchronized. This arbitrariness leads to infinite possible coordinate transformations (T) in GP, the simplest and most convenient of which are the Galilean T . Similarly there are infinite possible T in SR and the Lorentz T is one of the two most convenient ones. The other suitable T is that of Ives-Tangherlini which implies absolute simultaneity and a noninvariant speed of light. This allows a revival of the ether, although no longer considered as a classical fluid, but as the electromagnetic field radiated by all particles of the universe.

This ether should produce the slowing down of clock rate and longitudinal shortening of the moving rods which are the peculiar properties of SR (Cavalleri, Bernasconi 1989, pag. 545).

They dealt with the synchronization. They showed how the arbitrariness of synchronization leads to infinite transformations and theories equivalent to RT, once the clocks are synchronized. The theories depend on the synchronization parameter. The research in new general transformations, using also the Ives-Tngherlini ones, will be a remarkable cornerstone in Selleri's future research in FRT.

The (Bell 1976) paper is a brilliant essay on the way to teach relativity, relating to the earlier ideas. A knowledge, this latter, more fruitful in recognizing the counterintuitive results of SRT:

If I had the opportunity to teach this subject I would emphasize the continuity with earlier ideas. Usually it is the discontinuity which is stressed, the radical break with more primitive notions of space and time. Often the result is to destroy completely the confidence of the student in perfectly sound and useful concepts already acquired (Bell 1976, pag. 67).

Bell made an experiment using three spaceships, bounded by a thread. When the rockets speed up the thread, normally right long, becomes shorter, because of Fitzgerald contraction, and finally it breaks:

It must break when, at sufficiently high velocity, the artificial prevention of the natural contraction imposes tolerable stress. Is it really so? (Bell 1976, pag. 68).

Bell asks for some answer at CERN. A lot of people gave the wrong answer.

A distinguished experimental physicist refused to accept that the thread would break, and regarded my assertion, that indeed it would, as a personal misinterpretation of special relativity. We decided to appeal to the CERN Theory Division for arbitration and made a (not very systematic) canvas of opinion in it. There emerged a clear consensus that the thread would **not** break.

[...] It is my impression that those with a more classical education, knowing something of the reasoning of Larmor, Lorentz and Poincaré, as well as that of Einstein, have stronger and sounder instincts (Bell 1976, pag. 68. **Bold** in the original text).

In the footnote n. 3 and n. 4 of Bell paper, he told about acceleration and velocities:

Violent acceleration could break the thread just because of its own inertia while velocities are still small. This is not the effect of interest here. With gentle acceleration the breakage occurs when a certain velocity is reached, a function of the degree to which the thread permits stretching beyond its natural length.

[In a circular orbit] it follows from Lorentz invariance and stability considerations that any sufficiently smooth acceleration process will produce the same Fitzgerald contraction and Larmor dilation (Bell 1976, pag. 78).

After having explained very clearly the contraction and dilation by Lorentz-Fitzgerald-Larmor-Poincaré, as well as the SRT, passing by moving objects to moving subjects, i.e., observers, finally Bell illustrated the differences between Lorentz and Einstein, based on diverse philosophical and style approach, a point and a distinction that will be addressed by Selleri:

The approach of Einstein differs from that of Lorentz in two mayor ways. There is a difference of philosophy, and a difference of style.

The difference in philosophy is this. Since it is experimentally impossible to say which of two uniformly moving systems is really at rest, Einstein declares the notions 'really resting' and 'really moving' as meaningless. For him only the relative motion of two or more uniformly moving objects is real. Lorentz, on the other hand, preferred the view that there is indeed a state of real rest, defined by the 'aether', even though the laws of physics conspire to prevent us identifying it experimentally. The facts of physics do not oblige us to accept one philosophy rather than the other. And we need not accept Lorentz's philosophy to accept a Lorentzian pedagogy. Its special merit is to drive home the lesson that the laws of physics in any one reference frame account for all physical phenomena, including the observations of moving observers. And it is often simpler to work in a single frame, rather than to hurry after each moving object in turn.

The difference of style is that instead of inferring the experience of moving observers from known and conjectured laws of physics, Einstein starts from the hypothesis that the laws will look the same to all observers in uniform motion. This permits a very concise and elegant formulation of the theory, as often happens when one big assumption can be made to cover several less big ones. There is no intention here to make any reservation whatever about the power and precision of Einstein approach.

But in my opinion there is also something to be said for taking students along the road made by Fitzgerald, Larmor, Lorentz and Poincaré. The longer road sometimes gives more familiarity with the country (Bell 1976, pag. 77).

The role played by acceleration and velocities will always be for Selleri a core matter on his study on Twin Paradox, and a matter of strong debates with other scholars. In Selleri's belief, acceleration does not enter in the matter of time dilation in the *Gedankenexperimente* on Twin Paradox. And the acceleration in a circular orbit will be discuss by Selleri in his study on the Sagnac effect. Selleri criticized SRT also doing a philosophical choice, feeling the Lorentz approach as closer to his ideas. His great admiration for Einstein did not prevent him, for the sake of scientific truth, from criticizing

the Machian approach of the German physicist in the building of SRT. The so-called *ipse dixit* is completely useless and harmful for Selleri, who felt himself always really close to Galilei. In addition, Selleri often used the spaceships (chosen by Bell to illustrate the Fitzgerald contraction and the Larmor time dilation) during his talks in seminars at schools.

Finally, the (Prokhovnik 1979) surely constituted a great influence for Selleri, since the author stated in the abstract how Builder approach deserves to be considered basic in every criticism of SRT:

The main ideas of Builder's approach to Special Relativity are outlined and evidence for their self-consistence is presented. It is suggested that Builder provides an unambiguous physical interpretation of Special Relativity which must be taken into account in any critique of the Theory.

These authors dealt with all topics which Selleri will develop: ether and Lorentzian relativity, Sagnac effect and so on. I will exhibit how the great contribution of Selleri is based on the so-called Inertial Transformations (further referred to as IT), which replace LT, and are related to a synchronization parameter which presents diverse values for every of infinite theories equivalent to SRT. Selleri will find a specific value for this parameter, which disregards between all the above theories.

Only the so-called "Weak Relativity", which shows an absolute synchronization, is able in explaining all results of SRT and also ten bugs found in SRT, which is not able to solve them, unless there are strange logical leaps. The case of the Sagnac effect sounds basic, because, being it experimental, can discriminate between the two theories. Later on I will introduce in detail the explanation of the Sagnac effect as proposed by Selleri. In his opinion, Selleri made a great work which were very proud with. This path was reached after several step and some impasse, some *cul-de-sac*. As is always the case in science in regular progress towards new knowledge. But I am going to reveal it later, as told before.

Concerning the role of ether after Einstein reconsidered it, as well as the role of the above scholars who contributed to the neo-Lorentzian theories of ether, Barone well expressed in some excerpts taken from his talk on “La TRR⁸⁵⁸ a 90 anni dalla sua formulazione”⁸⁵⁹:

Up to now, there is a strand of research in physics whose aim is to try to recover the ether, whose presence could be observed with a new generation of experiments also connected to cosmology.

G. Builder and S. Prokhovnik, Australian physicists, and J.S. Bell, Irish one, worked in this direction, leading the way for many others. Builder, in two articles, “Ether and relativity” and “The constancy of the velocity of light”, both from 1958, published in the *Australian Journal of Physics*, makes a very acute criticism of TRR. In particular, he argued that the speed of light must be interpreted as “the speed of light measured in an IRS and that the measurement must be considered as the average on the outward and return journey”.

[...] Assuming this procedure of synchronization of the clocks and the hypothesis of an ether associated to a universal reference system connectable to the mass of the fixed stars, Builder arrives at the same conclusions as the TRR.

H.E. Ives in an article of 1945, published in *Phil Mag.*, managed to justify the contraction of the lengths, the dilation of the times and the increase of the mass by studying the interaction of an electromagnetic wave with a free particle, completely reflecting and imposing the conservation of the energy and the momentum both in the absolute reference system and in an inertial one.

Ruderfer and Prokhovnik referring to the works of Ives and Bouilder, formulated a relativistic theory of the ether, which explains all the results of Einstein. The set of these attempts to formulate a theory based on the ether, compatible with TRR, gave rise to the so-called “neo-Lorentzian theories of the ether” [...].

On the other hand, cosmological observations on the isotropy of matter in the Universe, on the background radiation at 2.7 K and on the origin of dark matter, necessary to justify the high speed of rotation around its own axis of some stars placed at the limits of spiral galaxies, Milky Way included, and which at 90% would be diffused in a spherical halo around them, lay in favour of a fundamental reference system for the propagation of light (Michele Barone 1995. My *Italics*).

⁸⁵⁸ TRR is for Teoria della Relatività Ristretta (Restricted Relativity Theory, RRT).

⁸⁵⁹ (Barone 1995) in the following. The original text in Italian is in Addendum 3, p. 1051.

Now I would like to start with the analysis of the first two papers written by Selleri in 1990, centered in an analysis of the SRT in the light of the ether theories.

In Chapter 2, I introduced the travel notebook in which Selleri received an advice by Winterberg in order to publish a paper in FRT on the German journal *Zeitschrift fur Naturforschung* (see page 567). It was his second papers, though, because the first one was released on the proceedings of PIRT 1990: “Special Relativity as a Limit of Ether Theories”⁸⁶⁰.

Selleri showed the unsteadiness of the SRT, which easily could be refurbished in an ether theory, surely simpler to develop, once you accepted the existence of a privileged frame, as Selleri did. Since the title is clear that SRT for Selleri is only one among the possible, infinite theories, as expressed by Cavalleri and Bernasconi:

By assuming the validity of the principle of inertia and the existence of a privileged frame, the transformation laws between inertial frames are investigated. If the principle of relativity is finally assumed, the two unknown functions of absolute velocity acquire their well known relativistic expressions. It is concluded that special relativity theory is “unstable”, in the sense that any shift away from it, however small, leads to an ether theory (Selleri FRT 1990a, pag. 508).

The interest in the foundations of the Special Relativity Theory (SRT) has recently been growing for at least two reasons:

- i) the historical researches carried out by Keswani (1966), Zahar (1973) and Tyapkin (1982), that have led to a better understanding of the roles of Lorentz and Poincaré and on the alternative lines of thought that are logically open;
- ii) the discovery of Bell’s inequality, and the consequent development of the Einstein, Podolsky and Rosen paradox that has become an experimentally testable contradiction. In this connection Bell has explained that “The reason I want to go back to the idea of an aether here is because in these EPR experiments there is the suggestion that behind the scenes something is going faster than light”.

Many interesting papers have been devoted to the study of SRT. Among the recent ones one can mention those by Sachs, Kennedy, Spavieri, Maccarrone and Recami, Maciel and Tiomno, Winterberg and Sjödin.

⁸⁶⁰ Selleri F. (1990), “Special Relativity as a Limit of Ether Theories”, in *Physical Interpretations of Relativity Theory*, C. Duffy (ed.), London: British Society for the Philosophy of Science, pp. 508-514. (Selleri FRT 1990a) in the following.

Two conferences (PIRT 88, 90) have mostly been devoted to SRT in recent years and the quality of the papers presented is a sure sign that the whole field is growing (Selleri FRT 1990a, pag. 508).

[...] one might feel an interest in “improving” the theory of relativity and in finding a formulation that avoids the postulate of relativity altogether. A preliminary investigation of this problem is described in the following sections [principle of inertia and its consequences, physical and formal distinction between absolute and moving inertial frames, units of space and time, transformations laws from absolute to moving frames, and between to moving frames, relativistic limit] (Selleri FRT 1990a, pag. 509).

[...] $E(v) = v/c^2$, eventually with $c=\infty$, is a *necessary* consequence of the relativistic postulate. Any other function $E(v)$, even if very very close to v/c^2 , but different from it, would single out a privileged frame and would thus effectively constitute an ether theory. The same conclusion holds for the limit $\Delta(v)=1$ required by the relativistic assumption. We are so led to the conclusion that special relativity as a theory is “unstable”, in the sense that any violation of the conditions $E(v) = v/c^2$; $\Delta(v) = 1$ ** however small, leads necessarily to an ether theory. It follows also that there must necessarily exist ether theories compatible with all the experimental evidence that is usually invoked to stress the validity of special relativity.

Depending on the violation of ** one can consider ether theories of three types

- 1) $E(v) = v/c^2$; $\Delta(v) \neq 1$
- 2) $E(v) \neq v/c^2$; $\Delta(v) = 1$
- 3) $E(v) \neq v/c^2$; $\Delta(v) \neq 1$ (Selleri FRT 1990a, pag. 513).

The second paper was released two months after, on November 1990: “Space-Time Transformations in Ether Theories”⁸⁶¹. It is basically the same paper of (Selleri FRT 1990a), a little bit refitted to be publish on a journal more than on the Proceedings of PIRT. He summarizes the outcomes and the diverse researches on the issue:

By assuming the validity of the principle of inertia and the existence of a privileged frame, the transformation laws (TL) between inertial frames are investigated in ether theories. For one dimensional space the TL’s are fixed up to two undetermined functions of absolute velocity, $\Delta(v)$ and $E(v)$.

⁸⁶¹ Selleri F. (1990), “Space-Time Transformations in Ether Theories”, *Zeitschrift fur Naturforschung*, 46A, pp. 419-425. (Selleri FRT 1990b) in the following.

If the principle of relativity is finally assumed, these functions acquire their well known Lorentzian expressions Δ_L and E_L . It is concluded that special relativity theory is “unstable”, in the sense that any shift, however small, of Δ away from Δ_L and/or of E away from E_L leads to an ether theory. In Earth-based experiments one can expect deviations from c of the two-way and one-way velocity of light of the order of 10^{-12} and 10^{-9} , respectively

The interest in the foundations of the Special Relativity Theory (SRT) has recently been growing for at least two reasons:

- i) The historical researches carried out by Keswani (1966), Zahar (1973) and Tyapkin (1982), that have led to a better understanding of the roles of Lorentz and Poincaré and on the alternative lines of thought that are logically open;
- ii) The realization that Einstein was opposed to ether only around 1905 but later reverted to this conception in connection both with General and with Special Relativity;
- iii) The lasting influence of older papers such as those by Ives (1945, 1979), Dirac (1951), Builder (1958, 1979) and Prokhovnik (1963) in which the formal apparatus of SRT had been considered from an ether point of view;
- iv) The discovery of Bell’s inequality, and the consequent development of the Einstein, Podolsky and Rosen paradox that has become an experimentally testable contradiction. In this connection Bell has explained that “The reason I want to go back to the idea of an aether here is because in these EPR experiments there is the suggestion that behind the scenes something is going faster than light”.

Many interesting papers have been devoted to the study of SRT. Among the recent ones one can mention those by Honig (1976), Mansouri and Sexl (1977), Sjödin (1980), Winterberg (1987), Maciel and Tiomno (1989), Cavalleri and Bernasconi (1989) and Spavieri (1990).

Two conferences (PIRT 88, 90) have mostly been devoted to SRT in recent years and the quality of the papers presented is a sure sign that the whole field is growing (Selleri FRT 1990b, pag. 419. See within this paper for references).

These first two papers are still embryonic and generic, but already based on the analysis of theories that deviate from the SRT and on the unveiling of the state-of-the-art in the field. The clear influence played by the scholars (in the footnote 857 at page 671) addressed Selleri, in these first two papers and in his early activity in FRT, to his path paved with ether theories, absolute reference frame, and the beginning of the weakness and the narrow range of validity of the LT. The generalization of the LT, done with arbitrary parameters, will then acquire a precise meaning, still getting to the Inertial Transformations.

But it will take several years for Selleri. In his path, as in FQM, he will often declare that in a short time there would be profitable developments for his research, in order to promote his work, and against all odds. Sending his papers to other scholars, he will write in several letter that the postulated equivalence between the present impossibility of revealing the motion of the privileged or absolute reference system and the denial of its existence, as well as the analysis of the concepts of absolute space and time, eliminated as a consequence of the conventional relativity of simultaneity, has led to FRT on which it was preferred not to investigate anymore and to a closure against any different thought, which has glorified the theory of relativity as an infallible and perfect truth.

Unlike the FQM field, however, here Selleri will managed to produce an alternative theory.

3.5-THE NEXT ACTIVITY ON FOUNDATIONS OF RELATIVITY THEORY: 1993-2013⁸⁶². MAIN THEMES AND PAPERS

After two years without any publication in FRT, Selleri regularly published two papers each year in 1993, 1994, and 1995. In 1994 he wrote a third paper which will be published in 1996.

In 1993 Selleri published “On the Meaning of Special Relativity if a Fundamental Frame Exists”⁸⁶³, in which he retraced the history of the refusal of the ether, linked to a naïve understanding of Michelson-Morley experiment. Then he analyzed the kinematics and dynamics of SRT, showing how time dilation is an established physical property of nature, thanks to the CERN study of muons in cosmic ray physics; and the Hafele-Keating experiment, who in 1977 found an asymmetry in atomic clocks travelling eastwards and westwards with respect to other ones remained on the ground.

⁸⁶² In over twenty years of activity from 1993 to 2013, Selleri published almost continuously in FRT, as well as FQM. There will also be several other unpublished FRT articles. The lack of publication will be accentuated in the last six years of Selleri’s life, with several difficulties in publishing papers (only 8 papers in 6 years), but with the publication of his final book on Weak Relativity.

⁸⁶³ Selleri F. (1993), “On the Meaning of Special Relativity if a Fundamental Frame Exists”, in *Progress in New Cosmologies*, Arp H. et al. (eds), London/New York: Plenum, pp. 269-284. (Selleri FRT 1993a) in the following.

The physical foundations of the theory of special relativity are critically examined from the point of view that a fundamental inertial frame exists in nature. The idea is implicit in the older works of Lorentz and Fitzgerald, and in the more recent work by Janossy, Builder, Prokhovnik, Bell and others. Even in the event of modifications to the theory, certain elements will have to be retained: the usual relativistic formulae for energy and momentum, the full equivalence of mass and energy, Lorentz contraction and time dilation of moving bodies (Selleri FRT 1993a, pag. 269).

[...] the time dilation [effect] in moving systems is, in fact, one of the best established facts of physics, given the very considerable experience of particle physicists with beams of unstable objects (pions, kaons, hyperons, etc) in high-energy accelerators. The situation is analogous in cosmic ray physics: without time dilation, the muons that decay with a lifetime of $2 \cdot 10^{-6}$ sec and are produced about 20 km above sea level would all disappear within 2 km of the production site, while it is experimentally known that a considerable fraction of them reach sea level.

But time dilation has also been established at the macroscopic level. Hafele and Keating (1972) placed several cesium atomic clocks aboard ordinary commercial around-the-world jet flights. One flight circumnavigated the earth travelling in the eastward direction; the other in the westward direction. The clocks were compared with similar clocks that remained on the ground. Relative to the ground clocks, the clocks that travelled eastward lost 59 ± 10 nanoseconds, while those travelling westward gained 273 ± 7 nanoseconds (GPS). These results are in excellent agreement with the predictions of standard relativity theory, predictions depending both on the flight velocity and on Earth's gravitational field (Selleri FRT 1993a, pag. 283).

The second 1993 paper is “Clock Synchronization and Relativity”⁸⁶⁴. Here started the deep Selleri's work, because he began challenging the synchronization of clocks. he searched some proof for the conventionalism of the relative synchronization introduced by Einstein.

In addition, Selleri started to considerate the so-called Tangherlini transformations as an alternative to LT. The former were introduced in 1961 and stated that, given $\beta = v/c$, and $\gamma = 1 / \sqrt{1 - \beta^2}$, the formulae between a privileged reference frame, $S_0(x_0, t_0)$ and a moving system $S(x, t)$ are:

⁸⁶⁴ Selleri F. (1993), “Clock Synchronization and Relativity”, *Hadronic Journal Suppl.*, 8 (4), pp. 471-483. AND in *Fundamental Questions in Quantum Physics and Relativity*, Selleri F. (ed), Palm Harbor: Hadronic Press, pp. 172-184. (Selleri FRT 1993b) in the following. I used the layout of the latter source.

$$x = \gamma (x_0 - vt_0) \quad t = t_0 / \gamma$$

Four different procedures for synchronizing clocks in inertial frames are discussed within a very general set of transformation laws. We conclude that Mansouri-Sexl obtained only part of the consequences of Einstein's synchronization, but further arguments are found in favour of their thesis that the choice of synchronization is purely conventional. The puzzling relationship between the Lorentz and the Tangherlini transformations is discussed (Selleri FRT 1993b, pag. 172).

[...] All the differences with respect to the SRT are however only due to the different conventions about clock synchronization. Thus we arrive at the paradoxical conclusion that a theory explicitly based on the Tangherlini transformations, and thus on the belief on the existence of a privileged frame, can be practically equivalent to the SRT which negates already in its axioms that such a frame may exist.

Such a puzzling situation will obviously require further study before it can be fully rationalized (Selleri FRT 1993b, pag. 184. My underline).

The Tangherlini's path will be an example of a road taken by Selleri which lead only to partial solutions, though.

The 1993 was also a year full of communication and study for Selleri. Several correspondence seems confirming this trend.

In a letter to Bruno Carbonaro, professor of mathematics at the University of Basilicata, Selleri stated on the one-way velocity of light, and Tangherlini transformations, trusting in better study for their consequences, being them not able to form a group. Anyway, Tangherlini was the first for Selleri in abolishing the spatial term in the time equation.

The one-way speed of light is not measurable according to many "experts" I have heard about in international conferences or spoken to personally. Prokhovnik himself made this statement in Trani [in 1992].

Since from classical physics one gets all special relativity except that a different speed from c in one direction comes out, people do this postulate in my opinion only to hold on to everything [turns lemons into lemonade]. There is really no empirical basis, except that measuring it in one direction is much more difficult and inaccurate. Sorry, is not Roemer's experiment such a measure? The light in fact goes from Jupiter [here to Earth].

I can imagine ways in the laboratory to measure the speed of light in only one direction thanks to cogwheels, but I will limit myself to telling you an experiment that Bartocci told me over the phone. Take a laser, attach it firmly to a rock and record the point of arrival of its light 1 km away. Then see what happens during a year. If the speed of light is composed with the speed of the Earth around the sun, which is 104 times smaller (30 km/s against 300,000 km/s) there must be deviations of 10^{-4} radians, that is 10 cm at that distance! The light dot should describe an ellipse for Lorentz, stay still for Einstein! Easy and beautiful, shouldn't it? But since mala tempora currunt is obvious no one will do it!

I am convinced that Lorentz was right and I already know what the right theory is: it is the one based on Tangherlini's transformations that abolished the term containing x in the transformation of time. So he had Lorentz's contraction and the dilation of time intervals. His, however, was an ether theory, as we can immediately see for example from the fact that his transformations do not form a group. Remember that in my work I demonstrated that every transformation different from Lorentz's one describes an ether theory.

There would be a big work to do again to see what are the electromagnetic consequences of Tangherlini's transformation⁸⁶⁵ (Selleri to Carbonaro, 3 February 1993-FSPA. Selleri underline, my bold).

Perhaps the most significant detail I found was at MBPA in Athens, where I recovered a correspondence between Prokhovnik (which was at the Department of Mathematical Sciences at the University of New South Wales in Australia) and Selleri. Unfortunately, the fax sent by Prokhovnik to Selleri is almost unreadable. I reported the excerpts that I was able to identify.

Selleri still reveals his embryonic state of knowledge of the SRT, related to Lorentz/Fitzgerald contraction, as introduced by Bell, and now clearer thanks to Prokhovnik's contributions, thus confirming the influence of the latter. In addition, he admits how he did not know well Lorentz and Builder paper.

I can state with the greatest possible arbitrariness, and conscious of being able to be disavowed at any time, that Selleri read his influencers papers in this order: (Bell 1976), (Prokhovnik 1967), (Prokhovnik 1979), and (Builder 1958). The right order of Post (1967) and (Cavalleri, Bernasconi 1989) rests unknown. The most relevant thing is that he read all of them, actually. Going back to Selleri's letter to Prokhovnik, the former stated:

⁸⁶⁵ The original text in Italian is in Addendum 3, p. 1052.

Dear Simon,

I have been studying your papers/books and now understand special relativity much better than I did before. I have often been able to redo all needed calculations and obtain the relevant results. There is only one point which I have not fully grasped. I fully agree that gravitational and electromagnetic fields shrink for a moving charge according to the Heaviside formula. I also understand that if the field changes, also the ground state should change accordingly. I understand it qualitatively. But I have been unable to see a rigorous proof and moreover I have seen that Bell has struggling on the same problem, in HOW TO TEACH SPECIAL RELATIVITY, for the case of hydrogen atom. Is it reasonable to conclude that there is only a qualitative argument but not proof that the shrinking should obey precisely the Lorentz/Fitzgerald formula? I ask this because I do not know all the works by Lorentz and by Builder and I am afraid of having missed something important. Would you kindly comment on this point? Thank you. (Selleri to Prokhovnik, 11 June 1993-MBPA. Selleri's CAPITAL, my underline).

Prokhovnik, which suffered a severe illness, answered with a handwritten fax 3 weeks later⁸⁶⁶:

Dear Franco,

as you can see, I am writing from my home, as it has become too hard for me to go to the University. My 'motor neuron disease' almost immobilised me and I use a 3-wheel walking frame to keep my 'going' at home.

Now, you pose some hard and fundamental questions which have been of concern to me also for many years. The basic problem is that we don't understand clearly what we ??? by a "field". Quantum electrodynamics suggests that the Coulomb 'field' (as well as the weak and strong 'forces') are interpretable in terms of photon or ??? exchanges, and that 'graviton' may apply similarly for the understanding of gravitational fields, though I consider this last conjecture as insufficient. Sufficient that I agree with Einstein that changes in a gravitational field (e.g., due to the movement of the source) are transmitted by 'gravitational waves' (a form of energy) [...] the shrink of light; this notion enables me to derive the retarded potential [...] result for a body moving with respect to a fundamental ??? frame – the imagine ??? in which light propagates at speed c in all directions at any point stationary in this frame.

You will find my early thinking about your problem in my 1969 book, *The Logic of Special Relativity* ??? where I conjecture a special form of the retarded potential field and its consequences. This was ?? I had discovered the work of Heaviside and his ??.

⁸⁶⁶ I put three bold question marks (???) where I cannot understand the text, regardless of the length of the missing text.

My more advanced response to the problem appears in the 1985 book, *Light in Einstein Universe*, pp. 77-79.

I deal with the hydrogen atom situation (in a manner already suggested by Larmor in 1897!) and pp. 85 which deal with the more general retarded potential consequence. The latter argument rests heavily the importance of maintaining the spherical ??? of forces linking a particle to neighbouring particles of a system of such. it also rests in Mathematical Interpretation (*Trans. Royal Society*, 181, 675 (1896)) that a spherical particle (or be must assume the shape of a 'Heaviside ellipsoide') in the presence of a retarded potential field his mathematical argument involved the notion the maximum effect of a field on a body must be normal to the surface of the body.

Even so, can we identify ??? respect on a system of such particles even if each of these has an ellipsoidal form? After all it would be (might be?) sufficient for the system to be stable without contraction as long as there is linear symmetry of forces with each particle; that there is then a cancellation of forces in any direction - even when these cancelling forces are different for different directions?

To deal with this argument we must ask what controls the distance between the atoms or molecules of system ??? a body (or fluid) ??? and hence the density of a system?

I am very ??? that this ??? distance depends on a balance between the (repulsing) coulomb force and the (attracting) gravitational ones between any pair of atoms (or molecules) of the ???.

The net coulomb field near the periphery of each atom (or molecule) must be the same (negative and minuscule) for each of these, generating thereby a ??? between any pair of these. However, the gravitation operating between any such pair generates an ??? between them; so that the stable distance between this pair of neighbouring particles is determined by the distance for which the two fields balance precisely.

Now if the coulomb field is due to photon ??? between the nucleus and electrons of an atom as suggested by QED (though Searles argument may also be relevant here), then this ??? and the configuration of the atom (or molecule) contract in the direction of motion exactly as is described on pp. 77-79 of my 1985 book. Note that this ??? implies a ??? for moving system by the factor $(1-u^2/c^2)^{1/2}$ as ??? deducted by Larmor! Note also that the net ??? field on the periphery of the ???, ellipsoidal config ??? remains unchanged (and constant over the whole periphery) as if the retarded potential ??? remembering that the gravitational field associated with any such pair of particles similarly ??? by the retarded potential consequence ??? of a moving system is contracted ???.

There is another associated problem that has ???, the retarded potential ???.

In the context of the existence of a fund. frame I_F which "gravitational (energy) waves" ??? propagate, it follows the effect on a ??? of unit mass due to the gravitational field ??? body of mass M , moving with velocity $\underline{u}$, ??? I_F is given by

$$F = -GM/r^2 * (1-u^2/c^2) / (1- u^2/c^2 \sin^2 \theta)^{1/2}$$

For any direction of r making an angle θ with the direction of $\underline{u}$. For $\theta=0$ we obtain

$$F = -GM/r^2 * (1-u^2/c^2)$$

As surmised in my 1967 book with the contraction consequence deduced by Heavyside, Fitzgerald, Lorentz and Larmor. Builder (1958) simply accepted Lorentz argument and result on this matter.

However, for $\theta=\pi/2$ we obtain

$$F = -GM/r^2 * 1/(1-u^2/c^2)^{1/2},$$

Suggesting, ostensibly by a similar argument as for $\theta=0$ that the expansion of the system in the direction normal ???.

To my knowledge, nobody has explained nor resolved this apparent anomaly, though a ??? have queried it. However, following ??? with my (V.M.) colleague, Trevor Morris, we think there is a simple answer which I ventured to [mention] in my contribution on Trani conference, and which appears as a footnote in the ??? (which you should ??? outlining my contribution).

The answer lies in the derivation and the meaning of the retarded potential formula considers the effect of the field of a ??? on a unit mass particle stationary (see appendices 5 of my 1985 book and also pag. ???).

The corresponding effect on a moving (in I_F) must be less than that on a stationary ??? and for a co-moving (with the field source ???) particle, the effect is diminished by the $(1-u^2/c^2 \sin^2 \theta)^{1/2}$ corresponding to a sort of aberration effect. Hence the net effect on a mass particle commoving with the source body is

$$F = -GM/r^2 * (1-u^2/c^2) / (1-u^2/c^2 \sin^2 \theta)$$

I believe that this is a valid argument and result, and consistent with the theory ??? of the retarded potential field and with the ??? and physical interpretation of special relativity. However, as you can see, there remain many lacunae in our understanding of fields and their effects, providing an ??? of investigation for present and future scholars or PhD students. I hope that I have answered your question and I suppose that this will rise others.

With kindest regards (Prokhovnik to Selleri, Birchgrove, NSW, 1 July 1993-MBPA. My underline and *Italics*).

Although very patchy and likely unsatisfactory to reconstruct, nevertheless I decided to report the above fax for its intrinsic historical value.

In 1993, Selleri experienced the writings and publication of a *Dictionary of Physics* together with Patricchio.

In a letter to Bartocci, Selleri talked about his activity on FRT, revealing his joy and excitement:

[...] But, going back to relativity, I am having a wonderful experience after reading the works of Builder and Prokhovnik: the theory of special relativity is the theory of the ether! That is to say, it is the theory of absolute velocities clearly indicated by the only known solution of the twin paradox. For me it is a reversal of position, and I no longer despair to arrive shortly to indicate where the “breakdown” of the SR will occur. The relativity is a wonderful thing that everyone would have to accept, if the world of physicists were not troubled by that awful crisis that makes them close their doors to every new idea, to spasmodically defend old and outdated ideas.

[...] I am correcting the drafts of an encyclopaedia of physics [...] which contains many historical, philosophical and critical entries. We made it in two physicists from Bari. Then there are 4 introductory prolusions, all of them of alternative physics:

1. Narlikar on astrophysics.
2. Owen on geophysics (expanding Earth).
3. Tyapkin on RS with extensive agreements with the ideas of Builder and Prokhovnik.
4. Me on MQ.

Hopefully the press will notice the novelty of this work, books are always in danger of working for nothing⁸⁶⁷ (Selleri to Bartocci, undated, possibly before 1993, year of publication of Dictionary-FSPA. My underline).

In the minutes of the Physics Degree Course Council at the University of Bari, it is attested that Selleri held the complementary course in Relativistic Physics, practically focused on GRT, for the academic year 1993/94. It sounds curious because Selleri did not basically produced any work on GRT, even though he was interested in starting something in this issue.

In 1994, Selleri published two other papers on the synchronization, pointing out how the choice of an absolute inertial frame could allow a natural synchronization.

In the abstract of the first paper, “Velocity-Symmetrizing Synchronization and Conventional Aspects of Relativity”⁸⁶⁸, Selleri stressed how Mansouri and Sexl developed a program which looked for possible deviations from SRT in the outcome of various experiments:

⁸⁶⁷ The original text in Italian is in Addendum 3, pp. 1052-1053.

⁸⁶⁸ Selleri F. (1994), “Velocity-Symmetrizing Synchronization and Conventional Aspects of Relativity”, in *Waves and Particles in Light and Matter*, Garuccio A., van der Merwe A. (eds), London/New York: Plenum, pp. 439-446. (Selleri FRT 1994a) in the following.

We show that a very natural synchronization procedure is expressed by the requirement that the velocity of S_0 relative to S be equal and opposite to that of S relative to S_0 , if S_0 is the “absolute” inertial frame and S any other frame. This new procedure is compatible with Einstein synchronization and with slow-transport synchronization only within the special theory of relativity. The practical equivalence of the Lorentz and Tangherlini transformations is discussed (Selleri FRT 1994a, pag. 439).

[...] Since the difference between Tangherlini and Lorentz is conventional, Mansouri and Sexl argued that the two theories are completely equivalent as far as the explanation of experimental results is concerned. This conclusion is truly extraordinary, since it empties the principle of relativity (and its negation) of any physical role. A theory explicitly based on ether, in which a privileged frame exists and is recognized as such by all observers, leads to all the well-known predictions of special relativity! The main argument of Mansouri and Sexl in favour of their thesis is the clock paradox (Selleri FRT 1994a, pag. 443. My underline).

The second 1994 paper, “Theories Equivalent to Special Relativity”⁸⁶⁹ was the beginning of a stronger activity by Selleri. He began promoting it as an example to start from. It belongs to the conference organized by Selleri in Olympia in 1993. According to Barone, the co-organizer, in that venue Selleri spoke for the first time about his work in FRT. It sounds correct because, even if in the proceedings of PIRT 1990 there is the (Selleri FRT1990a), from the letter I showed sent by Selleri to Duffy, the former talked about EPR. In this (Selleri FRT 1994b), the author stated:

The purpose of this paper is twofold: (1) to discuss the basis of the Lorentz transformations showing that the invariance of the velocity of light has in them a role even more important than usually believed, and (2) to find the complete set of theories empirically equivalent to the special theory of relativity (STR) under the assumption that the one-way velocity of light is not measurable. In particular, it will be shown that any modification of the coefficients of the Lorentz transformations, however small, gives rise to an ether theory, in the sense that the modified theory necessarily predicts the existence of a privileged frame that in principle can be detected experimentally. Therefore all the theories equivalent to STR but based on different transformation laws, must necessarily negate the validity of the relativity principle.

⁸⁶⁹ Selleri F. (1994), “Theories Equivalent to Special Relativity”, in *Frontiers of Fundamental Physics*, Barone M., Selleri F. (eds), London/New York: Plenum, pp. 181-192. (Selleri FRT 1994b) in the following.

We will come thus to the surprising conclusion that if the one-way velocity of light is not measurable, the content of the relativity principle is entirely conventional, since it can be affirmed or negated without any practical change in the predictive power of the theory (Selleri FRT 1994b, pag. 181).

The two guiding principles to start from are that empty space is homogeneous, that is, all points have the same physical properties; and that also time is homogeneous, that is, all properties of space remain the same with passing time.

Selleri started to create general space-time transformations, with general functions of the space-time coordinates in the formulae to link two inertial systems S and S' , where in the former any clocks can be synchronized according to Einstein's procedure. The most important situation lies in the transformation of time, because it is known that SRT put the space in the time equation. The parameter within this equation is a synchronization function. Then Selleri made some simplifications on the axes. Working on the one-way velocity of light and the invariance of the (two-ways) velocity of light, Selleri reach four equations for time and the 3D-space with only one unknown parameter.

In a letter sent to Ulrich Schröder, Selleri stated that the above paper, i.e. (Selleri FRT 1994b):

[...] is an introductory paper in which all calculations are performed in detail and never repeated again. If interested you should probably start from this one. The main result is the inverse velocity of light relative to a moving inertial frame. It is a long equation, but essential and nobody had obtained it before, not even Mansouri and Sexl (Selleri to Ulrich Schröder, 27 August 1996-FSPA. My underline).

In 1995, the year after, Selleri published two papers. In both of them he analyzed the opportunity to construct a new set of space-time transformations between inertial systems. Starting from the absolute synchronization (the real turning point, actually), he managed to create different transformations with respect LT, which are not compatible with the principle of relativity.

The first paper is “Inertial Systems and the Transformations of Space and Time”⁸⁷⁰. In the abstract, Selleri set two empirically statements as a framework to create new transformations. The other assumptions he made are those of a homogeneous space and time:

The two-way velocity of light is c in all inertial systems.

Time dilation effects take place with the usual relativistic factor.

These two empirically based statements are used for constructing a set **T** of space-time transformation laws between inertial systems that can explain all available experimental evidence. Any two transformations belonging to **T** differ only for a convention about clock synchronization. All elements of **T** violate the relativity principle, the Lorentz transformation excepted.

The simplest element transforms time independently of space variables. It is based on absolute synchronization and can be called “inertial transformation”.

A new interesting picture of physical phenomena is thus developed. When accelerations come into play the equivalence between elements of **T** is broken and the inertial transformation emerges as closest to physical reality (Selleri FRT 1995a, pag. 342. *Italics and underlining in the original text*).

Selleri reaches this new set of transformations:

$$\begin{aligned}x &= (x_0 - \beta ct_0) (1 - \beta^2)^{1/2} \\y &= y_0 ; z = z_0 \\t &= (1 - \beta^2)^{1/2} t_0 + d_1 (x_0 - \beta ct_0) + d_2 (y_0 + z_0).\end{aligned}$$

The formula for the inverse one-way velocity of light is:

$$1/\hat{c}(\theta, \varphi) = 1/c + [\beta/c + d_1(1 - \beta^2)^{1/2}] \cos \theta + d_2 \sin \theta (\cos \varphi + \sin \varphi).$$

Then he stated that:

They represent the complete set **T** of theories equivalent to STR: if d_1 and d_2 are varied, different elements of **T** are obtained, which are all equivalent to the Lorentz transformation as far as the explanation of experimental results is concerned. Different values of d_1 and d_2 are obtained from different clock synchronization conventions. In all cases except STR such theories negate the relativity principle.

⁸⁷⁰ Selleri F. (1995), “Inertial Systems and the Transformations of Space and Time”, *Physics Essays*, 8 (3), pp. 342-349. (Selleri FRT 1995a) in the following.

We thus come to a surprising conclusion: since the one-way velocity of light is not measurable independently of clock synchronization procedures, the content of relativity principle is essentially conventional, since it can be affirmed or negated without any practical change in the predictive power of the theory. The condition $d_1=d_2=0$ arises from the so-called absolute synchronization, gives rise to a transformation different from the Lorentz transformation, and is therefore not compatible with the relativity principle. It is nevertheless particularly simple, elegant and easily deducible:

$$\begin{aligned}x &= (x_0 - \beta c t_0) (1 - \beta^2)^{1/2} \\y &= y_0 \quad z = z_0 \\t &= (1 - \beta^2)^{1/2} t_0 \\1/\hat{c}(\theta) &= (1 + \beta \cos \theta)/c\end{aligned}$$

(Selleri FRT 1995a, pag. 345).

In the second paper, “Space, Time and their Transformations”⁸⁷¹ which takes up and develops the first one, Selleri builds what will be the future Inertial Transformations (IT) and analyzes their consequences. The new set of transformations is defined by Selleri as either “Absolute transformations” or “Inertial transformations”. He will lean towards the latter:

We construct a set **T** of space-time transformations between inertial systems which are completely equivalent for explaining the experimental evidence, by starting from two empirically based assumptions: (1)The two-way velocity of light is c in all inertial systems and in all directions; (2)Time dilation effects take place with the usual relativistic factor. The Lorentz transformation is an element of **T** and any two elements of **T** differ only as to a convention regarding clock synchronisation. The simplest choice is to transform time independently of space co-ordinates (absolute synchronisation) and can be called “inertial transformation”. When accelerations are considered the equivalence is broken and the inertial transformation emerges as closest to physical reality (Selleri FRT 1995b, pag. 25).

Their consequences include an absolute simultaneity, an absolute time dilation and an absolute length contraction.

⁸⁷¹ Selleri F. (1995), “Space, Time and their Transformations”, in *Special Issue on Space, Time, Motion-Theory and Experiment, Chinese Journal of Systems, Engineering and Electronics*, 6 (4), pp. 25-44. (Selleri FRT 1995b) in the following.

In a letter to Dipankar Home, Selleri explained the difference between the Tangherlini transformations and his absolute transformations, which later were christened as IT:

$$\begin{aligned}x &= \frac{x_0 - \beta c t_0}{\sqrt{1 - \beta^2}} \\y &= y_0 \\z &= z_0 \\t &= \sqrt{1 - \beta^2} t_0\end{aligned}\quad (1)$$

$$\begin{aligned}x_0 &= \sqrt{1 - \beta^2} \left[x + \frac{\beta c}{1 - \beta^2} t \right] \\y_0 &= y \\z_0 &= z \\t_0 &= \frac{1}{\sqrt{1 - \beta^2}} t\end{aligned}\quad (3)$$

$$\begin{aligned}x' &= \frac{\sqrt{1 - \beta^2}}{\sqrt{1 - \beta'^2}} \left[x - \frac{(\beta' - \beta)c}{1 - \beta^2} t \right] \\y' &= y \\z' &= z \\t' &= \frac{\sqrt{1 - \beta'^2}}{\sqrt{1 - \beta^2}} t\end{aligned}\quad (5)$$

A transformation having the form (1) has once been written down by Tangherlini while (3) and (5) do not exist in the scientific literature: a good name for (1)-(3)-(5) is “absolute transformations”, not only because they are consequences of absolute synchronization, but also because they lead to absolute time dilation and absolute length contraction. The absolute transformations can be shown to form a group whose invariants are those linear combinations of the space-time variables of the moving frames which equal position and time of the privileged frame S_0 . Instead, **the Tangherlini transformations (1) do not form a group. This is the reason way it is important to distinguish “Tangherlini” and “absolute”** (Selleri to D. Home, 29 May 1995-FSPA. Selleri’s underline, my bold).

Selleri will experience the fact that neither Tangherlini transformations nor his absolute (later inertial) transformations form a group.

In June 1995, in a letter sent to Roland Dishington, Selleri was deciding for a new name, maybe “inertial” for his transformations. Until May 1995 he had used the term “absolute”. And he also speaks about Sagnac effect, a relevant experimental support for his new IT and the future theory of Weak Relativity:

The Sagnac effect has never been properly explained. All books derive the usual formula in the laboratory system from which the disk is seen rotating, but forget to do anything on the rotating disk.

The reason is simple: if they apply the Lorentz transformations and use a constant velocity of light they get no effect, thus contradicting what they obtained in the laboratory. And general relativity cannot help them because it gives symmetrical corrections for the two beams of light traveling in opposite directions on the disk. The same job can be done with my “absolute transformations” (bad name, I am looking for a better one; perhaps “inertial transformations”) for which the velocity of light is not constant anymore, and then of course a perfect agreement is found with the result of the laboratory. So I can solve an old, hidden difficulty of standard theory (when they have a difficulty they hide it; when some experimental results disagree with their predictions, they forget about them). But there is more. The previously described calculations do not remain the same if the laboratory is moving through ether. A new calculation shows that the effect depends not only on the disk rotational velocity, but also on the earth absolute velocity. The effect is probably too small to be measurable (10^{-10}) but it exists, and we find so a clear-cut way of observing absolute motion! (Selleri to Dishington, 29 June 1995-FSPA. My underline).

Selleri introduced his research on Sagnac effect, explaining it in terms of the new set of transformations.

In a letter to Eftichios Bitsakis, of August 1995, Selleri established for the first time the better statement of “inertial” transformations, more than “absolute” transformations:

I have almost finished two important papers on my reformulation of special relativity: as you know, I do not use Lorentz transformations, but the “inertial transformations”, which before I called “absolute transformations” (I do not like that definition anymore because it could become misleading) (Selleri to Bitsakis, 23 August 1995-FSPA. My underline)

The 1996 sounds as the year of the enhancement of Selleri’s activity in FRT. The two papers written in this year will be used as a references in many others by Selleri himself and will be also cited by other scholars, together with (Selleri 1995b).

They are “Noninvariant one-way velocity of light”⁸⁷²; and “Noninvariant one-way velocity of light and particle collisions”⁸⁷³.

In a letter to Costa de Beauregard written in 2003, Selleri traces back in practice the beginning of his real activity in the field to the above 1996 papers, i.e. (Selleri FRT 1995a), (Selleri FRT 1996a), and (Selleri FRT 1996b):

Dear Olivier,

As you can imagine this research in the foundations of relativistic physics is a fascinating and exciting activity, more so than in quantum physics where the truth is more hidden and more difficult to discover.

I am sending you, in this envelope, the 3 papers from which everything really started (R9, R10, R11). (Selleri to O. Costa de Beauregard, 17 December 2003-FSPA. My underline).

(R9, R10, R11) represents the classification of Selleri’s papers in FRT, corresponding to the above three papers, respectively.

In the (Selleri FRT 1996a), Selleri stated that:

After discussing in the first five sections the meaning and the difficulties of the principle of relativity we present a new set of space-time transformations between inertial systems (“inertial” transformations), based on three assumptions: (1) The two-way velocity of light is c in all inertial systems and in all directions; (2) Time dilation effects take place with the usual relativistic factor; (3) Clocks are synchronized in the way chosen by nature itself, e.g., in the Sagnac effect. We show that our new transformation laws can explain the available experimental evidence in spite of the implied noninvariance of the one-way velocity of light (Selleri FRT 1996a, pag. 641).

The one-way velocity of light in moving inertial systems (e.g., on Earth) has never been measure accurately. Often people stress that in order to measure it one needs synchronized clocks, but that in order to synchronize clocks one must know the one-way velocity of light, so that the logical situation becomes circular.

⁸⁷² Selleri F. (1996), “Noninvariant one-way velocity of light”, *Foundations of Physics*, 26 (5), pp. 641-664. (Selleri FRT 1996a) in the following.

⁸⁷³ Selleri F. (1996), “Noninvariant one-way velocity of light and particle collisions”, *Foundations of Physics Letters*, 9 (1), pp. 43-60. (Selleri FRT 1996b) in the following. Actually, (Selleri FRT 1996a) was released after (Selleri FRT 1996b). But between the references of the latter appeared the former, indicating that it was written before. On the other hand, Selleri decided to classify the former first.

All the laboratory experiments (from Fizeau, Foucault, Michelson, to the recent ones) measured instead the *two-way* velocity of light. Since such measurements are obviously possible with just one clock, the synchronization problem did not arise. [Only the famous experiments on the occultations of Jupiter's satellites (Römer 1676) and on the aberration of light (Bradley 1728) were one-way measurements, even if not very precise ones].

When Einstein formulated his theory of relativity he *postulated* that the velocity of light has always the same value c in all directions and relative to all inertial frames.

There is now some agreement among physicists at least on the conclusion that the constancy of the one-way velocity of light is a useful *convention* and that it must not be considered as something dictated by objective properties of the natural world (Selleri FRT 1996a, pagg. 641-642. *Italics* in the original text).

The history of the synchronization sounds very long, starting with Poincaré and continuing with Einstein, Reichenbach, and Jammer. Einstein in 1916 declared the conventional choice of the synchronization: “that light requires the same time to traverse the path AM [...] as the path BM [M being the midpoint of the line AB] is in reality *neither a supposition nor a hypothesis* about the physical nature of light but a *stipulation* which I can make of my own free will in order to achieve a definition of simultaneity” (in Selleri FRT 1996a, pag. 642. *Italics* in the original text; and in (Nutricati 1998), pag. 27). Then Selleri cited Reichenbach and Jammer, pointing out their analysis on the conventionality of synchronization:

Reichenbach considered the following situation: In an inertial system S , a flash of light leaves point A at time t_1 , is reflected back in point B at time t_2 , and arrives again in A at time t_3 .

The problem is how to synchronize the clock near point B with the clock near point A. In the theory of relativity, it is *assumed* that the one-way velocity of light has the same value [in the two path], [...] and then:

$$t_2 = t_1 + \frac{1}{2} (t_3 - t_1)$$

Reichenbach commented:

“This definition is essential for the special theory of relativity, but it is not epistemologically necessary. If we were to follow an arbitrary rule restricted only to the form

$$t_2 = t_1 + \varepsilon (t_3 - t_1) \quad 0 < \varepsilon < 1$$

it would likewise be adequate and could not be called false.

If the special theory of relativity prefers the first definition, i.e., sets ε equal to $\frac{1}{2}$, it does so on the grounds that this definition leads to simpler relations” (Selleri FRT 1996a, pag. 642. *Italics* in the original text).

Jammer in 1979 was sharper than Reichenbach, still opening to any value of the parameter ε :

“One of the most fundamental ideas underlying the conceptual edifice of relativity, as repeatedly stressed by Hans Reichenbach and Adolf Grünbaum, is the conventionality ingredience of intrasystemic distant simultaneity. [...] the ‘thesis of the conventionality of intrasystemic distant simultaneity’, or briefly, ‘conventionality thesis’ consists in the statement that the numerical value of ε need not necessarily be $\frac{1}{2}$, but may be any number in the open interval between 0 and 1, i.e., $0 < \varepsilon < 1$, without ever leading to any conflict with experience” (cited in Selleri FRT 1996a, pag. 642).

Given that Einstein synchronization method is not only compatible with the principle of relativity, but is rather its most important and direct consequence, and given that it is a conventional choice, Selleri stated that other theories have the same “right of citizenship” in the framework of relativity events.

[...] if a scientific statement is true it cannot be refused, but if it is only conventional it becomes immediately interesting to look for alternatives, which will be based on conventions different from the usually accepted one. [...] if the constancy of the velocity of light is conventional it is obviously legitimate to study theories in which this constancy is not true. But by so doing, the principle of relativity will also be violated. This can only mean that the relativity principle itself is only a useful human convention and not a fact of nature (a conventional truth cannot be a necessary consequence of an objective truth). These considerations seem to weaken the *necessity* to accept the strong formulation of the relativity principle: it is possible and interesting to explore alternative paths (Selleri FRT 1996a, pagg. 643-644. *Italics* in the original text).

Selleri started to argue on the several relationship between the PR and physical reality, with the presence of iperdeterminism; and between the PR with the equivalence principle.

Then he started to examine the parameter, which he called e_1 , in the equation of time

$$t = \sqrt{1-\beta^2} t_0 + e_1 (x_0 - \beta c t_0)$$

It represents the presence of the spatial component in the equation of time, still pursuing an asymmetry in the transformations between two systems.

The parameter e_I could assume different values from diverse clock-synchronization *conventions*.

Applying the absolute synchronization, for Selleri the simplest possibility is given by $e_I=0$, that is, having a space-independent transformation of time. In his book on WR Selleri will emphasize how it could be the only possible value in order to explain ten situations in which RT fails.

Taking into account some accelerated system, Selleri gets in touch with the rotating disk and the Sagnac effect (I recalled the influence on Selleri by E. J. Post), one of his cornerstone in the development of WR. In this paper Selleri gave even an explanation in the Laboratory and the Disk.

[...] the Sagnac effect is well known to take place in such conditions: a monochromatic light source placed on the disk emits two coherent beams of light in opposite directions. These travel along a circumference concentric with the disk, until they reunite in a point A and interfere, after a 2π propagation. The result can be achieved by forcing the light to propagate tangentially to the internal surface of a cylindrical mirror. The positioning of the interference figure depends on the disk rotational velocity (Sagnac effect). Most textbooks deduce the Sagnac formula [...] in the laboratory, but say nothing about the description of the phenomenon given by an observer placed on the rotating platform, while our approach based on the inertial transformations gives the right answer (Selleri FRT 1996a, pag. 652. *Italics* in the original text. My underline).

According to Selleri and this latter paper, a privileged inertial frame is thus seen to exist.

One of the problems Selleri found in his path and described in the above paper was the opportunity by IT to form a group. Selleri had to give a negative answer on this issue.

Concerning this problem, Selleri wrote to Prof. Philippe Guéret, with whom he had written a paper in 1988:

[...] The fact that you want to work on group theory interests me a lot: there is my group of inertial transformations to study. See § 8 of NONINVARIANT ONE-WAY VELOCITY OF LIGHT, which is entitled: “The inertial transformations form a group” [The ‘quasigroup’ of inertial transformations, actually].

What are the invariants of this group? If you see it by sight, please tell me now. However, the idea should be developed, what do you think?⁸⁷⁴ (Selleri to Guéret, 14 February 1996-FSPA. Selleri’s capital)

In another letter to George Galeczki, Selleri debates on the lackness of group properties showed by the inertial transformations:

Dear George,

You and I do not seem to agree about group properties: the inertial transformations do not form a group, but this is not a problem. Groups are like the snow: it is nice to see it, but you can live perfectly happily without (people living in Madeira are an example). If, eventually, you feel you would like to see some snow you go in a country where it is rich and abundant (Iceland, for example). If you feel you would like to see groups at work in physics take the time evolution operators with a time independent Hamiltonian. **Nobody worries that the space-time transformations of general relativity do not form a group. Why should I worry if my inertial transformations do not form a group if everything is nice and consistent?** (Selleri to Galeczki, 6 April 1998-FSPA. Underline in the original text, my **bold**).

Anyway, with respect to (Selleri FRT 1996a), in a letter to Garuccio, Selleri stated how it was, at that time, his best paper on FRT:

Noninvariant one-way velocity of light **is my most significant relativistic work** with criticism of the principle of strong relativity (the one that necessarily leads to Lorentz's transformations) and with the development of inertial transformations. Paragraph 6 is dedicated to Sagnac and in the most important formula [concerns precisely this effect seen from the disc] only if $e_1 = 0$ is the right result, all the other theories, relativity included, give the wrong result⁸⁷⁵ (Selleri to Garuccio, 26 Agosto 1996-FSPA. Selleri’s underline, my **bold**)

⁸⁷⁴ [...] Il fatto che tu voglia lavorare alla teoria dei gruppi mi interessa molto: c’è il mio gruppo delle trasformazioni inerziali da studiare. Vedi il § 8 del lavoro NONINVARIANT ONE-WAY VELOCITY OF LIGHT, che è intitolato: “The inertial transformations form a group” [“The ‘quasigroup’ of inertial transformations”]. Quali sono gli invarianti di questo gruppo? Se lo vedi a occhio ti prego di dirmelo subito. Comunque occorrerebbe sviluppare l’idea, che ne dici? (Selleri To Guéret, 14 February 1996-FSPA).

⁸⁷⁵ “Noninvariant one-way velocity of light **è il mio lavoro relativistico più importante** con le critiche al principio di relatività forte (quello che porta necessariamente alle trasformazioni di Lorentz) e con lo sviluppo

In the second paper of 1996, that is, (Selleri FRT 1996b), Selleri stated that:

Recently published space-time transformations between inertial systems (different from the Lorentz transformations) are reviewed. Energy and momentum are defined consistently with these new transformation laws. Formally they equal the usual relativistic expressions only in the privileged frame, but numerically they must do so in all inertial frames. All the precise experimental data concerning thresholds for inelastic processes, particle masses, and so on, can thus be explained also within this new theoretical framework (Selleri FRT 1996b, pag. 43).

In (Selleri FRT 1996b) he argued that the one-way velocity has never been measured accurately, because doing this would required to synchronize clocks. But in order to synchronize them, the one-way velocity is requested. So, the logical situation becomes circular, it sounds as a logical loop. Except for Römer and Bradley experiments, the others measured the two-way velocity of light. While Einstein was building the SRT, he postulated the constant value of c in all directions and relative to all inertial frames. Many authors believes that the one-way velocity of light is not measurable as a matter of principle. But historically, Selleri already showed how several physicists (Mansouri & Sexl, Reichenbach, Jammer, etc.) stressed that “the constancy of the one-way velocity of light is a useful convention an that it is not necessarily dictated by objective properties of the natural world” (Selleri 1996b, pag. 44).

Concerning 1997, Selleri wrote a paper, “Noninvariant Velocity of Light and Locally Equivalent Reference Frames”⁸⁷⁶, in which he points out how only velocities matter in the case of length contraction and time dilation events.

delle trasformazioni inerziali. Il paragrafo 6 è dedicato a Sagnac e nella formula più importante [riguarda proprio tale effetto visto dal disco] solo se $e_1 = 0$ si trova il risultato giusto, tutte le altre teorie, relatività compresa, danno il risultato sbagliato” (Selleri to Garuccio, 26 Agosto 1996-FSPA. Selleri’s underline, my bold)

⁸⁷⁶ Selleri F. (1997), “Noninvariant Velocity of Light and Locally Equivalent Reference Frames” in *New Developments on Fundamental Problems in Quantum Physics*, Ferrero M., van der Merwe A. (eds), Dordrecht: Kluwer Academic Publishers, pp. 381-394. (Selleri FRT 1997a) in the following.

The large lifetime increase of muons observed in storage ring experiments is independent of muon acceleration. We assume this to be a manifestation of a general principle: **only velocities matter for length contraction and time dilation phenomena**. Therefore local measurements of space, time and velocity in an accelerated reference frame must give the same results as those in a “locally equivalent” inertial frame having the same instantaneous velocity.

This principle is used for deducing the velocity of light both along the rim of a rotating platform and relative to inertial reference frames, and leads to noninvariant values. A set of transformations of space and time different from the Lorentz transformations can be deduced [They do not form a group but multiply in a selective way, similar to that of time evolution operators] (Selleri FRT 1997a, pag. 381. Underline in the original text).

Selleri also published a paper with F. Goy, “Time on a rotating platform”⁸⁷⁷, with which he starts a deep analysis of clock synchronization on a rotating platform, which is conventional when only inertial frames are involved. In the accelerated frames the one-way velocity of light is nonstandard, as in the case of the synchronizing clocks of GPS. In the abstract they stated:

Traditional clock synchronisation on a rotating platform is shown to be incompatible with the experimentally established time transformation. The latter transformation leads directly to solve this problem through nonisotropical one-way speed of light. The conventionality of some features of relativity theory allows full agreement with existing experimental evidence (Goy, Selleri FRT 1997b, pag. 17).

[...] one is free to choose either the standard synchronization, or a nonstandard one leading to a noninvariant one-way velocity of light. Either choice will allow full agreement with experimental facts. However we have shown that the conventionality of the synchronization procedure is not preserved in accelerated systems, and that a theory free of logical contradictions must choose a one-way velocity of light which is nonstandard when measured in the accelerated frame.

By the way, this is exactly what is already done in practice by physicists synchronising clocks around the earth by means of light signals (Goy, Selleri FRT 1997b, pag. 28).

⁸⁷⁷ Goy F., Selleri F. (1997), “Time on a rotating platform”, *Foundations of Physics Letters*, 10 (1), pp. 17-29. (Goy, Selleri FRT 1997b) in the following.

In “Noninvariant One-Way speed of Light and Locally Equivalent Reference Frames”⁸⁷⁸, Selleri put his sight in the analysis on the rotating disk. The new step is that, starting by the conventional role of the clocks synchronization, which gave an opportunity to have several theories, currently Selleri shows how nature solves the issue: “*it is not true that the synchronization procedure can be chosen freely because Einstein’s convention leads to an unacceptable discontinuity in the physical theory*” (Selleri 1997c, pag. 82, *My italics*). In the abstract, Selleri stated that:

We consider an isotropical inertial reference frame (“stationary”) and in it a uniformly rotating circular platform of radius R . The velocity of light relative to the rim of the platform is calculated and found to have values necessarily different from c . This remains the same if R is increased but the peripheral velocity is kept constant. Since by so doing any small piece of the circumference can be considered better and better at rest in a (“moving”) inertial system, the velocity of light relative to this system can be deduced. Noninvariant values are obtained and shown to coincide with the predictions of our recently published “inertial transformations” (Selleri FRT 1997c, pag. 73).

Concerning (Selleri FRT 1997c), in a letter to Dr. Bahram Mashhoon, Selleri describes the discontinuity between accelerated and inertial frames, and how this discontinuity could disprove the Einstein’s conventional choice of synchronization:

The [...] paper contains a striking result which is now also debated by other authors: inertial frames are discontinuously described from accelerated frames, no matter how small is their acceleration, if the Lorentz transformations are adopted for the former. What I call the “inertial transformations” (they could be called “generalized Tangherlini” transformations as well) are thus shown to be a logical necessity of theoretical physics. I hope you will find time to think about these matters, especially about the discontinuity problem which opens up exciting perspectives (Selleri to Bahram Mashhoon, 6 March 1998-FSPA. *My underline*).

⁸⁷⁸ Selleri F. (1997), “Noninvariant One-Way speed of Light and Locally Equivalent Reference Frames”, *Foundations of Physics Letters*, 10 (1), pp. 73-83. (Selleri FRT 1997c) in the following.

Again in 1998 Selleri recognizes the remarkable contribution by Tangherlini. In the same year, Selleri found the chance to publish a review paper, too, "The relativity principle and the nature of time"⁸⁷⁹. In a letter to prof. Roland Dishington, he stated that this paper "[...] is a qualitative introduction to my line of thought, and it becomes very explicit in the last section" (Selleri to Dishington, 17 November 1997-FSPA).

In the abstract, Selleri recalls the historical ideas on the nature of time, the experiments on the slowing down of moving clocks, the twin paradox, the Lorentz reformulation of SRT and so on:

Old and recent ideas concerning the nature of time are reviewed, starting from Mach's refusal of Newton's absolute time. Many experiments show that the slowing down of moving clocks is a real phenomenon. Such must then also be the so-called "twin paradox", owing the name to its evident incompatibility with the philosophy of relativism (not to be confused with the theory of relativity). The Lorentz reformulation of special relativity started by postulating physical effects of the ether, but accepted Einstein's clock synchronisation. Only because of this Lorentz could not understand the advantages of an easily deducible different theory. As stressed by Popper, one of the main problems of the usual approach is the introduction of a super deterministic universe. Recent results, obtained by the author, show that a theory is possible, based on relative time but on absolute simultaneity, in which the conceptual difficulties of relativity are avoided (Selleri FRT 1997d, pag. 1527).

Concerning the twin paradox, in (Nutricati 1998) Selleri challenged several critics on the role of the acceleration in the asymmetrical ageing of the twins. He affirmed that there are two main strands in which the researches on the paradox of twins are inserted:

- those who acknowledge the speed of the travelling twin as the cause of the slowdown in its biological processes;
 - those who instead try to attribute the same slowdown to the accelerations suffered at departure, at the moment of the inversion of the direction of flight and on arrival.
- Obviously the authors of the second strand try to save the perfect symmetry of straight and uniform movements required by the principle of special relativity.

⁸⁷⁹ Selleri F. (1997), "The relativity principle and the nature of time", *Foundations of Physics*, 27 (11), pp. 1527-1548. (Selleri FRT 1997d) in the following.

However, their position is indefensible, as was demonstrated by Builder [in 1958] with a simple argument: in physics one can recognize the cause of a phenomenon by varying it and verifying the existence of corresponding variations in the effect. In the case of our twins, if the one travelling doubles the length of the paths of uniform rectilinear motion leaving the acceleration and braking processes unchanged, it finds its age difference doubled from the one left on the ground.

Nothing changes, on the other hand, if you change the acceleration while leaving the speed intact.

Therefore, speed and not acceleration is responsible for asymmetric ageing⁸⁸⁰ (Nutricati 1998, pagg. 23-24. My underline).

In 1998 Selleri released “On the existence of a physical and mathematical discontinuity in relativistic theory”⁸⁸¹, in which:

An isotropic inertial reference frame (“stationary”) is considered and in it a uniformly rotating circular disk of radius R . The velocity of light relative to the rim of the disk is calculated under very general assumptions and found to satisfy. This remains the same if R is increased but the peripheral velocity of the disk is kept constant. Since by so doing any small part of the circumference can be considered (for a short time) better and better at rest in a (“moving”) inertial system, **there is a discontinuity between accelerated reference frames with arbitrarily small acceleration and inertial frames, if** the velocity of light is assumed to be c in the latter. Elimination of the discontinuity is shown to imply for inertial systems a velocity of light, necessarily equal to that obtained from recently published “inertial transformations” (Selleri FRT 1998a, pag. 69. Selleri’s underline, my **bold**).

In the above letter to Dishington, Selleri claimed:

Thanks a lot for your kind letter, receiving it was a real boost! The fact is that I have written several papers following a new line of thought, but it seems that people are either shocked or uninterested. The usual silence surrounds what I do.

[...] ON THE EXISTENCE OF A PHYSICAL AND MATHEMATICAL DISCONTINUITY IN RELATIVISTIC THEORY, recently published with a different title on FP Letters. The essence of it is the figure in the last page. It shows that the postulate of the invariance of the velocity of light is untenable (Selleri to Dishington, 17 November 1997-FSPA. Selleri’s capital, my underline).

⁸⁸⁰ The original Italian text is in Addendum 3, p. 1053.

⁸⁸¹ Selleri F. (1998), “On the existence of a physical and mathematical discontinuity in relativistic theory”, in *Open Questions in Relativistic Physics*, Selleri F. (ed), Montreal: Apeiron, pp. 69-80. AND in *Relativistic physics and some of its applications, International conference, Athens, 25-28/6/1997*. (Selleri FRT 1998a) in the following.

In the 2000s, Selleri published several papers, without succeeding in publishing many others. In “Space and Time are better than Spacetime I and II”⁸⁸², he showed how several events, like length contraction, clock retardation, Michelson type experiments, aberration, occultation’s of Jupiter satellites, and radar ranging of planets, are insensitive to the “synchronization parameter” e_I , the free coefficient of x in the transformation of time. SRT is recovered for a peculiar value of e_I . Others events will be explainable only with the precise value of $e_I = 0$, which will be also the only possible value for accelerated systems, i.e., the Sagnac effect on a rotating disk.

In a note on FSPA, Selleri stated that in his (Selleri FRT1997a) the parameter $e_I = 0$ is a necessary consequence, albeit not general yet.

Selleri made several short comments on his papers. In a note in FSPA how (Selleri FRT 1995a) is the paper in which IT appeared for the first time, as well as the inverse and general IT; so, in (Selleri FRT 1997a) the parameter $e_I = 0$ is a necessary consequence, albeit not general yet.

Selleri emphasized how another paper, “On a logical problem in the theory of relativity and its overcoming”⁸⁸³, is in his opinion the best paper on discontinuity for rotating disk, with a proof of IT and some answers to the critics.

For the sake of thoroughness, I would like to mention here that Selleri also published several books on FRT before his last one on WR⁸⁸⁴, and a conference in Athens, originally scheduled in Olympia, RELATIVISTIC PHYSICS AND SOME OF ITS APPLICATIONS, which experienced a very

⁸⁸² Selleri F. (2001), “Space and Time should be Preferred to Spacetime - I”, in *Gravitation, Electromagnetism and Cosmology: Toward a New Synthesis*, Rudnicki K. (ed), Montreal: Apeiron, pp. 57-72. also known as Selleri F. (2001), “Space and Time are better than Spacetime - I”, in *Redshift and Gravitation in a Relativistic Universe*, Rudnicki K. (ed), Montreal: Apeiron, pp. 57-72. Selleri F. (2001), “Space and Time should be Preferred to Spacetime - II”, in *Gravitation, Electromagnetism and Cosmology: Toward a New Synthesis*, Rudnicki K. (ed), Montreal: Apeiron, pp. 73-86. also known as Selleri F. (2001), “Space and Time are better than Spacetime - II”, in *Redshift and Gravitation in a Relativistic Universe*, Rudnicki K. (ed), Montreal: Apeiron, pp. 73-86.

⁸⁸³ Selleri F. (1998), “On a Logical Problem in the Theory of Relativity and Its Overcoming”, in *Physical Interpretation of Relativity Theory*, Duffy M.C. (ed), London: Imperial College, pp. 375-386.

⁸⁸⁴ Selleri F. (2003), *Lezioni di relatività da Einstein all'etere di Lorentz*, Bari: Progedit; Selleri F. (ed), (1998), *Open Questions in Relativistic Physics*, Montreal: Apeiron; Selleri F. (ed) (2002), *La natura del tempo. Propagazioni super-luminali-Paradosso dei gemelli-Teletrasporto*, Bari: Edizioni Dedalo. ISBN: 88-220-6251-5.

painful road, being at risk of cancellation for a long time. In MBPA I found all letters with European Union for finding funds, letters to the invited speakers and so on.

Concerning the Italian book written by Selleri, “Lezioni di relatività da Einstein all’etere di Lorentz”, published in 2003, I mention here some excerpts by the letter he sent to some scholars in order to show a sort of overview on his research and his thought.

Selleri, always recognizing the intrinsic outstanding role of RT, was against its absolute and eternal correctness, still revealing some weak point. He introduced in the book a deep analysis of some paradoxes inherent in the RT, still submitting in the book the superluminal signals (SSL) and the Big Bang. The whole original letter in Italian is in Addendum 3, pp. 1053-1055.

LEZIONI DI RELATIVITÀ DA EINSTEIN ALL’ETERE DI LORENTZ

It is a heretical book, if “heresy” is opposition to established dogmas. It is instead orthodox, if “orthodoxy” is a search for truth as for G. Bruno and G. Galilei.

The theory of relativity has been successful in explaining many known phenomena and in predicting new ones. It therefore constitutes an important advance in our knowledge of the world. However, it is difficult to believe that it is an irreversible form of knowledge. On the contrary, if there is a lesson in epistemology (Popper, Lakatos, Kuhn) it concerns the conjectural nature of the physical theories of the twentieth century.

[...] The successes of relativistic theories are well known. And yet it would be wrong to conclude that any comparison with the experiments was invariably concluded with a perfect agreement. Physics is a human activity and from us it takes the defect of flaunting successes and hiding difficulties and failures.

Thus a great silence surrounded the Sagnac effect (of 1913) for which there is a real inability to explain it by the two relativistic theories, despite the attempts of some researchers (Langevin, Post, Landau and Lifshitz). Another silence has surrounded the meaning of the “Sagnac correction” of satellite physics, a sensational case of a theory that is used in this way: another one (Newtonian mechanics) is applied by sticking certain “relativistic corrections” to it, one of which is totally improper. Moreover, there are the half explanations of the aberration of starlight and the paradox of twins, phenomena for which the mathematical formalism of theory reproduces the observations, at the price of distorting the meanings beyond the lawful.

Behind the equations of a theory there is an enormous qualitative structure made up of empirical results, generalizations, hypotheses, philosophical choices, personal tastes, convenience.

When one takes note of this reality it is easy to understand that relativity, alongside its undeniable successes, can present weak points and can even survive some failures.

The correctness of mathematical formalism is not enough to homologate a structure as non-contradictory and not even a thousand physicists unconditionally favourable to theory are a sufficient guarantee, because almost always their thoughts have been oriented in an uncritical sense since university studies, towards an uncritical acceptance of the dominant theory

In reality the two relativistic theories are full of paradoxes, the solution of which, in a new context, is an objective of the book.

[...] We start from the “equivalent transformations” containing a free parameter, the coefficient of in time transformation (“synchronization parameter”) which expresses the conventional aspects of the theory in a synthetic way. Lorentz’s transformations are obtained as a particular case. Explicit calculations show that empirical observations are often insensitive to (Römer, Bradley, Fizeau, Michelson-Morley, Doppler, ...). Therefore in principle there are endless theories that can explain them equally well.

The transformations between inertial systems, applied to the real world, cannot avoid the question of accelerations because experimental laboratories have a small acceleration but not nothing. Accelerations, both linear (argument of the two spaceships) and centripetal (argument of the rotating platforms), break the symmetry between equivalent transformations. The only ones compatible with all known facts are the “inertial transformations”, based on and therefore on absolute simultaneity. Only with this can a discontinuity between inertial and accelerated systems be avoided. The Sagnac effect finally finds its complete explanation and satellite physics empties itself of its mysteries. Clear solutions of relativistic paradoxes emerge. Exemplary is the solution of the paradox of the twins: out of the way the relativism remains that the least ageing is always of those who have suffered the effects of absolute speed the most. The new theory shows that movement generates concrete effects on moving bodies. It must therefore be admitted that space is physically active, which clearly constitutes a return to the Lorentz ether. The explanation of relativistic phenomena by a theory based on the existence of a privileged system succeeds clamorously well.

If you will have the patience to read my book you will find other interesting things, about superluminal signals and the need to abandon the big bang, for example.

[...] all the theoretical models of the big bang find it absolutely essential to introduce the fourth dimension. It should then be noted that from a conceptual point of view these models have an unstable equilibrium, because they derive from the four-dimensional space of general relativity, which in turn is the product of the Minkowski space of special relativity.

Therefore the big bang depends essentially on the mixing of space with time in special relativity. In short, everything goes into crisis if the fourth Lorentz transformation is modified.

But this is exactly what I do by replacing Lorentz transformations with Inertial ones! In short, if the inertial transformations are right, the big bang cannot be true (Letter for *Lezioni di relatività*-FSPA. My underline).

Selleri started speaking on his new theory, which will be completed in 2011 with the publication of “Weak Relativity”.

As a member of the organizing committee, in a letter prepared to the host for the conference decided for Olympia in 2-5/10/1996 but finally held in Athens on 25-28/6/1997, Selleri stated his point of view on RT as well as the main source for his interest in FRT, explaining the need for a conference in FRT:

WHY A CONFERENCE OF THE FOUNDATIONS OF RELATIVITY

The true conceptual background of the two relativistic theories (special and general) has been re-emerging slowly in recent times, after more than half a century of domination of the neopositivistic conception that only mathematical rigor matters in a scientific theory. The positivistic standpoint had as a first consequence that Einstein's and Minkowski's roles were greatly inflated, while the great contributions of other physicists (Lorentz, Larmor, Poincaré,...) were practically forgotten. Einstein himself was strongly influenced by Mach in his youth and admitted that his special theory of relativity conformed with Mach's ideas. More than experimental data (Michelson-Morley etc), this cultural situation was the reason why typically realistic conjectures, such as that of ether, were eliminated in favour of abstract conceptions. At present time the domination of positivism does not exist anymore (at present time no singular current of thought is dominating the scientific milieu).

A correct understanding of the true history of relativity has recently produced several surprises, the first one being that people as important as Lorentz, Poincaré, Larmor, Fitzgerald had a fully realistic approach in relativistic physics. This they did not consider necessarily in contradiction with Einstein's theory.

For example Poincaré, often described as a conventionalist, repeatedly stated that ether was a necessary ingredient of physics. He did so even in the same page of his famous 1904 St. Louis paper where the first precise modern formulation of the relativity principle is given. It has also become evident that Einstein came back to the idea that a physical vacuum must exist, which he called “ether” in several papers from 1918 to 1955. He once told Popper that the greatest mistake of his scientific life was his youthful acceptance of positivistic philosophy.

It has been fully recognized that some fundamental ingredients of the two relativistic theories are basically arbitrary, the main one being the assumption of the so-called “Einstein's clock synchronization”.

This understanding surfaced at first in the works of epistemologists (Reichenbach, Grunbaum, Jammer, Searl, Cavalieri, Sjodin), then it influenced the works of physicists as well. But Einstein's synchronization is based on the assumed invariance of the one-way speed of light. Since conventional "truth" cannot be a necessary consequence of a true property of nature, it follows that invariance of one-way speed of light is not a law of nature! As a consequence the general-relativistic invariance of the ds^2 is also basically a human choice, rather than a property of the physical world.

This interesting and novel situation has brought new life in a field that many considered finally settled. Particularly remarkable has been the revival of the Lorentzian approach defended by Prokhorov [*Light in Einstein's universe*, Dordrecht: Kluwer, 1985] and Bell, the study of synchronization procedures different from the usual one, the discussion of Thomas rotation and of the Mocanu paradox. Great progress is being made towards a qualitatively better formulation of relativistic theory. At the present time there are several physicists active in the foundations of relativity. We believe that it will be useful to bring them together, with historians, philosophers of physics, and some other people active in related areas (EPR paradox, "superluminal" crossing of potential barriers, "superluminal" motion of clouds in quasars) in order to compare approaches and perspectives for a new development of modern physics.

The Olympia conference plan to have a set of overview talks concerning particular applications of the theory to practical problems: Sagnac effect and aircraft stabilizers, clock synchronization on the earth surface with using man-made satellites, nonlinear crystals and the hypothetical superluminal effects with correlated quantum pairs, quantum tunnelling and superluminal crossing of potential barriers, cosmological redshifts and their quantization phenomenon, relativistic conception of energy.

3.6-THE RISE OF A NEW THEORY: WEAK RELATIVITY⁸⁸⁵

At the end of his research, Selleri introduced the so-called Weak Relativity (WR). In some excerpts taken from the Selleri interview with Freire, he stated how, unlikely his activity in FQM, he managed to find a new theory, already in 2003 with the publication of his Italian book on relativity, i.e., "Lezioni di relatività":

[...] You see, my research in the foundations of quantum mechanics has the following strong points, the way I see it. First, the proposal of experiments to detect the objective reality of the probability waves.

⁸⁸⁵ All excerpts and statements introduced here, where not expressly indicated, refer to Selleri F. (2015), *Weak Relativity*, Barone M. (ed.), Athens: Kostarakis Scientific. ISBN: 978-618-81871-0-8, further referred to as (Selleri 2015). In this case, I did not change the acronym for the Theory of Special relativity introduced in the book, which is different from mine SRT.

Second, showing that the inequalities violated by experiments are very different from the original Bell's inequalities in many ways, so that they do not constitute a violation of local realism.

And third, proposing a set of new experiments with K mesons which could really allow one to discriminate between the Copenhagen approach and local realism. That is what I know. But this is not a new theory. It is only a set of ideas. But in relativity it is different. With relativity I could go much deeper and I have basically a new theory that can replace special relativity.

[...] And then a second point I would like to make is to stress again the importance of what I have done in the foundations of special relativity. Because now we have a completely new theory which is different from relativity.

[...] I published a book [...], "Lezioni di Relativita" in which there are five chapters. In the first three chapters there is a review of standard relativity and in the final two chapters there is a completely new approach and there is a full development of this type of ideas. So I was unable to build a new quantum theory, but at least I built a new relativity and I strongly believe it is correct (AIP 2003, My underline).

As a goal of his research in FRT, Selleri try publishing his book on WR, so as to disseminate quickly his ideas. The Italian version⁸⁸⁶ of the book (Selleri 2011) was released by a little Italian publishing house, Melquiades in Milan, and had a little spreading.

Then Selleri encountered considerable difficulties in the publication of the book's English edition. I show here again the letter to Springer (see page 637) inserting the full version, so as to show the importance of the book for Selleri:

-unpublished book: WEAK RELATIVITY which I submitted for publication to Springer something like 3 - 4 years ago. The contact was with a not very kind lady who probably kept it in a drawer for one year before refusing it. Question: is it possible that Springer reconsiders the negative decision, for example by sending the book to a new referee? Probably you can still find the text of WEAK RELATIVITY in the Springer archives. Otherwise, if you want, I will send a new copy.

I consider this book to be very important, as it extends my critical description of modern physics from quantum theory to relativity.

⁸⁸⁶ Some other excerpts in this section belong to (Selleri 2011) in the following.

In my previous books and papers I have shown that Heisenberg's indeterminacy relations and Bohr's complementarity principle do not forbid in any scientific way a space-time description in causal terms.

Symmetrically, in WEAK RELATIVITY I show that Einstein's relativity admits a conceptually simpler but experimentally equivalent (actually better) theory based on absolute simultaneity and on classical descriptions in space and time. Therefore also Einstein's relativity is an arbitrary principle which needlessly complicates the description of physical reality.

When it is replaced by my "weak relativity" principle (essentially Galilei's relativity) great steps forward are made in explanations, clarity, and understanding (Selleri to Tobias Schwaibold at Springer-undated-FSPA. Selleri's capital, my underline).

With respect to the difficulties on publishing the book, Barone stated very clearly that:

He was dissatisfied with Melquiades, but he himself decided to find Melquiades. He had tried with Roy Keys, who was thinking more about money, visibility, etc.. Then they had not reached an agreement, they had a quarrel.

Jaca Book, another publishing house with which he had already published, refused the book.

At an international level, business is business. It is not like here on a local level, that publishing houses select what to publish and what not.

In the introduction of the Italian book he thanked me for my contribution to the development. So I said to him to translate it into English and to get it published. He became convinced and told me to do everything.

After several re-workings and updates, he told me to make an introduction, because he was unhappy with the introduction of the Melquiades, made by a Puerto Rican scholar⁸⁸⁷, I think, who had exaggerated. "Selleri is the greatest etc". And Selleri did not like it. I did a sober thing by saying things as they are and he appreciated it a lot. Selleri wanted to write "with great contribution from Michele Barone". I had made a career, it is not that I was afraid (of being accused of being a heretic, of going against Einstein etc.), I thought my contribution was minimal. I told him, "no, the book is yours, I made the introduction, that's enough. I gave you the contribution". He was perplexed, he did not like my reaction. He thought I wanted to distance myself from the theory but it was not true. I was sorry because then he died and we stayed like that, without clarifying.

For the diffusion of the book I had the contract made. Kostarakis put the book online, incurred expenses, it took 200 copies. Because of the crisis he could no longer buy the paper to print the book, on the other hand he had to print a lot of things.

⁸⁸⁷ Actually has been Stephan J.G. Gift, at the University of the West Indies, Trinidad and Tobago.

But the price of paper skyrocketed. When I saw it in March 2019, the number of copies was low. But he is an international publisher, however, a guarantee in perspective.

In addition, the files concerning an unrefurbished version, which Selleri have been running among acquaintances, were suddenly and unexpectedly published online, without any permission. Luckily, the copyright for the latest version, however, is at Kostarakis. Some readers will read it online anyway, but it is not up to date.

I think that this one, in the USA, a stranger, had some contact with Roy Keys, who, not being able or not wanting to publish it, did Selleri a damage (Interview with Michele Barone, Athens, 17 June 2019).

In practice, Selleri never saw the release of his book in English, which was only published in 2015⁸⁸⁸.

Now I intend to show briefly the research path in SRT held by Franco Selleri to reach the astonishing discover of the precise value in the otherwise free parameter e_1 , from which the so-called Theory of Inertial Transformations (further refeerd to as TIT) rises, showing, moreover, with the specific case of the Sagnac effect, how it is able to explain not only all that the SRT explains but also what SRT fails to do.

3.6.1-THE SELLERI ROAD FROM LORENTZ TO INERTIAL TRANSFORMATIONS

The Einstein's SRT and GRT are very powerful tools that have explained a lot of phenomena and have foreseen others properly. For instance, the correct resolution of the precession of the perihelion of Mercury, the convertibility of energy into mass, the effects of speed and gravity on clocks and the weight of light. But they have also received a great deal of criticism regarding their reliability. It would not be right to conclude that any comparison of the theoretical predictions with the experiments was invariably solved with a perfect agreement. Even if the two RT are correct from the point of view of mathematical formalism, they are not coherent and free from contradictions. For instance, in the mathematical shell of SRT, the time-related report for two reference systems, called S_0 and S , the latter being in motion with respect

⁸⁸⁸ I recall that (Selleri 2011) is for Selleri F. (2011), *La relatività debole. La fisica dello spazio e del tempo senza paradossi*, Milano: Edizioni Melquifades. ISBN: 978-88-6218-711-4.; whereas (Selleri 2015) implies Selleri F. (2015), *Weak Relativity*, Barone M. (ed.), Athens: Kostarakis Scientific. ISBN: 978-618-81871-0-8.

to the former, depends only on x_0 , neither on y_0 nor z_0 , creating in this way a oddness between the points of space, which is conversely not only real but homogeneous. So, according to Selleri, in the time transformation:

something external, something unnecessary, something that maybe is going to get complicated the theory has been introduced. That is the original sin of the Special Relativity Theory⁸⁸⁹ (Selleri, 2011, pag 28).

As inherent in any physical theory, SRT and GRT can not represent the final form of knowledge. Selleri rightly points out that:

[...] the lesson to learn from epistemology (Popper, Lakatos, Kuhn) is about the conjectural, provisional, improvable nature of the physical theories of the XXth century.
[...] Einstein did not hide the transitoriness of his creations. On April 4, 1955 [in his last paper], he wrote: “The last, quick remarks must only demonstrate how far in my opinion we still are from possessing a conceptual basis of physics, on which we can somehow rely”. In a way this is a declaration of failure, but one has to admire the ethical dimension of the great scientist who had devoted the superhuman efforts of a lifetime to the attempt of reaching the deepest truths of nature and now, arrived at the end, declares to posterity: “I did not succeed” (Selleri 2015, pp. 26-27).

Indeed, the two theories of relativity are awashed in a series of paradoxes, as, for instance, the only partial explanations of the aberration of light and the clock paradox, phenomena for which the mathematical formalism of Einstein’s theory of relativity can reproduce observations but at the price of distorting the meaning of the symbols. Selleri makes explicit an absolutely incomplete list of paradoxes, limited exclusively to the SRT:

- 1) The idea that the simultaneity of spatially separated events does not exist in nature and must therefore be established with a human convention;
- 2) The relativity of simultaneity, according to which two events simultaneous for an observer in general are no more such for a different observer;
- 3) The velocity of a light signal, considered equal for observers at rest and observers pursuing it with velocity $0,99c$;

⁸⁸⁹ “[...] qualcosa di esterno, qualcosa di superfluo, qualcosa che probabilmente complicherà la teoria è stato introdotto. Questo è il peccato originale della teoria della Relatività Speciale” (Selleri 2011, pag. 28).

- 4) and 5) The contraction of moving objects and the retardation of moving clocks, phenomena for which the theory does not provide a description in terms of objectivity;
- 6) The hyper-deterministic block universe of relativity, fixing in the least details the future of every observer;
- 7) The conflict between the reciprocal transformability of mass and energy and the ideology of relativism, which declares all inertial observers perfectly equivalent so depriving energy of its full reality;
- 8) The existence of a discontinuity between the inertial reference systems and those endowed with a very small acceleration;
- 9) The propagation from the future towards the past, generated in the TSR by the possible existence of superluminal signals;
- 10) The asymmetrical ageing of the twins in relative motion in a theory waving the flag of relativism (Selleri 2015, p. 28).

These paradoxes can be completely overcome as soon as the new TIT is accepted. Selleri applies the theory to six different tests: the Sagnac effect; the rotating platform; the linear accelerations; the overcoming of the block universe; the aberration of starlight and the superluminal propagations.

Selleri founds, in his studies:

- A **set of experiments insensitive to clock synchronization**, that is, set of experiments with outcome predicted equally well by the TIT and the TSR. The set includes, for instance, experiments made by Michelson-Morley, Kennedy-Thorndike, Maiorana, Ives-Stilwell, Fizeau, the International Atomic Time;
- A **set of experiments preferring the TIT over the TSR/TGR**, that is, set of experiments predicted correctly by TIT, but not finding a rational explanation from TSR and/or TGR. The set includes, for instance, Sagnac effect, zero acceleration discontinuity of the velocity of light, aberration of the star light, block universe paradox;
- A **set of experiments preferring the TSR/TGR over the TIT**. Empty set (Selleri 2015, p. 58-59. Selleri's bold, my underline).

3.6.2-FROM LORENTZ TO INERTIAL TRANSFORMATIONS: MATHEMATICAL STEPS AND REMARKS

I am going first to introduce the mathematical steps in order to arrive to IT. It is well known that the Lorentz Transformations (LT), which form the basis of the SRT, are:

$$\begin{aligned}
x &= \frac{x_0 - vt_0}{R} \\
y &= y_0 \\
z &= z_0 \\
t &= \frac{t_0 - \frac{v}{c^2} x_0}{R}
\end{aligned}
\quad \text{where} \quad R = \sqrt{1 - \frac{v^2}{c^2}}$$

R. Mansouri and R.U. Sexl, in their three articles of 1977,⁸⁹⁰ pointed out that the LT contain a conventional term in the transformation of time which depends on x coordinate.

Selleri's reasoning is as follows: given two inertial reference frames, S_0 and S , the following standard assumptions are advanced:

1. Space is homogeneous and isotropic and time homogeneous, at least from the point of view of observers at rest in S_0 ;
2. Relative to the isotropic system S_0 the velocity of light is "c" in all directions, so that clocks can be synchronized in S_0 with the Einstein method and the one way velocities relative to S_0 can be measured;
3. The origin of S , observed from S_0 , moves with velocity $v < c$ parallel to the $+x_0$ axis, that is, according to the equation $x_0 = vt_0$;
4. The axes of S and S_0 coincide for $t = t_0 = 0$ (Selleri 2015, pp. 72-73).

Starting from these assumptions, the so-called General Transformations (henceforth, GT) are obtained:

$$\begin{aligned}
x &= f_1(x_0 - vt_0) \\
y &= g_2 y_0 \\
z &= g_2 z_0 \\
t &= e_1 x_0 + e_4 t_0
\end{aligned}$$

Analyzing the several parameter, it can be said that:

⁸⁹⁰ Mansouri R., Sexl R.U. (1977), "A test theory of special relativity: I. Simultaneity and clock synchronization", *Gen. Relativ. Gravitation*, Vol 8, pp. 497-513; "A test theory of special relativity: II. First order tests", *Gen. Relativ. Gravitation*, Vol 8, pp. 515-524; "A test theory of special relativity: III. Second order tests", *Gen. Relativ. Gravitation*, Vol 8, pp. 809-814, 1977.

- Lorentz contraction of moving objects is thus obtained for:

$$f_1 = \frac{1}{\sqrt{1 - \frac{v^2}{c^2}}} \quad \text{and} \quad g_2 = 1;$$

- Larmor retardation of moving clocks is obtained if we put:

$$e_1 v + e_4 = \sqrt{1 - \frac{v^2}{c^2}};$$

- Invariance of the two way velocity of light, is get for:

$$g_2 = f_1 \sqrt{1 - \frac{v^2}{c^2}} \quad \text{and} \quad e_1 v + e_4 = f_1 \left(1 - \frac{v^2}{c^2} \right).$$

Now, starting from GT, two conditions are introduced, based on solid empirical evidence:

- the speed of light in the two directions is the same in all directions and in all IRS, i.e. $c_2(\theta) = c$;

- the delay of the clock depends on the usual R-factor when the clock moves with respect to S_0 .

Then, the so-called Equivalent Transformations (ET) are obtained:

$$\begin{aligned} x &= \frac{x_0 - vt_0}{R} \\ y &= y_0 \\ z &= z_0 \\ t &= Rt_0 + e_1(x_0 - vt_0) \end{aligned}$$

In the ET, all theories comparable to SRT are present. For instance, Lorentz

Transformations are get as a special case of ET if: $e_1 = -\frac{v}{c^2 R}$.

As Selleri claimed, physical events require a precise value of e_1 :

The provisionally free parameter e_1 defines in S the simultaneity of distant events, or, which is the same, chooses the clock synchronization method to apply in S . Clearly, then, an appropriate denomination for e_1 is “synchronization parameter”. The only remaining unknown factor is e_1 .

Most experts of the foundation of the relativistic theories consider e_1 essentially a free parameter to be fixed by the convention concerning clock synchronization, but [here is showed] the opposite, namely that physical phenomena require a fixed value of e_1 , precisely $e_1=0$. A theory different from the TSR is clearly needed (Selleri 2015, pp. 80-81. My underline).

Thus, different synchronization conventions lead to different values of the e_1 parameter and to different theories of space and time which are, to a large extent, empirically equivalent. But, in all cases, except for the SRT, these values imply the existence of a privileged reference system.

Selleri argued that:

[...] the condition $e_1 = 0$ will be obtained six times, independently. The fact that so many proofs exist is a strong indication of the basic correctness in nature of absolute simultaneity (Selleri 2015, p. 82).

Absolute simultaneity, i.e. the absence of spatial coordinates in the transformation of time, thus leads to the Inertial Transformations (IT), with $e_1=0$:

$$x = \frac{x_0 - vt_0}{R}$$

$$y = y_0$$

$$z = z_0$$

$$t = Rt_0$$

While the LT of SRT introduces a symmetry between spatial and temporal variables, forcing the latter to a geometric role in a four-dimensional space, the IT, although not forming a group, according to Selleri:

[...] **imply a complete liberation of time from the merely geometrical role to which it had been forced in the Minkowski space.** Furthermore [they] predict that **the velocity of light** relative to an inertial system S moving with respect to the privileged system S_0 **is not isotropic**. A corresponding anisotropy is predicted for Reichenbach's parameter ϵ . By studying the multiplication properties of **the inertial transformations** it has been possible to show that they **do not form a group**.

There are no problems with the existence of the identical and inverse transformations, and also the associative law can be satisfied, **but it is not always possible to write a meaningful product of two inertial transformations, due to the presence of two absolute velocities v and v' in the transformation.** [If they are different, the product is no inertial transformation].

[Another] property implied by IT is absolute simultaneity [...].

[Its] existence [...] does not imply that time is absolute: on the contrary, the v -dependent factor in the transformation of time gives rise to time-dilatation phenomena similar to those of TSR. A clock at rest in S is seen from S_0 to run slower, but a clock at rest in S_0 is seen from S to run faster so that both observers agree that motion relative to S_0 slows the pace of clocks. The difference with respect to TSR exists because in a meaningful comparison of rates a clock at rest in S_0 must be compared with at least two clocks at rest in different points of S , and the result is therefore dependent on the “convention” adopted for synchronizing the latter clocks. Absolute length contraction can also be deduced [...]. (Selleri 2015, pp. 83-84. Selleri’s underline, my **bold**).

Selleri suggested his TIT, based on absolute simultaneity, as the best theory of space and time. It implies the existence of a privileged isotropic inertial reference system. He stated that:

[...] it is possible resynchronize clocks in all inertial frames in such a way as to select a different, arbitrarily chosen frame as “privileged”.

Such a resynchronization of clocks does not modify any empirical consequence of the theory, which is thus compatible with a new form of relativity principle, weaker than adopted by Einstein in the Theory of Special Relativity (Selleri 2015, p. 250).

Referring to the text *Weak Relativity* for a complete demonstration of the six situations in which TIT explain the above cases better than the TSR, the specific case of the Sagnac effect is discussed here in detail. Then the overcoming of the ten paradoxes, as explained by Selleri himself, will be shortly introduced.

3.6.3-AN APPLICATION OF WEAK RELATIVITY THEORY: THE SAGNAC EFFECT NATURALLY EXPLAINED

Let us introduce the experiment using Selleri’s statement:

In the Sagnac 1913 experiment a platform was made to rotate uniformly around a vertical axis at a rate of 1-2 rotations per second.

In an interferometer mounted on the platform, two interfering light beams, reflected by four mirrors, propagated in opposite directions along a closed horizontal circuit defining a certain area A . The rotating system included also the luminous source and a photographic plate recording the interference fringes. On the pictures obtained during a clockwise and a counterclockwise rotation with the same frequency, Sagnac observed the interference fringes in different positions and measured the displacement Δz by overlapping the two figures. This Δz is strictly tied to the relative time delay with which the two light beams reach the detector. Sagnac observed a shift of the interference fringes every time the rotation was modified.

[...] The experiment was repeated many times in different ways, with the full confirmation of the Sagnac results. (Selleri 2015, pp. 114-115).

But, unexpectedly:

[...] theoreticians seemed little interested in the Sagnac effect, as if it did not pose a conceptual challenge (Selleri 2015, p. 116).

More than a century after the birth of the Sagnac effect, no one has managed to give a theoretical explanation based on SRT and GRT.

I am going to show now how Selleri worked out in the case of the Sagnac effect and how TIT could give the exact result which is seen on the rotating platform, i.e., on the disk.

There is a temporal difference Δt_0 in the two paths of light propagating in opposite directions:

$$\Delta t_0 = \frac{2L_0 v}{c^2 R^2} = \frac{2Lv}{c^2 R}$$

$L_0 = LR$ is the disk circumference measured by observers, who see the rotating disk, at rest in the laboratory, whereas L is the circumference of the disk measured by observers at rest on the disk.

On the disk there are different speeds in the parallel and antiparallel case from which $\Delta t = \frac{2L\Gamma}{c}$. Seen from the disk, the delay is therefore

$$\Delta t = \Delta t_0 R \left(1 + \frac{c^2 e_1 R}{v} \right). \text{ The consistency of } \Delta t = R\Delta t_0, \Delta t_0 = \frac{2L_0 v}{c^2 R^2} \text{ and } \Delta t = \frac{2L\Gamma}{c},$$

allows us to find the proper value of e_I . In fact, setting $R = \frac{\Delta t}{\Delta t_0}$ and

requiring the same phenomenon, we have $R = \frac{2L\Gamma}{c} \frac{c^2}{2L_0} \frac{R^2}{v}$. By applying

$L_0 = LR$, we have $1 = \frac{c}{v}\Gamma$ and, as it is $\Gamma = \frac{v}{c} + ce_1 R$, the obtained result is only

possible for $e_I = 0$. Only the absolute simultaneity of TIT allows us to understand the Sagnac effect, giving it a rational description! For all other values of e_I you have wrong results. For instance, in the TSR, in which $e_1 = -\frac{v}{c^2 R}$, the wrong prediction $\Delta t = 0$ occurs.

After analyzing all paradoxes, Selleri's conclusion seems to be clear:

...the TIT explains all the examined experiments, while the two Einstein relativistic theories have serious problem with the experiments of the second set. In this way we see that there is an important logical space for a theory alternative to the TSR.

[...] In view of the results [...] which hopefully should constitute a blow to conventionalism, one can say that the simultaneity adopted in the TSR, more than conventional, is arbitrary and, it turns, not correct.

My recent research has shown that the arbitrariness of relativistic simultaneity opens a logical space to a different theory (the theory of the inertial transformations) that agrees with experiments better than the TSR. (Selleri 2015, p. 59. My underline).

Selleri identified how the relativistic theory lacks the flexibility that normally would be required to a physical theory. Selleri believed to meet the response in the mathematical instability of the theory, that is, stressing that the LT are necessary consequences of the relativity principle and the principle of the invariance of the velocity of light (the strong relativity, as he defines the above both principles).

Any violation of LT found at any time will imply that strong relativity itself does not hold as a description of nature. Selleri stated also that:

The only possible defense of the TSR is to assume that with the LT the theory has given mankind an everlasting final truth.

But also from a very general point of view to attack the notion of eternity to a man-made theory in modern times does not look like a brilliant idea (Selleri 2015, pag. 269).

Concerning the foregoing paradoxes introduced by Selleri, he submitted in his (Selleri 2015, pp. 265-268) the corresponding solutions he found⁸⁹¹.

He stressed how his approach gets rid of all of them and it “does not look anymore as a free choice, but rather as a scientific necessity, being a consequence of the many independent proofs of absolute simultaneity” (Selleri 2015, pag. 265).

Selleri uses the same order with whom he introduced the paradoxes:

- On the (relativistic) idea that the simultaneity of spatially separated events does not exist in nature and must therefore be established with a human convention, Selleri recalls how in the book he established that only the absolute simultaneity ($e_1 = 0$) can give a rational description of some physical phenomena, as the Sagnac effect, the linearly accelerating systems, the objective reality of inertial observers, and the superluminal propagation⁸⁹².
- On the relativity of simultaneity, according to which two events simultaneous for an observer in general are no more such for a different observer, Selleri shown that:

assuming ($e_1 = 0$) all inertial observers have the same reality, where reality is defined by the set of events simultaneous with a given event (e.g., the “here-now” event establishing the local present) (Selleri 2015, pag. 266).

- On the velocity of a light signal, considered equal for observers at rest and observers pursuing it with velocity as near as possible to c ,

after having established that $e_1 = 0$, the velocity of light relative to a moving reference frame is $c_1(\theta) = c / 1 + (v/c) \cos \theta$. Therefore, the speed of the light signal (absolute velocity c) relative to an inertial frame which is running after it (then, $\theta = 0$) with absolute velocity almost equal to that of light has a denominator $1+1=2$ and the limit velocity is $c/2$. This is a 50% reduction with respect to the relativistic prediction which remains the old dear c (Selleri 2015, pag. 266).

⁸⁹¹ Being a really short explanation, for some paradoxes, Selleri referred to chapters in the book where they are explained in detail. Obviously I cannot report the full clarification, so sometimes, the “explanation” resembles to a mere assumption statement.

⁸⁹² I am going to introduce this topic in Section 3.7.

- On the retardation of moving clocks, phenomenon for which the RT does not provide a description in terms of objectivity, this latter can be restored with the IT,

in terms of action of the ether on all the periodic phenomena which can be used to measure time. All this is very much in the realistic line of thought of Hendrik Lorentz (Selleri 2015, pag. 266).

- On the contraction of moving objects, phenomenon for which, once more, the RT does not provide a causal description, the objectivity can be restored with the IT,

in terms of action of the ether on every atom, with reduction of the atomic length in the direction of motion. Also this follows the realistic line of thought of Lorentz (Selleri 2015, pag. 266).

- On the hyperdeterministic block universe of relativity, fixing in the least details the future of every observer, the conclusion $e_1 = 0$ means that

the reality line is unique for all observers, independently of their state of motion (Selleri 2015, pag. 266).

- On the conflict between the reciprocal transformability of mass and energy and the ideology of relativism, in which SRT declares all inertial observers perfectly equivalent so depriving energy of its full reality,

the retrieval of the objectivity of energy and of the other physical quantities should rather aim at the inequivalence of the different reference frames knowing that there is one at rest in the ether, which has a more fundamental role.

[...] the objectivity of energy is fully recovered by working with the inertial transformations (Selleri 2015, pag. 267).

- On the existence of a discontinuity between the inertial reference systems and those endowed with a very small acceleration, the discontinuity is

in the ratio of the velocities of light along two opposite directions. It turns out to be a serious problem for all $e_1 \neq 0$. If one takes $e_1 = 0$, however, the discontinuity does not exist anymore and the relative difficulty is completely overcome (Selleri 2015, pag. 267).

- On the propagation from the future towards the past, generated in the SRT by the possible existence of superluminal signals, Selleri claimed that:

[...] the essence of the causal paradox lies in the impossible requirement that a superluminal propagation may overtake a set of clocks marking a progressively decreasing physical (then, not conventional) time. The [...] choice $e_1 = 0$, which [...] is selected by several phenomena in which the acceleration has a role, remains by far the best one also in the present context, being the only one not leading to the causal paradox. The same choice avoids the complications of the TSR describing all the propagations as forward in time for all observers (Selleri 2015, pag. 267).

- On the asymmetrical ageing of the twins in relative motion in a theory waving the flag of relativism, Selleri recalled how he dealt with

the differential retardation effects between separating and reuniting clocks ("clock paradox"). A variational method was used to show, both in TSR and in more general theories with arbitrary e_1 , that among all possible trajectories of a clock connecting two given points at two given times the rectilinear uniform motion requires the longest proper time. A complete resolution of the clock paradox is so obtained by giving an exhaustive unified description of all possible situations. Relativism does not apply and must be considered obsolete. Velocity (and nothing else) is seen to be responsible for the differential retardation effect. Of course it must be an absolute velocity! Hidden behind the relativism of Einstein's theory there is a physically active background (Selleri 2015, pagg. 267-268).

Of course, the foregoing statements are unclear, not having fully investigated all the chapters in which they are explained in detail, which obviously would have taken us beyond the scope of this thesis.

3.6.4-WEAK RELATIVITY:

NOT A DISRUPTIVE THEORY, THOUGH. SELLERI'S REMARKS

At the end of his research⁸⁹³, Selleri, very fair and square, admitted how his theory did not disprove SRT, having milder implications, despite its strong and contrasting outcomes. He obtained several proofs which show how his TIT could be better than SRT in his thoughts, but he also decided to analyze the issue fully, all-round. And something seems missing. I would like to emphasize that, in the whole Selleri's approach, the physical truth is paramount over any personalization or defence of one's own beliefs.

In the 18th chapter of his (Selleri 2015), Selleri stated why WR has precisely this adjective in his title, weak!

In a sense Einstein's relativity is a strong principle. When it says that the physical laws "are not affected" by a change of reference, it requires the laws of nature to have exactly the same form in all inertial frames.

From such a point of view the inertial transformations (IT), based on absolute simultaneity, have milder implications (Selleri 2015, pag. 248. His underline).

According to the above statement, a plausible name given by Selleri for the TIT is that of Weak Relativity. That's why the theory has this name!

In Selleri's thought, having built the physics of space and time on $e_I = 0$, i.e., on an absolute simultaneity, the main question was how to identify the privileged system, the absolute one. After attempts and failures, Selleri showed that:

[...] It is not possible to give a positive answer to such a question [...]. There is a well defined way to resynchronize the clocks of the universe (that is, the clocks of all inertial frames) which leads from the given privileged system to a new, arbitrarily chosen, "privileged" inertial system, where the privilege is the isotropy of space with respect to light propagation. Relativism pushed out of the door comes back through the window (Selleri 2015, pag. 248. My underline).

⁸⁹³ This section is mainly based on (Selleri 2015, pp. 248-249; and pp. 263-269). Even though there are not precise quotations, nevertheless my discourse is ascribable and can be traced back to the aforementioned pages.

According to Einstein's Strong Relativity, *the laws of physics are exactly the same in all inertial systems*; whereas the Weak Relativity, states merely that *it is impossible to measure the absolute velocity of the Earth*. This last principle does not demand necessarily the validity of LT, does not lead to them, and opens a logical space for new theories. The latter formulation, according to Selleri, is basically the original one as extended by Galilei to all physical phenomena.

Selleri also showed very fairly some unclear points of his theory, and makes profession of intellectual honesty giving priority to physics, even on the defence of the validity of WR. As a result of the new theory, the new relativistic requirements are much weaker than before, even if the LT has to be replaced by IT.

Anyway:

Experiments show (qualitatively) that absolute velocities exist in nature. The weak relativity principle accepts this, but maintains that they are not measurable. In spite of this, I must insist that the statements made [...] are correct.

The theory with the free e_1 applied to linear motions and to the Sagnac effect shows that only $e_1 = 0$ gives a rationally acceptable formulation. Similarly, only with $e_1 = 0$ one can obtain a reasonable description of aberration. The paradoxes of TSR disappear if $e_1 = 0$. The growing evidence for superluminal signals can easily be accepted if $e_1 = 0$, while it is incompatible with standard relativity due to the presence of the famous causal paradox. Therefore the best theory of space and time contains absolute simultaneity (Selleri 2015, pag. 249. My underline).

Nevertheless we must admit that our results may seem somewhat contradictory. On the one hand, they point to a theory of space and time in which such conceptions as absolute velocity, privileged frame and absolute simultaneity have a central role; while, on the other hand, a form of relativism comes back in the arbitrariness of the choice of the 'privileged' inertial frame (Selleri 2015, pag. 249; pag 265. My underline).

Then Selleri stated a honest but shocking statement, which does him credit in recognizing the "weakness" of his new theory, which is also very strong in showing and resolving the inconsistencies of the SRT:

[...] I can add that from the point of view of the inertial transformations the validity of weak relativity appears accidental, more than fundamental (Selleri 2015, pag 265. My underline).

3.7- SELLERI'S OTHER ACTIVITIES IN THE 1990s AND 2000s: SUPERLUMINAL SIGNALS. COSMOLOGY, ASTROPHYSICS, AND GPS WITH ALTON ARP AND TOM VAN FLANDERN. WITH A DIGRESSION ON NUCLEOSYNTHESIS AND A RECOVERY TO BOHR'S COMPLEMENTARITY AND HEISENBERG'S INDETERMINACY RELATIONS

In order to give the most complete view on Selleri's diverse and tireless activity, I would like to highlight here his research on superluminal signals and his overlook to Cosmology and Astrophysics, always trying to help other scientists (Alton Arp and Tom van Flandern, in such circumstance) which experienced some troubles for their ideas. Selleri has been unquestionably considered an heretic, even though, my guess, not for a blind opposition for its own sake to the dominant theories, but for the desire to give answers to the doubts that the same theories contain and still present. A desire that, as I have shown repeatedly, was linked to his philosophical, political and social convictions. Last but not least, he was also the editor in 2007 of a book on *Controlled Nucleosynthesis*⁸⁹⁴, a fairly unexpected field. Selleri stated in a personal document in FSPA that:

In the following years the problem which recalled my father's nice tale attracted me more and more. I can give no rational explanation about the reason why the thoughts about the nature and possible mechanisms of nuclear synthesis became habitual and transformed into a hobby which did not consume much time but was a sort of almost unintentional background work which contributed to train my imagination.

For many years I did not consider this problem seriously as I did not think that my contribution to its solution would be very meaningful in comparison with the efforts of true experts. But such a "hobby" would hamper nobody and nothing and, on the other hand, it was a fascinating business.

⁸⁹⁴ Adamenko S.V., Selleri F., van der Merwe A. (eds) (2007), *Controlled Nucleosynthesis: Breakthroughs in Experiment and Theory*, Dordrecht: Springer.

Considering stably functioning biological and technological systems composed of simple elements of the same type, similar to the way in which atomic nuclei are composed of nucleons, I looked for a hidden logic of their birth and evolution and for a mechanism which could help me with the unsolved problem of controlled nuclear synthesis.

I was troubled by the fact that the process producing a huge energy which is at the same time the main source of energy in the Universe arising spontaneously and keeping running in the stars seemed not to be reproducible under terrestrial conditions despite important efforts. For a long time it seemed to me illogical that the simplest energy producing process of fusion of the light nuclei, in which these nuclei take part easily by objective reasons, cannot be reproduced in an efficient controlled way, in spite of the fact that science has attained a high level (FSPA).

3.7.1-ON SUPERLUMINAL SIGNALS.

A LONG-STANDING ACTIVITY.

With respect to superluminal signals (SLS in the following), as stated by (Baracca, Bergia, Del Santo 2017) and (Del Santo preprint 2020),

Between 1979 and 1981, Selleri tried to stress the paradoxical aspects of quantum entangled, again with the aim of proving a breakdown of quantum mechanics, by proposing protocols for superluminal signalling, which he presented in the course of scientific meetings (without however ever publishing his ideas).

These proposals were similar to those put forward by Nick Herbert from the [...] Fundamental Fysiks Group in the USA: the so-called “QUICK” experiment in 1979 and the “FLASH” in 1981 (Herbert, 1982; see also Kaiser, 2011). Selleri became very sympathetic with these proposals, which were however conceived with the different spirit of actually achieving superluminal communication. After a series of correspondence, Selleri and Herbert met at a conference in Perugia and the former “was besotted with Herbert’s latest proposal [...]. More important, he pushed copies of Herbert’s preprint on several other colleagues, and convince an experimental physicist from Pisa to mount a real test of Herbert’s design” (Kaiser, 2011) (Del Santo 2020 preprint, pag 8. My underline).

[...] Ghirardi recalled two preprints by Selleri on this topic: “Einstein locality and the quantum-mechanical long-distance effects” 1979, based on his presentation at a meeting in Udine, and a second preprint by Cufaro Petroni, Garuccio, Selleri, and Vigier, 1980⁸⁹⁵ (see Kaiser, 2011).

⁸⁹⁵ Cufaro-Petroni N., Garuccio A., Selleri F., Vigier J.P. (1980), “Sur la contradiction entre la théorie quantique classique (idéalisée) de la mesure et la conservation du carré du moment angulaire total dans le paradoxe

Herbert Pietschmann recalls that Selleri presented before him and Walther Thirring a proposal for superluminal communication during the leave of absence that Selleri spent in Vienna in 1980-1981 (Pietschmann interviewed by Del Santo on 15-11-2016--Del Santo 2020 preprint, footnote n. 13, pag 8).

Actually, my research shows how Selleri continued this research, still writing nine papers between 1980 and 2006 in the issue of superluminal signals, eight of which were edited. Specifically, he published first the preprint recalled by Ghirardi as Selleri F. (1980), “Einstein Locality and the Quantum Mechanical Long Distance Effects” in *Dynamical Systems and Microphysics*, Blaquiere A., Fer F., Marzollo A. (eds), Wien/New York: Springer-Verlag, pp. 393-412)⁸⁹⁶. It emerges as another example of his unified research, which goes beyond individual fields of research. In the years of his research on FRT, he dealt with FQM and SLS, which he will also apply to FRT. Selleri analyzed the EPR paradox pointing out that it

called attention, for the first time, to the quantum mechanical treatment of widely separated events and stressed the necessity that any reasonable physical theory treats them as independent (Selleri Long Distance 1980, pag. 393).

Furthermore, Selleri started discussing the proof of Bell’s inequality following Bell’s “Theory of local beables”,

which seems of great generality since it is based only on the assumption of relativistic causality, i.e., on the idea that an event in space-time is determined exclusively by the events of its backward light-cone (Selleri Long Distance 1980, pag. 397. Selleri’s underline).

In the same paper, after recalling the well known Bell-type experiments, Selleri dedicates a paragraph to SLS, expanding his work submitted one year

d’Einstein, Podolski et Rosen”, *Comptes Rendus de l’Academie des Sciences*, 290B, pp. 111-113. See the Bibliography,

⁸⁹⁶ (Selleri Long Distance 1980) in the following.

earlier in a paper written with Garuccio in 1979, “Quantum Mechanics, Angular Momentum Conservation and a Possible Action at a Distance”⁸⁹⁷:

A development of the EPR-paradox which allows one at least in principle to send arbitrarily complicated signals with velocity larger than that of light over arbitrarily large distances has recently been found by A. Garuccio and the present author [in (Selleri, Garuccio 1979)] (Selleri Long Distance 1980, pag. 403).

Selleri exposed how QM leads to an instantaneous propagation, dealing with QED and a part of relativistic theory.

Selleri approached whether or not the eventual violations of Bell’s inequality can carry a signal. He underlined how Bohm and Hiley described this opportunity as a road which led to a violation of the principles of RT, still allowing faster than light (FTL) signal. According to the above overview by Selleri, there are signals exchanged instantaneously:

[...] there seem to be problems in reconciling with special relativity a full validity of the quantum mechanical treatment of distant systems (Selleri Long Distance 1980, pag. 407).

Selleri’s conclusions sounded very clear, as a confirmation of the struggle between locality and QM:

The contrast between Einstein locality and quantum mechanics is now becoming increasingly clear. [...] if Einstein locality will be found to be violated in nature, as the preliminary experimental evidence seems to imply, instantaneous transmission of signals between points with arbitrarily large distance shall have to be admitted. The least that it can be said is that such a possibility is against the spirit of special relativity. Theoretical investigations of “tachionic” effects have in recent years shown that their existence is compatible with the formalism of the theory of relativity: one has however a reversal of causes and effects and very funny descriptions of physical reality should be accepted. If Einstein locality survives as a true property of nature then quantum mechanics shall have to be modified (Selleri Long Distance 1980, pag. 410. My underline).

⁸⁹⁷ Garuccio A., Selleri F. (1979), “Quantum Mechanics, Angular Momentum Conservation and a Possible Action at a Distance”⁸⁹⁷, in *Centenaire de la Naissance D'Albert Einstein 1879-1955*, Parigi 6-9 Giugno 1979. (Selleri, Garuccio 1979) in the following.

Anyway, the reversal of causes and effects was accepted by many scholars, (think to the interpretation of the positron, for instance).

According to Selleri, QM wants to eradicate the locality, but then the entangled states are the SLS foreseen by the QM, and they have to propagate instantaneously. But they would thus counteract the RT. So, to save the QM, which shows another big contradiction, currently, a “solution” to justify the QM approach to the theory of measurement is that of decoherence. The faster than light effect is present but not real, not physical.

Selleri published two other papers in the 1980s and, still confirming his long-standing research on this topic he wrote six papers between 2002 and 2006, more than twenty years after, five of which were published.

In 1980 he wrote, jointly with Vigier, “Unacceptability of the Pauli-Jordan Propagator in Physical Applications of Quantum Theory”⁸⁹⁸, published on *Lettere al Nuovo Cimento* after less than two months.

They analyzed the opportunity of “superluminal influences between independent measurements in EPR-type experiments”, as proposed by Costa de Beauregard in order to move information backward into time.

Recalling the discover of the positron by Anderson (See also pages 222-223 in this dissertation) the authors stressed an evidence in favour of the propagation of energy only toward the future:

Recent discussions on the theoretical possibility of superluminal influences between independent measurements in EPR-type experiments (a possibility which results from quantum theory but, pending Aspect’s and Rapisarda’s experimental results are not yet firmly established) have reached a crucial stage with Costa de Beauregard’s explicit proposal to utilize the Pauli-Jordan propagator to move information backward into time. As is known, he has proposed to “telegraph into the past” by moving an advanced potential from the measurement-event *A* to the source *S*, followed by a retarded one from *S* to *B*. Clearly this process (or the opposite one) is not equivalent to sending negative energy from *A* to *S*, i.e. to shoot negative energy in a backward time direction.

⁸⁹⁸ Selleri F., Vigier J.P. (1980), “Unacceptability of the Pauli-Jordan Propagator in Physical Applications of Quantum Theory”, *Lettere al Nuovo Cimento*, 29 (1), pp. 7-9. (Selleri, Vigier Propagator 1980) in the following.

Indeed following Einstein and Feynman this would be equivalent to the creation of a particle-antiparticle pair at S with positive energy flowing forward in time, as usual, and would constitute no change with respect to the standard interpretation of QM which leads to violations of Bell's inequality.

What in reality Costa de Beauregard now claims using the Pauli-Jordan propagator is that one can send positive energy backward into time, so that energy can travel from A to S (and from S to B) in an overall covariant process.

[...] The important fact that we extract from [...] quotation [on Anderson discovery of the positron] is that positive-energy states, moving forward in time (as both electrons and positrons can be considered) lose momentum, while crossing a lead plate. The origin of this phenomenon is well known: the interactions of the charged particle with the atoms of the plate are practically always such that the particle loses energy, momentum and velocity. If one were willing to contemplate the possibility that a positron is actually an electron moving backward in time, one should conclude that such a motion through a plate would consist of a long series of very unlikely interactions each resulting in a gain of velocity of the positron interacting in the plate. This is clearly a process with entropy decreasing. Obviously we witness here an exchange of entropy and negentropy: this is the reason why one cannot use antimatter, reinterpreted as normal matter propagating backward in time to send signals to the past.

[...] The very good agreement of dispersion relations with experiments is therefore a very strong piece of evidence in favour of the propagation of energy only toward the future. The counterproof is the fact that, when the so-called Puppi-Stanghellini discrepancy appeared, the general reaction was that this discrepancy, if confirmed by experiments, would have implied a revisal of the causality conception of quantum field theory. This discrepancy, however, disappeared as soon as more accurate experimental data were collected (Selleri, Vigier Propagator 1980, pagg. 7-9).

In 1989, as a confirmation of the interest for Herbert work, Selleri published "Modified Herbert Experiment"⁸⁹⁹. In the abstract he stated that Herbert's result can be restored using an amplifier, but the superluminal signals are however not acceptable. He also proposed two solutions to the emerging paradox:

The EPR experiment proposed by N. Herbert (1982) was supposed to provide a mechanism for superluminal signals.

⁸⁹⁹ Selleri F. (1989), "Modified Herbert Experiment", *Bulletin of Calc. Mathematical Society*, 81, pp. 490. (Selleri on Herbert, 1989) in the following.

It has however been shown by several authors that the assumptions made by Herbert are not correct because quantum theory rules out the existence of a device capable of amplifying an arbitrary state.

However the amplification of any given state by a device designed specifically for that state is not ruled out. We show that the existence on an amplifier of the latter type could be enough for restoring Herbert's result. Superluminal signals are however not acceptable and the latter conclusion is paradoxical. There are two possible solutions of this new form of the EPR paradox. In the first one it is assumed that an amplifier actually reduces the photonic wave packet with which it interacts, contrarily to theoretical expectations. This can be checked in single photon experiments quite independently of the EPR paradox. In the second solution every photon of an EPR pair must have well defined values of all types of polarization (circular, linear along an arbitrary direction,...) and quantum theory can thus be shown experimentally to be incomplete (Selleri on Herbert, 1989, pag. 490. My underline).

In the period between 2002 and 2006, Selleri wrote six papers, five of which were published in some conference proceedings and in several journals as *Foundations of Physics Letters* or *Foundations of Physics*. The papers are the following:

- Selleri F. (2002), "Superluminal Signals Require Absolute Simultaneity", *Bullettin de la Société des Sciences et des Lettres de Łódź*, 52, pp. 63-78. AND in *Proceedings of the International Conference Ideas of A.A. Michelson in Mathematical Physics*, Mathematical Conference Centre Bedlewo: Poland, August 2002. (Selleri SLS 2002a) in the following;
 - Selleri F. (2002), "Superluminal Signals allow to detect absolute motion", Unpublished. (Selleri SLS 2002b) in the following;
 - Selleri F. (2002), "Our Absolute Velocity", in *Physical Interpretation of Relativity Theory*, Duffy M. C. (ed), London: British Society for the Philosophy of Science, September 2002. (Selleri SLS 2002c) in the following;
 - Selleri F. (2003), "On the Fizeau Experiment", *Foundations of Physics Letters*, 16 (1), pp. 71-82. (Selleri SLS 2003a) in the following;
 - Selleri F. (2003), "Superluminal Signals and Causality", *Annales de la Fondation Louis de Broglie*, 28 (3-4), pp. 507-523. (Selleri SLS 2003b) in the following;
 - Selleri F. (2006), "Superluminal signals and the resolution of the causal paradox", *Foundations of Physics*, 36 (3), pp. 443-463. (Selleri SLS 2006) in the following.
- Selleri's approach was focused on this way:

If such signals have unlimited velocity the causal paradoxes generated in the theory of special relativity can be solved by considering a set of theories empirically equivalent to one another (set to which the SRT belongs), differing in clock synchronisation only.

By applying the principle of relativity to the whole set we show that the solution of the causal paradoxes is possible only by assuming absolute simultaneity in the transformations of space and time.

This conclusion is not only compatible with the classical experiments on which the SRT is based, but is also required by other independent considerations (Selleri SLS 2002a, pag. 63. My underline).

In his early studies in FRT, Selleri considered the existence of superluminal propagations possible, perhaps even probable, pointing out almost four different cases of a SLS occurrence. Maybe the main issue is the chance to apply the absolute simultaneity as a solution of the paradoxical presence of SLS:

From the theoretical point of view it has been shown [Barut, Maccarone, Recami 1982] that superluminal solutions of the relativistic equations exist. For example, Maxwell's equations have solutions representing electromagnetic waves propagating with arbitrarily large group velocity, a rather unexpected result.

Experimentally, evidence of phenomena propagating with velocity larger than c has been found in different areas, such as:

- 1) astrophysics: if quasars are taken to be at redshift distances (big bang model), then superluminal expansions up to $45c$ have been observed [Zenzus, Pearson book, 1987]. But of course Arp (1987) showed that redshift quasar distances are unreliable. However, even within our Milky Way (where distances are well established) there is evidence of something moving with superluminal velocity;
- 2) tunnelling photons: ever since 1992 it had been showed in Cologne that tunnelling wave packets can move with superluminal group velocities inside the barrier. The result was confirmed by independent experiments carried out in Berkeley (1993). The phenomenon had been predicted theoretically earlier;
- 3) microwave propagations: microwave pulses have been observed to propagate in open air with a velocity of about $2c$ [Ranfagni, Mugnai 1996];
- 4) X-shaped waves: Bessel pulses with an X-shaped structure have been predicted theoretically and observed experimentally [Saari, Reivelt 1997; Mugnai, Ranfagni, Ruggeri 2000]. Their superluminal velocity is $c/\cos \theta$, where θ is the cone angle of the Bessel beam (Selleri SLS 2002a, pagg 63-64).

Selleri revealed how SLS exclude retarded and advanced simultaneity, enabling only the absolute simultaneity. In (Selleri SLS 2002c), he stated that:

The causal paradox generated by superluminal signals in the theory of relativity is shown to be naturally solved if the SLS propagate in a Lorentz ether at rest in the privileged system S_0 . It is then possible to use SLS in an experiment measuring the velocity of the laboratory with respect to the ether (Selleri SLS 2002c. My underline).

But, unlikely the previous paper, Selleri proposed here an experiment to measure the absolute velocity in the laboratory:

Given two generators of SLS, we consider them identical if, when placed in the same inertial system side by side, they can generate at the same time two SLS (e.g., two X-shaped pulses) which arrive simultaneously in a detector placed at any distance in front of them.

The condition that the [two] pulses emitted [...] have the same velocity in S_0 is clearly satisfied, even if one moves parallel and the other one antiparallel to the velocity of its generator, since they propagate in the Lorentz ether independently of source velocities. [...] it then becomes possible to measure the absolute velocity of the laboratory in which such an experiment is carried out (Selleri SLS 2002c).

In (Selleri SLS 2003b) and (Selleri SLS 2006) he argued that electromagnetic superluminal signals have been detected in several experiments, and once again the causal paradox generated by SLS in the theory of relativity is shown to be naturally solved if the SLS propagate in a Lorentz ether at rest in the privileged system S_0 . There is an experimental evidence for electromagnetic signals propagating with superluminal group velocity. And, according to (Selleri SLS 2006):

The causal paradox generated by superluminal signals (SLS) in the theory of relativity does not exist in the theory with $e_I=0$. The irrelevance of SLS for the Einstein, Podolsky and Rosen paradox is pointed out (Selleri SLS 2006, pag 443).

Selleri was against the superluminal signals, in order to avoid the entanglement, to save the locality.

In FRT, considering the absolute simultaneity and $e_I=0$, he reached the outcome that the superluminal signals are irrelevant for EPR paradox. They are used only in a QM description: “In all cases EPR paradox has clearly nothing to do with superluminal propagations” (Selleri SLS 2006, pag 462).

3.7.2 SELLERI'S LINK WITH ALTON "CHIP" ARP AND TOM VAN FLANDERN

Selleri wrote his first two papers before graduating on Einstein, on the *Memorie della Società Astronomica Italiana*, and on supernovae in the magazine *Coelum*. Selleri returned to astrophysics and cosmology with Arp and van Flandern, always in his search for the truth, aimed at concretely encouraging the proponents of alternative points of view, of probable turning points that would explain the problems of mainstream physics, despotic and poor in ideas and explanations.

Arp's research is related to the evidence that the number of quasars that are associated with nearby galaxies is increasingly strong in spite of the very large redshift of many of these objects. A new redshift-generating mechanism is therefore needed. Such ideas should be considered together with the exciting evidence for redshift "quantization".

Selleri already came into contact with Alton Arp's ideas at the end of the 1980s. In a letter to Marcello Pera, Selleri admitted that he already knew Arp's work, still revealing that maybe his knowledge is prior to 1988:

[...] There is a certain Arp who convincingly argues to me that the redshifts are not of Doppler origin, and therefore that the big bang is probably all a big lie. Do you know him??⁹⁰⁰ (Selleri to M. Pera, 20 September 1988-FSPA. My underline).

One year later, in a letter to Paolo Maffei⁹⁰¹, Selleri showed to know well the Arp's approach, doing even a seminar on him, and this could be another evidence of an earlier knowledge⁹⁰²:

[...] Since things are better understood from a distance, perhaps the Arp/astrophysics ratio can be more enlightening for me than the Selleri/quantum physics ratio, given the inevitable subjective aspects that complicate the latter.

⁹⁰⁰ "[...] c'è un certo Arp che sostiene in modo per me convincente che i redshifts non sono di origine Doppler, e quindi che probabilmente il big bang è tutto una gran balla. Lo conosci?" (Selleri to M. Pera, 20 September 1988-FSPA).

⁹⁰¹ Paolo Maffei (1926-2009): Italian astrophysicist. He was among the pioneers of infrared astronomy. He linked his name to Maffei 2, an intermediate spiral galaxy in the constellation of Cassiopeia which is part of the group of galaxies dedicated to him, called Maffei 1.

⁹⁰² The original Italian text is in Addendum 3, pp. 1055-1056.

The question is this: is it possible that Arp is right, that the quasars are close, but that all astrophysicists refuse to see the light? **I hope very much that the answer is negative, I would really prefer it.** There would be the stubborn error of an individual who does not want to adapt to any price, but the average health of astrophysicists would be saved. If instead the answer were positive, given that Arp has been advancing his thesis for several decades, it would have to be concluded that we have fallen into a period of absolute obscurantism and cowardice in which no one, no matter how powerful and no matter how much he is in the highest astrophysical laboratories, dares to stand up for the truth. Because here the truth depends directly on observations and is in any case more accessible, it seems to me, than the quantum one in which theoretical and philosophical considerations obscure so many minds. **The fact is that I can say that I know Arp's arguments (I also did a seminar on them)** and I do not see what prevents their systematic destruction, if they are false, in the eyes of those who, like me, cannot forget them. Instead I find a book by Sagan that barely talks about them a little bit, to say that he might even be right! And Dallaporta in Padua was a possibilist. I am very interested in your 100% sincere opinion. **I promise not to tell anyone about it.** [...] But I would also like to understand if it is worth it or not that I continue to dedicate my time partly to Arp. Where is the truth? (Selleri to P. Maffei, 4 February 1989-FSPA. Selleri's underline, my **bold**).

Maffei answered postponing until they would see each other in person, but I have not found any development to this situation, if not in the next letter, sent by Selleri to Tullio Regge. The letter is undated but I suppose it is later than the previous one because Selleri claims to have spoken with Maffei:

But did you read Arp? I studied it. And for a whole year. That people do not accept my ideas is normal, since they are theoretical proposals. But Arp presents a large number (about 30) of independent experimental evidence. They are very convincing, with the only reservation that I did not do the experiments and I can not know if he invented them. So I have worked with several astrophysicists: Maffei, Bertola, Braccisi. Even with different nuances, there is not one who accepts Arp's conclusions.

But there is not even one who disputes the validity of the experimental data. They reject Arp with specious arguments ("he is isolated") but not on the scientific field. From these contacts I have come to the strengthened conclusion that Arp is right, and that the scientific community of astrophysicists is more closed and dogmatic than that of high energies, and much more.

They reject Arp because the reasons for the red shift of quasars are not theoretically clear if he is right. Nice way of reasoning!⁹⁰³ (Selleri to Tullio Regge-FSPA. My underline).

⁹⁰³ The original Italian text is in Addendum 3, p. 1056.

Selleri invited Arp at several conferences, as at Rimini Meeting in 1989, “in the round table substantially dedicated to Halton Arp, a name that posterity will remember more than any other astrophysicist of our century” (see page 560). And in April 1989, when Selleri was defining with Jaca Book a comparison on Arp’s advancement work of the Italian version of his book *Quasars, redshoft and Controversies*. Selleri was publishing Arp’s book in Italy, and he also going to meet him in Germany (see page 565). This could be another confirmation of a knowledge of the work of Arp really prior to 1989, see the fact that the Italian translation of book is at an advanced step. In a letter to Duffy, Selleri claimed that:

I am translating into Italian the book by Halton Arp and I am very positively impressed. [...] in my opinion he is giving a new wonderful cosmology and proofs that the “big bang” needs a general revision (Selleri to Duffy, 14 March 1989-FSPA).

In a letter to Prof. A.E. Paparodopoulos, together with the impressive statement about Selleri’s interest in Hegel and Engels dialectics, he admitted his poor knowledge of cosmology, asked for some concept, and explained in detail the Arp approach and his confinement in the scientific community, linked, according to Selleri, to his discoveries that would cast doubt on the big bang:

Thank you very much for your long, honest and deep letter. First of **all I must confess of being no expert of cosmology**. Therefore you know more and it is difficult for me to stay at your level.

[...] **I am 100% materialist, and also consider as interesting the Hegelian/Engelsian dialectics. In spite of these choices I am not interested in Big Bang from a philosophical perspective:** to put it briefly, there is always something we do not understand. A static universe without a beginning is not understandable, what came before Big Bang is also not understandable, and therefore the situation is even.

The creationists seem to me not very intelligent, considering that already Plato put God outside time: why, then, a beginning in time should be considered a proof of a divine intervention? Conclusion: I have no philosophical prejudice against your choices.

[...] My point about extragalactic astrophysics is only the following: **there are extremely interesting experimental results (mainly by Arp and Napier) showing that a great deal of objects with large redshift are near (near as some of the near galaxies) and showing that a new fantastic phenomenon - redshift periodicity - exists in the sky both for galaxies and for quasars (with different numerical content). No theory unable to accept naturally these results should be taken seriously. This is all I care about.** I am shocked to see that most astrophysicists simply go ahead as if these results did not exist. In particular the whole astrophysical establishment violently ignores them - if I may so say - obviously because they do not fit in its dogmas. This shows in the clearest way the terrible deep crisis we are passing through: most physicists do not care anymore about experimental evidence!

I often disagree with Arp, otherwise. His models of the universe seem to me too naive to be believable. But his experimental results (his observations) are another matter. Nobody says that he invented them. All experts I spoke with about Arp say that he is a good, honest physicist, but that nevertheless he is wrong in saying that quasars are near. It is impossible to understand who is right without studying Arp's results. I did that by inviting him for seminars, by having his book translated into Italian, by studying his book and several of his important papers, by inviting him to conferences (Olympia is the latest). The conclusion I have drawn is that he has about 40 independent pieces of evidence that quasars are near. The people saying that he is wrong have not devoted the same honest attention to Arp's results, they only hope that he is wrong because they are committed to a different conception and they do not want to document themselves because they are afraid to be considered traitors by their friends and bosses (Selleri to Paparodopoulos, 23 June, 1995-FSPA. Selleri's underline, my **bold**).

The relationship with Thomas (Tom) Van Flandern⁹⁰⁴ is subsequent to that with Arp and it is related to GPS positioning system and his advantages for atomic clocks and for Selleri research on FRT. The main document is related to a seminar held on 21 September 1999, "What the Global Positioning System Tells Us about Relativity". Together with the abstract, there is an introduction by Selleri:

Abstract: We present an overview of the Global Positioning System (GPS), with emphasis on what relativistic effects might be seen in this marvelous laboratory of flying atomic clocks. The observed clock rate changes are compared with theoretical expectations.

⁹⁰⁴ Thomas van Flandern (1940-2009): See: https://en.wikipedia.org/wiki/Tom_Van_Flandern; <https://ui.adsabs.harvard.edu/abs/2011BAAS...43..023D/abstract>. Both accessed 24 September 2020.

This comparison includes the isotropy of the speed of radio signals, which is expected to always be the speed of light, but in practice is not. That leads us to the concepts of “GPS clocks” and “GPS time”, and how these compare to nature’s time scale.

Once the experimental results are firmly in place, we examine the implications for understanding relativity, with somewhat surprising results. Interpretations in terms of Einsteinian and Lorentzian relativity are compared. In the end, we show how the propagation speed of gravity can resolve any remaining ambiguity of interpretation in favor of Lorentzian relativity (FSPA).

Tom received his Ph.D. in Astronomy from Yale University (USA) in 1969. He spent 20 years at the U.S. Naval Observatory, where he was the head of the Celestial Mechanics division. Now he has founded a Washington-based organization, Meta Research, which promotes research on ideas otherwise not funded simply because they conflict with the dominant theories of astronomy. He is the editor of the quarterly Meta Research Bulletin. He is research associate at the Physics Department of the University of Maryland, where he studies the accuracy of the Global Positioning System.

The North Atlantic Books publishing house published his book, “Dark Matter, Missing Planets and New Comets” (2nd edition in 1999). Like his scientific articles, the book is critical of many standard ideas in Astronomy, such as the Oort Cloud, the dirty snowball, and the Big Bang theory. Tom also leads expeditions to observe eclipses for researchers and the public. His additional interests include health, biochemistry and computers, with an emphasis on presentation graphics. One of his recent articles “The speed of gravity - What the experiments say” [*Phys. Letters A* 250, 1 (1998)] is of exceptional interest and is highly recommended for reading⁹⁰⁵ (FSPA).

3.7.3 PHILOSOPHICAL APPROACH AND RECOVERY ON BOHR’S COMPLEMENTARITY AND HEISENBERG’S INDETERMINACY RELATIONS

In the second half of the 1990s Selleri also found room for two papers of philosophical and historical interest on Bohr’s complementarity and Heisenberg’s indeterminacy relations.

In the first one, “Complementarity vs Causality in Space and Time”⁹⁰⁶, Selleri deeply analyzed how the complementarity principle is strongly related to the fundamental ideas of Danish existentialism, thanks to the seminal

⁹⁰⁵ The original Italian text is in Addendum 3, pp. 1056-1057.

⁹⁰⁶ Selleri F. (1995), “Complementarity vs Causality in Space and Time”, in *Advances in Fundamental Physics*, Barone M., Selleri F. (eds), Palm Harbor: Hadronic Press, pp. 381-397. (Selleri 1995) in the following. See there the references for the statements in the text by Rosenfeld, Jammer, and Bohr.

influence played by Søren Kierkegaard, through Harald Höffding, on Bohr view.

In order to understand Bohr's complementarity we have to start from the observation that Bohr can by no means be considered an Hegelian.

[...] Particle and wave are two contradictory elements which are clearly related to another pair of opposites, those of continuity and discontinuity. But this partial acceptance of the Hegelian views does not at all imply that for Bohr the contradictions can be resolved in higher syntheses: on the contrary, the refusal of such resolutions can perhaps be considered the most important struggle of his life! One of his pupils, Rosenfeld, so described the situation: "While the great master (Planck, Einstein, Born and Schrödinger) were vainly trying to eliminate the contradictions in Aristotelian fashion by reducing one aspect to another, Bohr realized the futility of such attempts. He knew that we have to live with this dilemma [...] and that the real problem was to refine the language of physics so as to provide room for the coexistence of the two conceptions".

It is very clear that to live with a dilemma is something very different from trying to resolve it. **The day in which I understood** (*Bohr's Complementarity in the Light of Modern Researches in the Foundations of Quantum Theory*) **that the Danish philosophers Søren Kierkegaard (1813-1855) and Harald Höffding (1843-1931) entertained exactly the same opinion about the impossibility to resolve contradiction, I thought that the nature of Bohr's complementarity had finally become clear: complementarity was imported in physics from a secondary branch of philosophy, Danish existentialism!** (Selleri 1995, pagg. 382-383. My bold).

[...] It is easy to agree with Max jammer when he writes: "There can no doubt that the Danish precursor of modern existentialism and neo-orthodox theology, Søren Kierkegaard, through his influence on Bohr, affected also the course of modern physics to some extent".

To the above one can add that the influence of Kierkegaard on Bohr was not limited to some marginal features of quantum theory, but concerned the very essential idea of complementarity.

In fact, for Kierkegaard, contradictions in life and in natural sciences are rigid and impossible to overcome. This conviction went together with sharp attacks against Hegel's rationalistic idea that contradictions can always be resolved at a higher level. On the contrary, thought Kierkegaard, contradictions are irreconcilable and constitute alternatives that mutually and definitely exclude one another (Selleri 1995, pag. 383).

If the conceptual continuity between Kierkegaard and Bohr is clear, it is more difficult to find a direct connection between them because the former died half a century before the latter entered in the university. There is however plenty of evidence of important indirect connections, the key figure being Höffding. Among the early reconstructions of Bohr's cultural background one should read a little known but important paper by Nils Svartholm (1967, 1990 in Selleri) which provided a reconstruction of the relationships between Kierkegaard and Höffding and between Höffding and Bohr. More recently there have been studies by D. Favrholt (1985), J. Faye (1991), and R.N. Moreira (1994).

The importance of Höffding's personality for his own cultural formation has been so described by Bohr in a posthumous homage (1931) during a talk at the Royal Academy of Sciences and Letters: "[I had] the privilege of having been in close contact with Höffding since my early youth, because my father was an intimate friend of him and I had, at all stages of my life, the possibility of benefiting from his true scientific and philosophical spirit".

[...] All this evidence strengthens the conclusion obtained in the present paper, that Bohr created complementarity under the influence of the philosophical ideas of Danish existentialism. By doing so he freely decided to introduce a strange new idea into physics, the idea that fundamental problems cannot be solved and that we have to live with dilemmas! (Selleri 1995, pag. 384. Selleri's underline, my **bold**).

[...] In Hegelian terms we can say that the thesis (space-time) and the antithesis (causality) can never be united in a synthesis, but must remain forever as opposed and irreconcilable. As we saw, the latter observation is essential for understanding the historical and cultural roots of complementarity.

[...] Anyway our conclusion is the following: the Einstein-Bohr disagreement arises from a different definition of reality.

As such the disagreement belongs at the same time to philosophy and to physics, and it should anyway be obvious that it is not possible to stop a rich and powerful line of thought - such as Einstein's - simply by assuming that his premises are forbidden. It is a fact of the history of modern physics that Bohr never spent a word in trying to justify his philosophical position, which he adopted under the well documented influence of S. Kierkegaard, H. Höffding and W. James. Therefore we are today completely free to adopt a different line of thought (Selleri 1995, pag. 396. Selleri's underline, my **bold**).

In the second paper, "Some Remarks Concerning Heisenberg's Indeterminacy Relations"⁹⁰⁷, Selleri showed how is possible to overcome the

⁹⁰⁷ Croca J.R., Selleri F. (1999), "Some Remarks Concerning Heisenberg's Indeterminacy Relations", *Communications in Mathematical and Theoretical Physics*, India, 2 (1), pp. 61-69. (Croca, Selleri 1999) in the following. I found only a version whose layout follows the order 1-7

relations exploiting the contradiction and analyzing some outcomes which undermine them:

Two different interpretations of Heisenberg inequalities are compared. In one of them they are simply uncertainty relations, they limit our knowledge but a priori say nothing about objectivity. In the standard interpretation they are instead considered as indeterminacy relations: in the objective reality the electron does not possess simultaneously well defined position and momentum. We show that these two interpretations are technically incompatible: if the first one is accepted, the second one cannot hold. We also review some recent results which undermine in other ways the indeterminacy relations (Croca, Selleri 1999, pag. 1. Underline in the original text).

[...] The conclusion that one cannot simultaneously measure position and momentum of an electron with arbitrarily small errors does not imply that such quantities have no physical meaning and must be considered objectively undetermined, even if Copenhagen followers claim that such entities do not exist before measurement. Moreover, one can show that the validity of the uncertainty relations still allows one to calculate simultaneous values of position and momentum *in the past* with any desired accuracy. About this Heisenberg wrote: “then for these past times $\Delta x \Delta p$ is smaller than the usual limiting value, but this knowledge of the past is of a purely speculative character, since it can never (because of the unknown change in momentum caused by the position measurement) be used as an initial condition in any calculation of the future progress of the electron and thus cannot be subjected to experimental verification. It is a matter of personal belief whether such a calculation concerning the past history of the electron can be ascribed any physical reality or not”.

Heisenberg’s own ‘personal belief’, which has since become the dominant point of view, was that one should always refrain from attributing physical reality to any retrodicted coordinate or momentum values. Fortunately, it is possible to disagree. Indeed it is possible to calculate position and momentum in the past with a precision better than Heisenberg’s relations (Croca, Selleri 1999, pagg. 1-2).

[...] It is useful to consider two different ways of looking at the Heisenberg’s inequalities (HI);

HI.1. The standard interpretation considers them as indeterminacy relations: in the objective reality before and after an act of observation perhaps there is nothing at all, but if something really exists (an “electron”) it possesses no position and momentum better defined than in [the indeterminacy relations]. The electron is actually smeared out in a volume of phase space satisfying [the indeterminacy relations];

HL.2. In a different interpretation they are simply uncertainty relations, they limit only our knowledge, but a priori say nothing about objectivity. The electron can have well defined properties, but in general we are unable to know them in a way better defined than in [the indeterminacy relations]

[...] [For Heisenberg], before measurement, position and velocity do not exist in reality. This is a philosophy with a Machian flavour, which is not even fashionable today, after the errors made by Mach have been amply recognised.

It is instead possible to adopt a realistic viewpoint, assuming that both position and velocity of a quantum system exist before any measurement, and claim that Heisenberg's relations are to be interpreted simply as uncertainty relations concerning the future, while they are violated in the past if our knowledge has been increased by a new measurement made in the present. It is then natural to conclude that as indeterminacy relations they are violated at all times (Croca, Selleri 1999, pag. 3. Underline and **bold** in the original text).

[...] Croca stressed that thanks to the new generation of microscopes the way leading to a violation of Heisenberg's inequalities acceptable by all physicists, whether realists or positivists, seems now open. In fact it is possible to show that the working of one of these non-Fourier optical microscopes violates, in practice, the usual Heisenberg's inequalities by very significant factors. It is now becoming apparent that even the statement that one cannot simultaneously measure position and momentum of an electron with arbitrarily large precision is probably not correct (Croca, Selleri 1999, pag. 5).

This approach is in line with Dirac's 1963 remarks: "I think one can make a safe guess that the uncertainty relations in their present form will not survive in the physics of the future". These results could be dangerous for the entire quantum theory, about which Feynman wrote: "The uncertainty principle "protects" QM.

Heisenberg recognised that if it were possible to measure the momentum and the position simultaneously with a greater accuracy, the QM would collapse. So he proposed that it must be impossible. The people sat down and tried to figure out ways of doing it, and nobody could figure out a way to measure the position of anything - a screen, an electron, a billiard ball, anything - with any greater accuracy. QM maintains its perilous but accurate existence".

In the light of the previous arguments it appears that Feynman's statement is justified only if one sticks to a strict positivistic philosophy (the only reality consists of acts of measurement). For a realist the completeness of quantum theory has already collapsed at least since 1935 (Croca, Selleri 1999, pag. 6. My underline).

3.8-ANY LEGACY IN FRT, THOUGH? CONTEMPORARY REACTIONS, A QUICK SUMMARIZATION AND BEYOND

Being the last topic in which Selleri made researches, and being him dead before the publishing of the English version of his last book on WR, the *summa* of his activity in FRT, there are no many sources to analyze. Anyway, it sounds useful to see the contemporary reactions to Selleri's research in FRT via some correspondence, which clearly shows that there has been a debate and the sharp ideas of Selleri encountered the interest of several scholars.

The idea of a recovering of the ether was the topic of the following three letters. After a conference in Lodz in 1992, Napier⁹⁰⁸ wrote to Selleri:

[...] I was fascinated by your talk at Lodz. [...] I had not realised that the aether question was such a live issue, but following your talk, I felt that perhaps we should update and submit the paper (Napier to Selleri, 28 September 1992-FSPA).

In 1996, Silvio Mora recalled the work of Selleri, stressing his clarity, and agreeing on the conventionality of the principle of relativity:

My interest [in the possible survival of the ether, in the compatibility of the ether with SRT] partly arose from amazement at the protests that some valuable scholars sometimes made about the solution given by relativity to the paradox of twins and partly from Ives' strange refusal to accept the relativistic interpretation of the transversal Doppler effect, which he highlighted for the first time with Stilwell, in his famous experiment that tested [also at the same time] the dilation effect of time. You posed with great clarity the problem of the search for all the possible spatio-temporal transformations equivalent to those of Lorentz but not containing the principle of relativity.

[...] as you rightly point out, the principle of relativity is based on a convention adopted on the synchronization of clocks, which is independent of the knowledge of c on a one-way path, a measure never made and perhaps not executable⁹⁰⁹ (S. Mora to Selleri, 12 May 1996-FSPA. My underline).

⁹⁰⁸ It could be Austin Napier (1947-), currently at Tufts University. There is no reference in the letter.

⁹⁰⁹ The original Italian text is in Addendum 3, p. 1057.

In 1998, Emilio Santos stated his preference to the ether with respect to the SRT itself, which he would like to keep:

I liked to know your reason for changing relativity. I consider it necessary to reintroduce the ether. I agree that the ether is necessary because the vacuum of physics cannot be imagined as a truly empty space. According to me, the TR should not be changed, but if the necessary existence of an ether implies the change of relativity, it should be changed⁹¹⁰ (E. Santos to Selleri, 6 May 1998-FSPA).

In 1999 Selleri and Yanhua Shih, who was at Baltimore working in quantum optics with PDC at that time, had a correspondence in order to interchange on some relativistic experiments:

Dear Professor Selleri,

I am so happy to learn from Augusto that you will be able to visit my laboratory soon. It is very important for me to learn your opinions about several relativistic experiments I have planned to do in the near future (Y. Shih to Selleri, 17 September 1999-FSPA)

Dear Professor Shih,

Thanks again for your friendly and highly interesting invitation. Augusto knows that I am very much interested in your physics (Selleri to Y-Shih, 24 September 1999-FSPA).

Jammer experienced a long-standing relationship and correspondence with Selleri, as I showed in the whole dissertation.

In a 1996 letter, Jammer asked for some information on Selleri's proposal:

Indeed, I am interested in what you call "additional curiosities", namely in clock synchronization and relativity, in fundamental questions in quantum physics and relativity. The reason: I am still planning to write a book on "synchronization" (Jammer to Selleri, 7 November 1996-FSPA).

Selleri answered after two weeks, sending him several papers. In them, he revealed his reliance in the Inertial Transformations, explained the value of synchronization and some outcomes of his researches, and also pointed out

⁹¹⁰ The original Italian text is in Addendum 3, p. 1057.

once more the importance of history in physicists' education. Finally, he recalled the role of Tangherlini's Transformations in his research, still defining them as a case of his IT :

Dear Prof. Jammer,

Thank you for your interest in my papers. The two dealing with synchronisation are joined to this letter. I am very interested, as usual, in your work: if you write something on the subject, let me know, please. Your books are of enormous value to people active on the foundations of modern physics: in times in which history has been forgotten by most physicists it is impossible to do any meaningful research without going back to the real problems agitating the scientific minds at the times in which the contemporary theories were built. You have given us history of physics in the deepest and best documented way and we are very grateful to you for learning so much from your work.

Please pay attention to the following: the choice of synchronisation is not really free, nature forces us to adopt a well defined synchronisation, the absolute one. We have been distracted up to now, and have become blind because we liked special relativity and its successes. However the above conclusion is scientifically inevitable. **The right transformations between inertial frames are not the Lorentz transformations, but those I called "inertial", which have the Tangherlini transformation as a particular case.**

If you want to convince yourself pro or against **the above statements (they are of course "very strong", but I cannot avoid making them to you, since I consider them logically inescapable)** you can consider the following:

- 1) All the paradoxes of SRT disappear with the new transformations, which are equally (or more) successful in explaining the experimental evidence;
- 2) The Sagnac effect can be explained from the point of view of the observer on the rotating platform only by using the inertial transformations;
- 3) SRT is literally "out of the physical world" because the one-way speed of light can be proven to be noninvariant in all but one inertial frame: see the new paper NONINVARIANT ONE-WAY SPEED OF LIGHT AND LOCALLY EQUIVALENT REFERENCE FRAMES, especially the figure in the last page which is very important;
- 4) The argument of the latter paper I have already extended from rotating frames with constant peripheral velocity, but acceleration decreasing to zero, to linear accelerations (also decreasing to zero). I am writing this new paper which I will send to *Phys. Rev.*, something I did last time 28 years ago, after publishing a dozen papers on PR (3 on PRL) in the beginning of my career ("One Pion Exchange model", ever heard?).

I hope you will devote some thought to these matters, and that you will not discard them as meaningless!

Making progress in fundamental matters is not so difficult, after reading books and papers like yours. What is almost impossible is convincing other physicists that the progress exists.

If you had suggestions, I would listen carefully. But, of course, do not worry too much.

With very best regards. Yours sincerely (Selleri to Jammer, 20 November 1996-FSPA.

My underline and **bold**)

Anyway, for the following years, Jammer did not write on the topic, perhaps revealing a few interest in Selleri's proposal, once he read the details. I did not found any answer by Jammer, still confirming his scarce interest in these FRT topic as developed by Selleri. Also this is an element in order to better understand the opportunity of a legacy in Selleri's activity in FRT.

But, in 2004, Jammer actually wrote a relevant paper submitted and dedicated for the special issue of *Foundations of Physics* for Selleri's seventieth birthday, confirming the relationship and the esteem with them. "The strange story of the concept that inaugurated modern theoretical physics"⁹¹¹ dealt with RT and the problem of synchronization. Jammer recalls that in 1983 he was in Bari, invited at the conference by Selleri, and that he had a talk on this subject. He wrote in the abstract:

"The strange Story of the Concept which inaugurated Modern Theoretical Physics" is the title of a lecture which I delivered on the invitation of Professor Franco Selleri at the University of Bari about 20 years ago. Since Professor Selleri himself has written several interesting papers on this concept and since the centennial of the birth of modern theoretical physics will be celebrated soon, I found it appropriate to dedicate this essay, containing so far unpublished critical and historical comments on this concept, to Professor Selleri on the occasion of his 70th birthday. It should be emphasized that the critical comments in this essay are not intended to question the validity of the theory initiated by this concept, a theory which in the realm of its applicability is daily corroborated by numerous experiments in high-energy laboratories all over the world.

Although these critical remarks refer almost exclusively to the first publication of this theory, a publication which has been hailed as "the most important paper written in the 20th century," they discuss profound problems of general importance for the study of the foundations of physics (Jammer 2004, pag. 1617. My underline).

⁹¹¹ Jammer M. (2004), "The strange story of the concept that inaugurated modern theoretical physics", *Foundations of Physics* 34 (11), pp. 1617-1641. (Jammer 2004) in the following.

The following two emails were both written by Selleri in 2004. The perception given by them is that Selleri seeks a comparison, better, a support from the great physicist and philosopher of science to his ideas. They all deal with the explanation by Selleri of his work in FRT. They were written in two days. In the first he stated:

Dear Prof. Jammer,

Thank you very much for sending to Alwyn van der Merwe your beautiful paper “The strange story of the concept that inaugurated modern theoretical physics”.

It is almost useless to say how proud I am to have your contribution in my *Festschrift*, and in fact I asked Alwyn to place it as first paper in the first one of two issues of this collection. I am doubly proud after reading it and seeing that it goes to the oldest root of modern theoretical physics. I never thought of it in that way, but after reading your paper it becomes obvious and natural: the Einstein synchronization of clocks IS the very beginning of it all, with its jump away from the attempt to tie the ideas of theoretical physics to the objective reality. Heisenberg was right in recalling this aspect of the relativistic theory as the conceptual forerunner of quantum theory.

Your paper raises my responsibility toward the research I am presently carrying out, as I am working precisely on that point, the assumed conventionality of the relativistic synchronization of clocks.

With gratefulness and great respect I send you my best regards, (Selleri to Jammer, 4 October 2004-FSPA. Selleri’s CAPITAL, my underline and *Italics*).

In the second email, Selleri referred to another paper written by Jammer in 1979 on FRT and presented at Varenna (although a Varenna Summer School on “Problems in the Foundations of Physics” was held in 1977, the proceedings appeared maybe in 1979), but Selleri knew it only at the end of 1990s. This email is a significant document for the deep analysis made by Selleri of his research as well as for the difficulties he met and that I have explained in the early section. It is a sort of cultural heritage of his approach in FRT. His book on WR will be published seven years after in Italian:

Dear Professor Jammer,

About 5 years ago a physicist in Tehran gave me a copy of your 1979 Varenna paper on the foundations of relativity.

[...] The reading of your paper gave me happiness and strength to continue my research on the physics of space and time (I am not exaggerating). It is a great piece of work. By that time I had already understood that the relativistic description of time is dialectic (contradictory in the good sense). It contains objective features with the retardation of moving clocks, and conventional features with the synchronization of distant clocks. It was obviously interesting to see whether a different convention gave a better description of the physical reality, especially after reading in your paper that a different value of the Reichenbach parameter could not lead to disagreement with experiments. From 1994 to 1999 I traveled a lot and discussed with many people. In this way I learnt something that it is very difficult, if not impossible, to read in the scientific literature: the relativistic theories are unable to explain the Sagnac effect.

Let me tell you how I succeeded in explaining the Sagnac effect. I had reformulated the transformations of the space and time coordinates between inertial reference frames by assuming only two empirical “facts”: (1) The two way velocity of light is invariant; (2) Clocks moving with respect to the privileged (isotropic) system undergo a retardation by the usual velocity dependent square root factor. This was enough, together with more trivial assumptions, to deduce transformations (which now I call “equivalent transformations”) containing a free parameter e_1 , the coefficient of x in the transformation of time. The remaining three equations of these transformations are identical with the corresponding equations of the Lorentz transformations. The parameter e_1 is simply related to the Reichenbach coefficient ϵ .

Well, it was obviously interesting to apply the theory to the rotating platform and to the Sagnac effect. The result was fantastic, only the choice $e_1 = 0$ gave a satisfactory explanation of that effect on the rotating disk. This meant that the equivalent transformations had to specialize and become the “inertial transformations” based on absolute simultaneity. In fact, think a moment: if time transforms like this: $t = t_0 R$, two events in different positions at the same t_0 will take place also at the same t .

A byproduct of this research was the discovery of a relativistic discontinuity between inertial systems and slowly accelerated systems.

The discontinuity goes away only if $e_1 = 0$. Its existence is the root of the terrible problems met by Langevin, Post, Landau-Lifschitz, Anandan, [...] in trying to explain with relativity the physics on a rotating platform.

Since then I found several other good reasons to adopt $e_1 = 0$, the “absolute synchronization” of Mansouri & Sexl. I only list them here, ready to give you more details, if you so wish.

The relativistic “explanation” of aberration works very badly because it is evident that the phenomenon does not depend on the relative velocity star-Earth, as explicitly assumed by Einstein in his 1905 article. With $e_1 = 0$ it should instead depend on the Earth absolute velocity and everything is explained.

The clock paradox was never given a satisfactory description in relativity (despite many opposite claims), while the theory with $e_1 = 0$ describes it perfectly and non paradoxically.

The growing evidence for the existence in nature of superluminal signals can easily be accommodated in the theory with $e_1 = 0$, while it is incompatible with relativity due to the presence of the famous causal paradox in which events belonging to the future of an observer can actively modify the past of the same observer.

All the paradoxes of the special theory of relativity disappear in a theory based on the inertial transformations. The only exception is the Ehrenfest paradox of the Lorentz contraction applied to a rotating disk, but this one has a different nature.

The result of all this is that I am firmly convinced that the good theory of the physics of space and time is the one with $e_1 = 0$ and not relativity.

I reached this conclusion nine years ago and see no reason whatsoever to negate it. I know how difficult it is to make people accept such a statement. It is probably impossible, but you see, I am curious and this is the reason why I became a physicist. My curiosity is now partially satisfied. We live in a world in which there exists a privileged inertial system which is physically active. We live in a world with the Lorentz ether. By the way, do you think that Lorentz would have been ready to give up his transformations to recover his ether? I tend to believe so.

Happiness can never be complete. To the above rosy picture I must add another piece of evidence of a different color. The situation becomes dialectic once more, but in a new way. The question is of course: how to detect the privileged system? After many attempts (all failed) I am now preparing a paper in which the impossibility to detect the ether is explicitly demonstrated. There is a well defined way to resynchronize the clocks of the universe (that is, the clocks of all inertial frames) which leads from the given privileged system to another, arbitrarily chosen, privileged inertial system, where the privilege is the isotropy of space, e.g. with respect to the propagation of light. It is a theorem (consequence of the assumptions of the theory of the inertial transformations) the existence of which was intuitively understood already by Mansouri and Sexl. **Relativism thrown out of the door comes back from the window. The best name to describe my theory sadly becomes “weak relativity”.**

In a way, this theorem demolishes the results I thought to have obtained with the previous developments. I said that there is a privileged system, where the ether is at rest. Now I say that it cannot be detected. **Can we believe in the existence of something that will never be observed by anybody? I cannot, and this is the only part of the positivistic philosophy that I accept without difficulty.**

The way out would be to say that one of the assumptions behind the theory of the inertial transformations is wrong. **The best candidate would then be the invariance of the two way velocity of light. If this invariance is broken, even very very slightly, then the privileged system becomes detectable.**

It is a matter for experimentalists. If I ask those of the past the situation is very confuse, with Miller, Kennedy-Thorndike, Jaseya et al. detecting an absolute velocity of about 10 km/sec and contradicting Michelson-Morley and others. That figure follows from the application of the Galilei transformations, but easily becomes 300 km/sec if slightly broken inertial transformations are used (with the same experimental data). If I ask the contemporary experimentalists I am told that the two way speed of light is invariant with the fantastic precision of 10^{-15} . If one accepts this figure, all the evidence collected by Miller, Kennedy and Thorndike, Jaseya et al. becomes pure illusion.

Dear Professor Jammer, I hope that this tale of my troubles was not too boring for you. If you feel like telling me your opinion I would be delighted to listen very carefully.

I remain, Sincerely yours (Selleri to Jammer, 5 October 2004-FSPA. My underline and **bold**).

Definitely a passionate and at times tragic admission, scientifically speaking.

It is crucial that Selleri reveals that he reached the cornerstones of his theory on WR as early as 1995. Here he is still looking for a solution, which he will not reach, as can be seen from what he expressed in his book on WR and which I mentioned in the previous sections.

Selleri published in 2004 a paper of *Foundations of Physics Letters*, “Curing One Discontinuity with Another: Comment on S.K. Ghosal et. al ‘On the Anisotropy of the Speed of Light on a Rotating Platform’”⁹¹² as an answer to a paper regarding the rotating platform and The Sagnac effect, still confirming the presence of a debate in outstanding journals, actually:

A recent paper by Ghosal, Raycheuduri, Chowdhury and Sarker (GRCS in the following) discussed a seven-years old argument in relativity concerning the speed of light as measured by an observer on board a rotating disk.

The argument, put forward by the present author, contains the theoretical proof of an anisotropy in the speed of light in a reference frame commoving with the edge of a rotating disk even in the limit of zero acceleration.

⁹¹² Selleri F. (2004), “Curing One Discontinuity with Another: Comment on S.K. Ghosal et. Al ‘On the Anisotropy of the Speed of Light on a Rotating Platform’”, *Foundations of Physics Letters*, 17 (6), pp. 599-606. (Selleri Curing, 2004) in the following

This conclusion challenges the internal consistency of the relativistic theories and undermines the basic tenet of the conventionality thesis of relativistic simultaneity. GRCS believe to be able to resolve the issue by recasting the original argument in the Galilean world and thereby exposing “the weak points of the reasoning leading to the fallacy”. We will defend the original argument and show that the treatment proposed by GRCS is as bad as the illness it asserts to cure (Selleri Curing, 2004, pag. 599).

Actually, Selleri, perhaps to attenuate the last sentence in the abstract, enhances the article by GRCS, and also highlights the topics in agreement, then goes on to scientifically explain the disagreements:

In spite of a [purely scientific] disagreement, to which the main part of this letter is devoted, there is much on which we agree in the otherwise excellent GRCS paper:

- (1) the statement that our discontinuity argument (“Noninvariant One-Way speed of Light and Locally Equivalent Reference Frames”) has so far met with evolving but inadequate responses (Rizzi, Tartaglia, Serafini, Minguzzi ecc);
- (2) the conclusion that the Sagnac effect (Sagnac, Post) cannot be a consequence of a “desynchronization” of clocks on the rotating platform;
- (3) the value judgment in favour of the absolute synchrony over the standard relativistic one, “unless one seeks to indulge in a double standard”;
- (4) the idea that recent experimental observations lead one to infer that the speed of light in vacuum is anisotropic for some inertial observers (Selleri Curing, 2004, pagg. 599-600).

Selleri, after explaining once more his concepts on the rotating disk, introduces the GRCS objection, based, according to him, on classical Galileian physics. They spoil the equality of the one-way speed, resynchronizing only a clock. But this single clock should mark two different times. “Hardly a satisfactory solution”, as Selleri expresses. The above set is then passed by a rod to the rotating disk in a qualitative way, still preserving the symmetry of the circumference. The velocity measured by two adjacent clocks could be the same, but GRCS stated that it is also possible choice any synchronization, still deciding for a resynchronization the clocks in such a way that the local velocity of light is the same everywhere along the disk border, but is different from the global velocity. And Selleri disagrees a lot.

Always remaining in the framework of the standard relativistic formalism, they introduce a useless complication.

The foregoing example was evidenced only in order to demonstrate the presence of a debate.

The seminal interview made with Freire, always gives many cues. Selleri explain his approach to Freire. The willingness in doing this interview is maybe the best confirmation to an interest in Selleri's activities.

But this interview was made in 2003, a year before the confessions of the weakness on the WR made to Jammer:

You see, in the foundations of relativity it becomes very clear that the theory of special relativity is based not only on experimental evidence but also on human conventions. There is a very important element of conventionality in the definition of simultaneity. That is to say, the time of special relativity should be seen as dialectical, that is contradictory in an Hegelian sense

[...] Poincaré, Reichenbach and Jammer are the people who have stated most strongly that the choice of having the one way velocity of light equal to c is a convention. And eventually I found that also Einstein agreed on conventionality. For example in the 1905 paper there is just one statement, but very clear. He says, more or less "By assuming the universality of the velocity of light we are not making an assumption about the physics of light but we are establishing a convention." So, you see, Reichenbach wrote a book entitled *The Philosophy of Space and Time* in which he introduced a parameter — called Reichenbach parameter - "epsilon". When the Reichenbach parameter is one-half you have special relativity. But all the values between 0 and 1 are possible. You have in principle theories that work as well as special relativity but are different, philosophically different from the theory of special relativity. So what I did was to - I was influenced by Prokhovnik and his books and by John Bell who wrote an article on the foundations of special relativity.

By other people as well. And what I did is to check if that was possibly true, I mean that you could develop theories which are different from special relativity and still fit all the experimental data, for example the Michelson-Morley experiment. And yes, it is true. You have an infinite set of theories. Working always on the experimental side I could explain Romer, Fizeau, Doppler, Michelson-Morley, and so on. Many experiments can be explained by these theories.

But then if you really wonder whether really it must be so, I mean that all possible experiments are fitted equally well by these theories or not, then you find that there are exceptions. Interesting exceptions are experiments with superluminal signals. There is considerable experimental evidence that they exist nowadays.

Other exceptions are phenomena with accelerations. Phenomena in which accelerations play a role, for example the Sagnac effect.

And so if you really want to explain the Sagnac Effect you have to take a different theory. You have to take a value of the Reichenbach parameter different from that of relativity. You cannot use $\epsilon = 1/2$. You must take something else. In this way you find that the right theory is the one assuming absolute simultaneity. Meaning that simultaneity is the same for all inertial observers. In such a theory there is a privileged system and the privileged system is that one in which Lorentz's ether is in rest. I must say from a logical point of view that this choice is not only possible, it is obligatory. Logically, strictly scientifically. Sociologically it is and will be very different, as the scientific community tends to be rather dogmatic on such matters. I expect my ideas to be accepted by very, very few. But, I repeat, from the scientific point of view it is obligatory to accept the Lorentz ether today (AIP 2003. My underline).

In the following years Selleri's research on the Sagnac effect was known as "Selleri's paradox". After Ghosal et al., I found "Ways to resolve Selleri's paradox", released in 2012 on arXiv by Klaus Kassner, at the institute of Theoretical Physics in Magdeburg. Sort of review paper, with no great outcomes, but always illustrative of a certain debate. As a confirmation to a debate in Selleri's research, in two letter to Michele Russo, Selleri stated:

my ideas provoke furious reactions from all sides, for example there is an ongoing debate on the Internet, with dozens and dozens of speeches, on what they call the "Selleri paradox", which would then be the matter of the discontinuity between rotating platforms and inertial systems of the SRT (Selleri to Russo, 25 Agosto 1998-FSPA).

I would like to point out that I did not start with a precise aim (as it could be to demolish the big bang). I merely understood that the synchronisation of Einstein's clocks was a convention, not a fact of nature. Albert himself says so in his 1905 article on relativity. Then I built the set of different theories only for the convention on the speed of light. Then I realised that they were not perfectly equivalent and that the one that contained the written transformation of time was better than the others, for three very good independent reasons. Everything else resulted as a consequence⁹¹³ (Selleri to Russo, 12 June 2002).

In the research programme he submitted in 1998 to the University of Bari in order to receive funds for his research, Selleri stated that:

⁹¹³ The original Italian text is in Addendum 3, p. 1057.

concerning the FRT, the starting point is the presence of a persistent difficulty of the existing relativistic theories in explaining the Sagnac effect, if the description is attempted on the rotating platform. Naturally it is not enough that the correct description of this phenomenon be given from the point of view of the laboratory observer, a consistent theory should be able to describe it at least from the the point of view of the observer at rest on the platform. There are incredible descriptions of the phenomenon (Anandan...), based on the idea that the length of a circumference depends on the direction (clockwise or counterclockwise) of its measurement process. It is obviously very unsatisfactory that ideas of this type find place on the best journals, but their existence is at least useful in pointing out the depth of the problem we are facing (AGAB-Progetti di ricerca-Richieste finanziamenti ricerca scientifica dip di fisica armadio D settore 22 palchetto 4 busta 1).

Regarding the beginning of Selleri's activity in FRT, he declared, in his Italian version on Weak Relativity in 2011⁹¹⁴ that: "I personally started working on relativistic issues approximately in 1994"⁹¹⁵. But, as for his reminiscence of his encounter with d'Espagnat's book on QM, my research has shown that the situation is a bit different.

I revealed in chapter 2, also using charts, how his first two papers in FRT (Selleri Ether 1990a and 1990b) were released in 1990 and how he kept on researching both in FQM and FRT from 1990 to 2003. Nevertheless, I emphasized how, from the previous sources I submitted, his commitment with FRT was born before, with a first sight in 1983. Indeed, I think that the Popper's disappointment showed at Bari conference in 1983 (see pag. 544) played a key role in Selleri's willingness to challenge the matter. Then, another step could happen in the same 1983 conference in Bari, with the talk of Jammer on SRT, even though Selleri recognized its value several years after. Another step was in 1985, with the course in SRT made in Europe. Even the meeting with Ludwik Kostro, who discovered the letter of Einstein with the new concept of ether could have had a role.

⁹¹⁴ Selleri F. (2011), *La relatività debole. La fisica dello spazio e del tempo senza paradossi*, Milano: Edizioni Melquíades. (Selleri 2011) in the following.

⁹¹⁵ io ho personalmente iniziato a lavorare sulle questioni relativistiche all'incirca nel 1994 (Selleri 2011, pag. 73)

According to Kostro himself and Barone, Selleri and Kostro met in 1986 at Bridgeport, in the USA, at a conference⁹¹⁶. In this respect, Barone stated:

[...] when Selleri began to take an interest in relativity is perhaps when he met Kostro who had invited him in 1986 to Bridgeport and/or the following year to Gdansk.

But Selleri told me, afterwards, that the one who really persuaded him that the foundations of restricted and general relativity had to be revised was Prokhovnik. I remember that I have this long fax that Prokhovnik had sent to Selleri, in which he answered many questions. But this fax is fading. But it was this episode that he told me that really pushed him to study relativity. And Bell too, yes exactly. Bell's article is in the book Speakable and Unspeakable.

From there he started evaluating the problems and came out with the work on FRT. When the Weak Relativity book came out, I said to him, "But what was the point of all this work?"

He did not dispute the RT in its entirety, perhaps mindful of the attacks that others had used against Einstein, Selleri never said he was against Einstein.

[...] I agree with the evidence in the book about WR! It is evidence that he keeps, that he has updated continuously. I was one of the first to work with GPS, in Russian oceanographic expeditions. And they had GPS, not the current one. They used Sputnik but they had the dynamic positioning system. So I started to study a bit more closely. I realized that the watches on satellites have an offset, when they start from the ground, which already takes into account the signal delay and therefore the synchronization is only done in this way. No offset, no synchronization. Now, how many physicists know about this? If you go to telecom magazines it's obviously known. And this thing makes you think of an absolute reference system. This frame was identified by Franco with fixed stars, but it can be identified with many things, the Background Cosmic Radiation, the sea of neutrinos, the dark energy, and so on. But it exists, it is proven. And so Franco, once he tried this, gradually found the transformations of Tangherlini, who had made them a long time ago. They knew each other, they were colleagues in Bologna. And then Selleri came to inertial transformations, as he called them (Interview with Michele Barone Bitonto (BA), 2 June 2019. My underline).

Looking at all the letters I submitted, a year between 1987 and 1988 could be a good proposal I made here for Selleri's real beginning in FRT activity.

⁹¹⁶ **1986**, 23-27 June, Bridgeport (CT), U.S.A.
NATO ADVANCED RESEARCH WORKSHOP-QUANTUM VIOLATIONS: RECENT & FUTURE
EXPERIMENTS & INTERPRETATIONS,
Org. by: University of Bridgeport
Selleri's Talk: Amplification of empty quantum waves

SUMMARY AND CONCLUSIONS

SUMMARY, OUTCOME, AND POTENTIAL INSIGHTS

In my dissertation, I have tried to show Selleri's contribution to the debate on PP, FQM and FRT. In the three chapters on his research, I took into account a lot of correspondence and endorsements by many scholars all over the world.

We have seen how Selleri's lines of research and approaches have stimulated many different groups to carry out experiments, as well as to continue his research. And he influenced many ordinary people to create their own ideas to submit to him and to get excited about his books.

Selleri's importance has been noticeable in his wide and very diversified work, and in the countless insights and quotations of the contemporary scholars, as well as in a sort of legacy related to his studies.

Looking at a correspondence sent by Olival Freire Jr, and Joan Lisa Bromberg to Onofrio Erriquez, and by Spencer R. Weart, and R. Joseph Anderson to Onofrio Erriquez himself the importance of Selleri's researches and the need to find a host to preserve his private papers are clearly evinced. This overview is aim at revealing how deep and far-reaching has been his presence in the international scientific debate. The correspondence is in FSPA:

Baltimore, USA, and Salvador, Brazil, 15 December 2003

Dear Dr. Erriquez,

We are two historians of physics working on the history of research on the foundations of quantum mechanics in the second half of the 20th Century. As you know, one duty that historians assume is that of identifying relevant historical materials and ensuring that they are preserved for the use of other scholars. One important category of historical records is the private papers of active scientists.

Last July, one of us (OFJ) interviewed Dr. Franco Selleri as part of our effort to record oral histories of protagonists in the field of foundations of quantum mechanics. The interview will be transcribed and deposited at the Center for History of Physics at the American Institute of Physics, USA. At that time, Dr. Selleri gave us a small sample from his private papers: 24 letters between himself and the philosopher Karl Popper, and some correspondence with the American physicist Leonard Mandel. We judged those letters to be important and concluded that Professor Selleri's papers should be examined and archived, in whole or in part, when he no longer needs them.

This is the reason we write you. Since the best place to preserve the private papers of a scientist is the library of the institution in which he worked, we would like to ask you if the Library of the Physics Department would be interested in taking this collection. If, additionally, the University of Bari could, with the help of an archivist, create a catalogue of the papers, it could be put on the website of the Center for History of Physics, so as to be available to historians everywhere.

We would be pleased if you could let us know if such an initiative would be possible.

Joan Lisa Bromberg

Olival Freire Jr.

December 18, 2003

Dear Dr. Erriquez,

We are writing at the suggestion of Joan Bromberg and in support of the letter that she and Olival Freire sent you recently regarding the papers of Dr. Franco Selleri. We have been pleased to be able to support some of the historical research that Bromberg and Freire have done on interpretations of quantum physics, and we were especially pleased to learn that in interviewing Dr. Selleri they found valuable correspondence which will be of interest to historians and other researchers. We believe that the best place for a scientist's papers in his home institution, since it is there that scholars will first seek a scientist's papers, and there that they will find administrative records of the institution, papers of colleagues, and related materials which will provide a well-rounded view of the scientist's work and the atmosphere in which it was carried out. Accordingly, we hope that the Physics Library of the University of Bari will be able to preserve Dr. Selleri's papers permanently and make them accessible to historians of science and other researchers. We are transcribing the oral history interview that Dr. Freire completed with Dr. Selleri, and our policy is to provide a copy of the transcript of interviews to the repository that houses a scientist's papers. So we will plan to send you a copy of the interview if the Library is able to preserve his papers.

If, on the other hand, the Physics Library does not have the facilities to preserve Dr. Selleri's papers and make them accessible to researchers, there may be other repositories that we might contact. We would be glad to receive your advice and suggestions.

Spencer R. Weart

R. Joseph Anderson

After this dutiful premise, I am going to sum up some steps and outcome I have analyzed during my research and that seem to me attractive for a possible enlightenment of the historical role played by Selleri in Physics, as well as in a better understanding of his character:

Introductory Notes

- I have outlined some hypothetical reasons behind Selleri's return to Italy after his experience in the USA.
- I have given some probable causes for Selleri's move from the University of Bologna to the University of Bari.
- I have reported on his efforts to achieve a full professor position through national contest.

Particle Physics

- I have investigated Selleri's thesis project, which dealt with calculating the spectra at an incident pion kinetic energy of 960 MeV. His thesis was a research on pion-nucleon interaction and the isobaric model.
- I have analyzed his stays at CERN in the period 1959-1961 and the active partnerships with Francesco Bonsignori and Ezio Ferrari. At CERN Selleri developed the Peripheral Model with Bonsignori, that is, a model consisting essentially "in neglecting both direct interaction and more of one pion exchanges between the incoming particle and the nucleon". And one of his idea, namely the One Pion Exchange Model, according to which "in some inelastic processes really what is going on is that one pion is exchanged. For example, if you have two protons colliding and producing one pion, let us say a positive pion, you have in the final state a proton, a neutron and a positive pion. The idea was that the target proton (for example) emitted a virtual positive pion, becoming a neutron, and that the incoming proton scattered elastically on this virtual pion producing it as real in the final state".
- I have introduced his papers on the nonconservation of the angular moment in weak interactions, a model he proposed to overcome some bugs in PP. This model represented a theme that created a great deal of interest in the world even by important scholars. I showed the huge correspondence with Emilio Segrè, Carlo Rubbia, John S. Bell, as well as some evidences recollected in some interview.

- I have also analyzed the issues that led Selleri to abandon the PP field and turn to FQM, and the crucial importance of the book by Bernard d'Espagnat, *Conceptions de la Physique Contemporaine* in this changeover. I was also able to find the precise period in which Selleri read the book and the subsequent landscape for his switch.
- I have researched in current debate a track of his legacy in PP. His work has always been the subject of interest and debate by several scholars. His letter on the The Aztec pyramid of elementary particles theoretical physics influenced a debate in the early 1970s.

Foundations of Quantum Mechanics

- I have widely pointed out how Selleri's acquaintance with Bernard d'Espagnat's fundamental book, *Conceptions de la Physique Contemporaine*, most likely occurred in 1966 and not in 1971-72, as Selleri himself mentioned.
- I have suggested a possible knowledge of QM by Selleri probably dating back to 1958, and in any case to 1965 and 1966. At any rate, prior to Selleri's activity in FQM which started with publications in 1969.
- I have found the deep link between his scientific activity and his social and political one. With several confirmations, I have shown how Selleri's political and philosophical beliefs were the foundations of his choices in physics research.
- I have described his key philosophical approach based on Local Realism, consisting on locality, reality, and time's arrow, as well as his interest for Georg Wilhelm Friedrich Hegel and Friedrich Engels.
- I have showed Selleri's early papers in this field, in which he has investigated all the current topics of his time, from the Hidden Variables to EPR paradox and Bell's inequalities, to the wave-particle duality and Empty Waves.

- His stay in Nebraska during 1970 and 1971 played a key role in his political awareness, providing a crucial basis of Selleri's approach to FQM, as well as a tremendous push towards a full employment in the FQM, already launched by Varenna 1970.
- I have emphasized his correspondence with Louis de Broglie inherent Selleri's proposal of an experiment with neutrinos in order to detect Empty Waves.
- I have dealt with Selleri's approach during the 1970s and 1980s in Italy, a period full of social clashes and political strong activity. I have evidenced his intense political activity and his peculiar activism in communicating a science to everyone, believing in the dialectical materialism and in the social responsibility of the scientist.
- I have revealed the key role played by Selleri as a counselor in the Steering Committee of the Italian Physical Society, still submitting all proposals he made and his contribution to the sensitive resolutions taken by the Society in those unusual years. Among the many inputs given, it is worth mentioning here that Selleri was undoubtedly the promoter of the Varenna Summer School on FQM in 1970 and on History of Physics in 1972, the seminal turning point for the widening of researches on FQM. He proposed and supported the withdrawal of the NATO funds to Varenna Summer Schools. He took care to bring proposals to solve the problems of the SIF magazine *Il Nuovo Cimento*.
- I have underscored how Selleri's proposal for Varenna 1970 was prior to the role played by Marcello Cini, whose role in the decision is instead overestimated by several scholars. Cini received the task of reading the book by d'Espagnat, already known by Selleri, and to report on it at the following meeting.
- I have underlined the role of Selleri as Secretary at the 1970 Varenna Summer School and his relationship with d'Espagnat, the Director of the School.

- In the SIF archives I have examined the actual presence of Bohm and de Broglie in 1970 Varenna Summer School, comparing my research with some ideas put forward by some scholars.
- I have introduced the activity of Selleri specifically in EPR and Bell's inequality after Varenna, when Selleri started researching in FQM with a lot of papers and the proposal of experiment, as well as his opposition to the Aspect's outcomes.
- I have analyzed a lot of his papers and the outcome he found, i.e., the numerous proposal of experiments in order to reveal the Empty waves, or to use kaon pairs in order to figure out between QM and LR. And also his introduction of many other Bell's inequalities, and the difference between strong and weak ones. And finally, on Selleri's priority for locality over determinism.
- I have mentioned a notebook and a travel notebook with a lot of interesting statements made by Selleri on Local Realism, the landscape of his times, the travel he made for conferences and meeting and the scholars he met.
- I have highlighted the tireless and intense activity which Selleri had in the 1980s, with a lot of dissemination papers on social topics, some remarkable books well reviewed, many conferences attended and organized, with the presence of the main scholars from all over the world, as Popper, for instance, and his coming to Bari; and the collaboration which were born from these meeting.
- I have described the probable role played by Selleri in the activities of the Italian Union of Scientists for Disarmament (USPID in Italian).
- I have brought attention to how Selleri released several books and papers on environmental analysis, peace issues, against obscurantism and irrationality in science. In addition, Selleri was planning to publish books he had already written on the Church's crimes against the free thought, and on Greek science.

- I have examined the Selleri route for the full professorship at the Bari University, the creation of a FQM group with several scholars who developed different researches, still collaborating with other scholars in the world (Vigier, Mandel, Shi, Monken, Croca, Santos, Moreira, and so on). I have also illustrated the activity of the group created by Selleri in FQM (which I christened 'The Group') and the difficulties faced in the relationship with the other research lines in Bari.
- I have broadened this view to the difficulty, still present in the 1980s, of being able to do FQM research in Physics Institutes and Departments around the world, specifically in the USA.
- I have specifically emphasized the research that Selleri made on Empty Waves during the 1980s with the attempts to reveal an effect of a real wave, still proposing a lot of papers and experiments, trying to perform them and also attending on Mandel's experiments.
- I have shed light on the effective role played by Garuccio in the paramount paper known as (Garuccio, Vigier, Popper 1981), really diverse by the familiar one.
- I have shown the Selleri remarkable approach to EPR in particle physics with the studies of neutral kaon pairs and the opportunity to perform an experiment in order to discriminate between QM and LR at Frascati, Stanford, or Japan.
- I have also emphasized the potential role played by Eberhard in the final execution of the experiment, as well as Selleri's desperate search to find a laboratory where the experiment could be carried out. Finally I have I set out the likely grounds for the failure of the experiment.
- I have investigated its passage to the FRT, drawing the precise period of time (probably date back to the years between 1985 and 1988, even if Selleri published his first paper in 1990) and analyzing the reasons behind this shift.

- By analyzing Selleri's publications and using statistical diagrams, I have made a statistical distribution of Selleri's papers during the years, in order to show how his two switchover he experienced (from PP to FQM, and from FQM to FRT) were differently spread in time. Notably, the PP-FQM switchover happened quite clearly in 3-4 years; whereas the FQM-FRT switchover took more than 12 years and was always quite "diluted".
- I have searched for a Selleri's legacy in FQM, finding the great modernity of his ideas, with the opportunity to perform a new potential experiment in Japan with neutral kaon pairs; or his influence in several scholars.

Foundations of Relativity Theory

- I have pointed out the historical opposition to the two theories of Relativity, the SRT and the GRT, also showing the Italian peculiarity.
- I have underscored the early influences that Selleri received by H.A. Lorentz, Builder, Prokhovnik, Cavalleri and Bernasconi, and Bell in his research on FRT.
- I have stressed Selleri's research for some transformations alternative to Lorentz transformations, by reintroducing a symmetry between space and time coordinates, his path paved with a new concept of ether, the absolute reference frame, the one-way velocity of light, and the absolute synchronization of moving clocks, against the conventional choice of the Special Relativity.
- I have introduced the road followed by Selleri in his research to reach the so-called Inertial Transformations (IT), and the arrival to Selleri's new theory, the Weak Relativity (WR).
- I have showed the mathematical passages from Lorentz to Inertial Transformations, with the synchronization parameter e_I and his peculiar value $e_I = 0$ in IT, which explains all the paradoxes related to Special Relativity.
- I have calculated a precise example, i.e., the Sagnac effect, in order to show how the new theory explains the real effect on the rotating disk, unlikely the null results obtained by the Special Relativity.

- I have analyzed that in FRT Selleri has really produced an alternative theory, which has solved many doubts about SRT, even if it is not a theory that replaces it. On the contrary, the theory has turned out to be paradoxically weak, although empirically rich in calculated theoretical results better than Special Relativity.
- I have made a short glimpse on the other activities played by Selleri in 1990s and 2000s, namely, the superluminal signals (SLS), the relationship with Alton Arp and Tom van Flandern, and some papers on Bohr's complementarity and Heisenberg's indeterminacy relations, showing how Selleri has always look after the historical and philosophical approach to Physics. Concerning the SLS, I revealed how Selleri had a continuous commitment with them, not only limited to 1970s and 1980s, as declared by some scholars, but also in 1990s and 2000s.
- I have established, also thanks to the overwhelming correspondence preserved in Selleri's private archive, his personal and scientific links with various Scientific Societies and with several very leading scholars, including Bell, Bethe, Allais, Rubbia, Segré, Jammer, de Broglie and Popper, and so on. Specifically, for the last three, I have shown extensive and lasting correspondence.

In every chapter I introduced a landscape of the topics involved, i.e., the particles development and the strong interactions in PP; the several interpretations of QM, the special theory of relativity and the opposition to it, using a lot of sources by several scholars, all available in the Bibliography.

The research has been possible with the great number of letters that Selleri interchanged with the main scholars of his times. This huge correspondence and unpublished documents has been found in his Private Archive and in the other Archives I have managed to check. Also the oral history interviews I managed to do with the various scholars have been full of findings, unpublished curiosities and information that was often crucial to the definition of a picture as organic and precise as possible.

Of course, the above summary is just a general overview, not thorough at all.

CONCLUSIONS

Concerning the weight of Selleri's scientific output, I took into account on Google Scholar a bibliometric research on his scientific production, albeit limited to only 175 publications, many of which related to years with a more complicated access to sources than today. It shows the following:

Publication years: 1960-2013

Citation years: 60 (1960-2020)

Papers: 175

Citations: 3424

Citations/year: 57.07 (acc1=30, acc2=17, acc5=6, acc10=5, acc20=1)

Citations/paper: 19.57

Authors/paper: 1.52/1.0/1 (mean/median/mode)

Age-weighted citation rate: 182.00 (sqrt=13.49), **138.88/author**

Hirsch h-index: 31 (a=3.56, m=0.52, 2560 cites=74.8% coverage)

Egghe g-index: 55 (g/h=1.77, 3078 cites=89.9% coverage)

PoP hI,norm: 26

PoP hI,annual: 0.43

Regardless of these statistics, it is undeniable that Selleri's research has contributed to a greater understanding of the fields of PP, FQM and FRT. He introduced several elements of reflection that are still the subject of various debates even if, in the end, the two main theories of the 20th century, namely QM and RT, seem to have held up quite well.

In the history of science it is also important to do research on paths that seem to have led nowhere. Both because, even in the event of failure, one has to try, in order to be able to grow; and because even in the ways without an apparent future, there are interesting cues and even fundamental influences, as I have tried to show with this thesis.

Selleri always investigated the philosophical and scientific problems in the main theories of twentieth century, QM, and RT, trying to resolve the several paradoxes related to these theories, surely contributing to the debate.

He always proposed numerous experiments in order to resolve the contradiction between Quantum Mechanics and Local Realism; or between the Theory of Special Relativity and his Weak Relativity. Or to detect the Empty Waves, or to overcome the loopholes related to the low-efficiency in detectors in Bell-Type experiments.

A lot of his papers were a deep description of the state-of-the-art in the field he researched, showing all contributions by the other scholars.

The lack of the creation of a school and a tendency not to push on social relations in order to obtain what he wanted in the diverse field of research are undoubtedly some of the limitations of Selleri's work.

But the extreme rigour of his character, if whilst has denied him some easy potential advantages, conversely has made him an irreproachable person, who could hold his head up high, still receiving numerous attestations of appreciation for his correctness and intellectual honesty. Several excerpts are present throughout this thesis.

He has always taken precise standpoint in his study of scientific problems, inserted in a political and social, historical and philosophical context. For instance, he has published a large number of articles on the danger of nuclear weapons and the usefulness of civil nuclear power, as well as against all anti-scientific drifts.

Paradoxically, in antithesis with his continuous concrete manifestations of importance given to the history and philosophy of physics, Selleri has made a ferocious selection of his writings and his library, certainly making it difficult to try to reconstruct the path of his scientific career.

Looking at Selleri works it has emerged the figure of a scientist who always felt free to deal with what really interested him. And an approach directed to a global, interrelated analysis of physical phenomena, as demonstrated by his introduction of extended models like the Peripheral Model, or the Weak Relativity, so as his devotion to Local Realism. It reveals in him a continuous ability to create acute and valid *Gedankenexperimente*, an absolute honesty in

searching for experimental evidence, quoting authors and presenting a clear state of the art inherent in each single topic.

Moreover, there is always a clear ability to go down different paths, even abandoning them, and a courageous admission of one's own failures.

Always objective, straight-arrow and straightforward, even at the price sometimes of net judgments and several breakups with other scientists without plea, he has always proposed to experimentally verify his theories.

In addition, as we have seen, Selleri has always conducted every research in physics taking into account a wide range that included the history and philosophy of physics, convinced that:

physics is a human activity and from us inherits the habit to parade the successes and to hide difficulties and failures...One should never forget that behind the equations of a theory there is a huge qualitative structure made of empirical results, generalizations, hypotheses, philosophical choices, historical conditionings, personal tastes, conveniences (Selleri 2015, p. 27).

I am going to cite several excerpts in which Selleri explain his view and gives a sort of overview of his activity

[...] there are people who have realised their dreams, you tell me about your brother and your husband. How I envy them! The hopes that I can make mine come true are very low, even though I trust that I belong to a movement that will sooner or later win. The crisis of physics, well hidden from the uninitiated, is devastating; for decades they have been wasting time behind expensive and useless projects. But the control of careers and publications leaves room for a small group of brave people who can freely write and speak, because nobody listens. In 1927 there was a genetic mutation in physics, because the irrationalists founded a new winning theory that settled the philosophical accounts with the Age of Enlightenment, with the Renaissance and with what good human culture had produced. Since then it has been groped in the dark and the “scientific” activity consists of a series of mathematical connotations and experimental research that try to corroborate them, but almost always falsify them. One can console oneself by saying that at least the experimentalists do not cheat. This is so, at least until the next leap in quality. Think only of the “great ideas” of contemporary physics: the quarks and the big bang. Well, there is ample evidence that both are wrong ideas: quarks do not exist and the big bang never existed.

But who cares, the ideas have established themselves, and one idea is worth the other when you don't understand anything! How sad. For what I am worth, I am the Italian son of this sadness and of this crisis. Tolerated, more than listened to. Respected a little bit, even if boring. After all, I can't even complain too much because our country is better than the others from this point of view. How many broken careers, how many isolated people around the world because they refuse to adapt to the current situation!

[...] But perhaps you too know how poor the world of science can be, even if you have seen it from a favourable point of view because a winning one!⁹¹⁷ (Selleri to Loris de Finetti, 3 February 1993-FSPA. My underline)

By 1969 I had come to the conclusion that there was something basically wrong in the way theoretical physics was practiced: too much high mathematics and too little understanding in space and time. Therefore I devoted myself to a critical study of the most fundamental theories, quantum mechanics and relativity. The rest of my scientific life is a consequence of these early decisions. My two most important books are *Quantum Paradoxes and Physical Reality* (1990) and *Weak Relativity* (2011), entirely devoted to relativistic physics. After fifty years of intense activity I have come to the conclusion that there was no good reason to abandon the fundamental ideas of classical physics: three dimensional space, time, cause, effect, particle and wave. (FSPA-2011)

There are behind me 30 years of hard work on the foundations of modern physics, quantum theory and relativity. It is like in any other activity, if you practice it you develop yourself. I do not feel superior to the best physicists around, generally speaking, but when it comes to fundamental matters it must be confessed that I do: most of them repeat only childish arguments overcome long ago. This activity is still unable to attract large masses of researchers, essentially because power (money, careers, etc) is elsewhere, but the number of people active in the foundations of modern physics is certainly increasing. We are really not many, percentwise, active on the basic issues. The other ones are slaves of the given dogmas! That is good for us, in a way: the competition is tolerable. However, if we achieve something important it will not be recognized and accepted by the community of physicists, and this is bad.

It is especially bad because I have just published (*in 1996*) a better theory than relativity. It is absolutely needed for very important reasons, but nobody cares (well, J. Brandes does, and so do a few other people). It is so frustrating that I almost need psychotherapy (it is only a joke again). This is my life. I have always been very severe with myself and do not allow daydreaming about non existing successes. But if something important is achieved it cannot be kept hidden, and I know I have done something now (Selleri to Silvia Längle, 18 March 1998-FSPA. My underline).

⁹¹⁷ The original Italian text is in Addendum 3, p. 1058.

I have been active in the foundations of modern physics over the last 30 years or so, in particle theory, quantum theory and relativity. My driving force was mainly curiosity, the wish to understand the extent of our knowledge of the physical reality. It was not my choice to be a priori critical of the existing theories, but a critical standpoint imposed itself very soon as a reaction to the superficial and dogmatic attitude existing within the scientific community when the basic issues were discussed.

The first negative impression came soon after starting to do research from watching a large community of physicists struggling in many countries to understand the properties of high energy interactions and achieving very little success in spite of the large amounts of funds expended, the highly developed technological means employed, and the sophisticated theoretical and mathematical methods used (dispersion relations, Mandelstam representation, Regge poles, ...). In meson-nucleon scattering not even the sign of the “measured” phase-shifts could be reproduced by the theoretical models. The field was nevertheless full of enthusiasm, a little bit with the spirit of explorers who are in a completely new land. Great promises were constantly made by the leaders: our theories would soon interpret the large amounts of new data collected by the experimentalists with the large accelerators of Europe and America. This was not enough for me: having decided that I had only one lifetime to invest in physics I soon shifted my interests to the foundations of quantum theory, very clearly responsible for the distortions of theoretical particle physics.

Other people must have been making similar choices because it is evident that in the last twenty years there has been a relatively large increase in that minority of physicists actively engaged in researches opposing one way or another the fundamental dogmas of our science.

In spite of the positive long term trend it is difficult to be optimistic about the near future, because the scientific community has become very conservative as far as the theories of modern physics are concerned. The physicists composing an experimental group will normally find very difficult to start a really fundamental research. Suppose they try to check experimentally the validity of some basic pillar of physics. There are two possible outcomes: If they find that the existing theory is correct, some people will immediately say that the result was obvious and that it was a waste of money to do the experiment. If they find instead that the existing theory is incorrect, the same people will say that their experiment is very probably wrong and nobody will dare contradicting them. Therefore it will become unlikely that the experiment will be repeated by another group, leaving those physicists (possibly for a long time) under the suspicion of having done a bad work. Such a situation makes fundamental physics a risky game. These are some of the consequences of the spreading of dogmatism within the scientific milieu. Repressive tendencies are not new, if already Galilei had to suffer from their existence. Basically they remained the same since the old times (Selleri: Some epistemological problems of modern physics-FSPA. My underline).

I am pleased that you consider me a “global and liberating” reference. To free the minds of physicists (or at least mine and a few friends’ minds) from the twentieth century distortions is a bit of the task I have given myself. They consider me arrogant for this, but I still don’t feel so: for me it was an existential choice, I could not have done otherwise without feeling unhappy.

Physics was a joyful life choice and I didn’t want it to be torn from me just because it was no longer fashionable to embrace rationalism. I don’t care about fashion, I just want the truth, of course as far as possible. I tried to understand the real reasons for certain negative choices and when I got there they seemed to me to be extra-scientific, imported from the lower areas of philosophy⁹¹⁸ (Selleri to Luciana Negro, 1 October 1999-FSPA).

Surely this dissertation is by no means exhaustive and can be considered as a starting point for the development of many issues as well as the completion of several shortcomings.

What to do, then?

There is a need to integrate this research with some operations, as:

- a better research of Selleri’s legacy through a detailed analysis of the current published papers on the issues developed by Selleri;
- some oral history interviews with scholars who attended experiments and conferences with Selleri. For instance, trying to know something on his stay in the USA during the period 1963-1965, and 1970-1971. Or something on the experiments he suggested in Bari on nonconservation of angular momentum;
- some research in other Archives to shed light on some dark point, as missing letters and documents;
- a need to make the Selleri’s private archive, FSPA, accessible and tidy to all scholars, with a huge archival work.

I hope that my dissertation will provide a basis for a debate involving new and further research ideas on these issues.

I would like to end this very long journey in my research on the figure of Franco Selleri with an unpublished note, which I found in FSPA. Almost an admission on his life and dreams; or an idea for some autobiography:

⁹¹⁸ The original Italian text is in Addendum 3, p. 1058.

Since high school Franco Selleri had felt a strong curiosity for the fundamental issues of physics. He dreamt about atoms and subatomic components of matter (electrons, protons, neutrons) and was struck by the structure of the universe: The books spoke of hundreds of billions of galaxies, each composed of about one hundred billion stars. His destiny was marked, he had to study physics at university. So it was, but he was disappointed with the atomic question.

In fact, the dominant doctrine of the schools of Copenhagen and Göttingen taught that one should not believe that matter is really composed of atoms. The “naive atomistic conception” at most could be adopted to bring order to physical phenomena, but only a colossal naivety could make one believe in its “truth”.

It was the end of hopes of understanding the world in which we live, but ours did not resign itself. If the dominant schools were right, then all attempts to give causal descriptions in space and time of atomic phenomena were doomed to failure. Forty years, two hundred articles and ten books after Selleri affirms instead that there is a logical space available to violate Heisenberg’s relations of indeterminacy and Bohr’s principle of complementarity, just as it is demonstrably possible to formulate the laws of movement in such a way as to reaffirm the existence of absolute simultaneity, denied by the principle of relativity. The conclusion is the defeat of the pessimism prevailing in 20th century physics, while the possibility of responding positively to the youthful demands of our own opens up.

Word had just spread of his scandalous research activity that there were comments in scientific circles, often fiercely negative for unorthodox theories, including those of real atoms. To defend them Selleri did not hesitate to sacrifice his relationships with other physicists, a painful but necessary choice to reaffirm the validity of “common sense”.

To give some examples, the Gotha of world fundamental physics participated in the international conferences organised by Selleri: David Bohm (London), Olivier Costa de Beauregard (Paris), Terletski (Moscow), Virendra Singh (Bombay), Mikio Namiki (Tokyo), John A. Wheeler (Texas), Leonard Mandel (Rochester), Henry Stapp (California), David Greenberger (New York) and many others from all scientifically developed countries,

The interest in Selleri’s scientific proposals shown by the philosophers of science must be carefully considered. In a letter dated 12-2-1987 Karl Popper writes: “My dear Franco, your letter of 2 December with your beautiful article ‘Properties of coherence of photonic amplifiers’ reached me only today. I am convinced that it will make the decision in favour of empty waves” .

Physics historians have also accepted the new proposals as admissible. The best known - Max Jammer - says: “Selleri has written with rigour and clarity a superb, comprehensible and informative book for every person interested in quantum mechanics, which is stimulating provoking thought even for those who do not share the philosophical conviction of the author”.

Although contacts with other theoretical physicists were sporadic and short-lived, his work soon gained general respect in a scientific environment where ideas opposed to his were largely dominant. For example, a well-known American physicist - D. Greenberger - writes about one of his books: "Here is a book I have been waiting for a long time, a sympathetic account of the reasons why a small band of physicists believe in various heresies such as the incompleteness of quantum theory and the existence of a causal interpretation of quantum phenomena (....) Professor Selleri is almost unique in being qualified to accomplish this feat, a true skeptic who nevertheless has a deep understanding of the Copenhagen interpretation. If you add to this an ever-present intellectual honesty and a fundamental sense of impartiality that is very evident in this book, you can appreciate what a special document it is (....) I am deeply impressed by the depth that Selleri has shown in presenting his alternative views".

Even more important is the opinion of Louis de Broglie, one of the founding fathers of modern physics. In a letter dated 17 May 1980 writes: "The research that Selleri carries out, whose works I know well, are centred on one of the most important problems of wave mechanics, that of the physical interpretation of the wave function (....) I can give a very favourable opinion of his research and I think that Selleri deserves to be fully helped and encouraged both for his past works and for those he is doing"⁹¹⁹ (Notes for an autobiography?-FSPA)

⁹¹⁹ The original Italian text is in Addendum 3, pp. 1059-1060.

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Caption:

PARTICLE PHYSICS-PP

FOUNDATIONS OF QUANTUM MECHANICS-FQM

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HISTORY OF SCIENCE-HoS

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ADDENDUM 1

SELLER'S LISTS & WRITINGS

The summary below is intended to be as exhaustive as possible and is the first of its kind concerning Franco Selleri, showing uniquely, unlikely the Bibliography, also the unpublished and typewritten papers, too, in order to preserve the best chronological arrangement of Selleri's work. It is the result of extensive research at the archives and I hope it will contribute to shed light on the Selleri's active and varied interests.

Here are reported the written and edited books, his papers in English as well as in different languages; his popularization articles on newspapers and the dissertations and doctoral theses he followed as a supervisor. In some cases, a brief caption describes some feature. I found 30 written books, 16 edited ones, 229 English papers (40 about PP, 125 regarding FQM, and 64 on FRT), 121 papers in other languages, and 20 articles on newspapers. So, in total, selleri produced 416 writings, being also an advisor for 39 degree and PhD theses.

From a historiographical point of view, I decided to use a chronological classification, in order to reveal how Selleri's three research fields often intertwined each other.

Anyway, I hope that any and possible shortcomings could also be integrated with a comparison between those who read this dissertation.

I have also brought here some of the many lists produced by Selleri to organize his writings. All reported a peculiar choice between his papers.

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[In 1988 he reached the 3rd edition with over 15,000 copies sold.](#)

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DEGREES AND DOCTORATE THESES (1962-2004)

Caption:

PARTICLE PHYSICS-PP

FOUNDATIONS OF QUANTUM MECHANICS-FQM

FOUNDATIONS OF RELATIVITY THEORY-FRT

HISTORY OF PHYSICS-HS

TEACHING PHYSICS-TP

- **PP** Moretti Elena (1962), “Risonanze fra particelle elementari”, Università di Bologna, Facoltà di Scienze, Laurea in Fisica.
- **PP** Turchetti Giorgio (1965), “????”, Università di Bologna, Facoltà di Scienze, Laurea in Fisica.
- **PP** Grecchi Vincenzo (1966), “Produzione periferica di mesoni ρ ”, Università di Bologna, Facoltà di Scienze, Laurea in Fisica.
- **PP** Graffi Sandro (1966), “Fotoproduzione di mesoni vettoriali”, Università di Bologna, Facoltà di Scienze, Laurea in Fisica.
- **PP** Brunini P. (1967), “Teoria dei poli di Regge e confronto fra diverse reazioni”, Università di Bologna, Facoltà di Scienze, Laurea in Fisica.
- **FQM** Giglietto Antonio (1968), “Meccanica quantistica e processi markoviani”, Università di Bari, Facoltà di Scienze, Laurea in Fisica.
- **PP** Murro Onofrio (1968), “Produzione di particelle strane in urti di alta energia”, Università di Bari, Facoltà di Scienze, Laurea in Fisica.
- **PP** Fortunato Donato (1969), “Violazione di CP nel decadimento del K^0 ”, Università di Bari, Facoltà di Scienze, Corso di Laurea in Fisica.

- **FQM** Petrone Giovanni (1970), “Approssimazione stocastica della meccanica quantistica e sua formulazione lagrangiana”, Università di Bari, Facoltà di Scienze, Laurea in Fisica.
- **FQM** Calò Carducci Carmelo (1970), “Meccanica quantistica e variabili nascoste”, Università di Bari, Facoltà di Scienze, Istituto di Fisica, Corso di Laurea in Fisica.
- **FQM** Garuccio Augusto (1972), “Realtà fisica e problema della conoscibilità in meccanica quantistica”, Università di Bari, Facoltà di Scienze, Corso di Laurea in Fisica.
- **TP** Di Glionda Matteo (1973), “L’insegnamento della meccanica quantistica nelle scuole secondarie”, Università di Bari, Facoltà di Scienze, Corso di Laurea in Fisica.
- **FQM** Augelli Vincenzo (1973), “Onde, corpuscoli e teoria quantistica”, Università di Bari, Facoltà di Scienze, Corso di Laurea in Fisica.
- **FQM** Bianchetti Marco (1975), “Genesi storica del principio di corrispondenza in meccanica quantistica”, Università di Bari, Facoltà di Scienze, Istituto di Fisica. Laurea in Fisica.
- **FQM** Scalera Giancarlo (1976), “Fattorizzazione degli stati in meccanica quantica”, Università di Bari, Facoltà di Scienze, Laurea in Fisica.
- **FQM** Fanelli Biagio (1980), “Sul significato del principio di corrispondenza”, Università di Bari, Facoltà di Scienze, Laurea in Fisica.
- **FQM** Carbone Agostino (1981), “Studio degli aspetti ondulatori e corpuscolari dei sistemi quantici”, Università di Bari, Facoltà di Scienze, Laurea in Fisica.
- **FQM** Galvan Bruno (1981), “Teorie probabilistiche locali”, Università di Pisa, Facoltà di Scienze, Laurea in Fisica.
- **TP** Altieri Maria (1982), “Una proposta per l’insegnamento della fisica: da Copernico a Newton”, Università di Bari, Facoltà di Scienze, Laurea in Fisica.

- **HS** Asprella Maria (1983), “L’evoluzione del concetto di energia”, Università di Bari, Facoltà di Scienze, Laurea in Fisica.
- **FQM** Pascazio Saverio (1983), “Rescattering dei fotoni negli studi sperimentali delle diseguaglianze di tipo Bell con cascate atomiche”, Università di Bari, Facoltà di Scienze, Corso di Laurea in Fisica.
- **TP** Minerva Maria (1984), “Indagine sul ruolo della matematica nella fisica”, Università di Bari, Facoltà di Scienze, Laurea in Fisica.
- **FQM** Ramalho Croca Josè (1985), “Efeito de Tunel na formulação causal da mecânica quântica”, Universidade de Lisboa, Faculdade de Ciências, Dottorato in Fisica.
- **FQM** Conenna Nicola (1986), “Studio di un’esperienza di amplificazione di luce di bassa intensità”, Università di Bari, Facoltà di Scienze, Laurea in Fisica.
- **FQM** Lepore Vito Luigi (1987), “Conseguenze fisiche della formulazione probabilistica del realismo locale”, Università di Bari, Facoltà di Scienze, Laurea in Fisica.
- **FQM** De Caro Liberato (1988), “Modelli locali con ‘Enhancement’ compatibili con le predizioni quanto-meccaniche”, Università di Bari, Facoltà di Scienze, Laurea in Fisica.
- **TP** Mascolo Francesco (1990), “L’insegnamento della meccanica quantistica nelle scuole medie superiori”, Università di Bari, Facoltà di Scienze, Laurea in Fisica.
- **FQM** Lepore Vito Luigi (1991), “Le teorie di campo quantizzato e la descrizione del fotone nello spazio-tempo”, Università di Bari, Dipartimento di Fisica, Dottorato in Fisica.
- **FQM** Schmidt Michael (1991), “An attempt to disentangle space and momentum coordinates”, Universität Bremen, Fachbereich 1 (Physik), Dottorato in Fisica.
- **FQM** Nobre Moreira Rui António (1993), “Contribuição para o estudo do conceito de complementaridade”, Universidade de Lisboa, Faculdade de Ciências, Dottorato in Fisica.

- **FQM** Foadi James (1993), “Diseguaglianze di Bell per una combinazione arbitraria di funzioni di correlazione”, Università di Torino, Facoltà di Scienze, Laurea in Fisica.
- **FRT** Iannelli Fernando (1993), “Sulla equivalenza fra le trasformazioni di Lorentz e quelle di Tangherlini”, Università di Bari, Facoltà di Scienze, Laurea in Fisica.
- **HS** Mango Rosanna (1996), “Newton e la nascita della fisica moderna”, Università di Bari, Facoltà di Scienze, Laurea in Fisica.
- **FQM** Cobianco Sebastiano (1996), “Il paradosso di EPR per coppie di kaoni neutri”, Università di Bologna, Facoltà di Scienze, Laurea in Fisica.
- **FQM** Risco Delgado Ramón (1997), “Probabilidad de detección, olas vacías y conversión a la baja”, Universidad de Sevilla, Facultad de Física, Dottorato in Fisica.
- **FQM** Foadi Roshan (1998), “Paradosso di EPR per coppie di kaoni neutri”, Università di Torino, Facoltà di Scienze, Laurea in Fisica.
- **FQM** D’Angelo Milena (1999), “Il teletrasporto in meccanica quantistica”, Università di Bari, Facoltà di Scienze, Laurea in Fisica.
- **FRT** Intini Francesca (1999), “L’effetto Sagnac”, Università di Bari, Facoltà di Scienze, Laurea in Fisica.
- **FRT** Romano Luigi (2004), “Il principio di relatività”, Università di Bari, Facoltà di Scienze, Laurea in Fisica.

SELLERI'S LISTS

I reported here some of the numerous lists prepared by Selleri. He made a great selection of his papers, deciding for the following as the most representative of his activity and production. I left the original layout as reported on Selleri's PC.

All codex near the papers show the belonging to several lists prepared by Selleri (T4, for instance, is referred to a list of all his papers; R1 is referred to a list on relativity papers, S45 to seminars, and so on) and not reported here, though. All papers showed here are already in Selleri's writings in the previous pages.

PHYSICS ARTICLES READING GUIDE

by F. Selleri

- 1 One pion exchange model (1960-1970)**
- 2 Elementary particles without angular momentum conservation (1968-1970)**
- 3 The empty wave question (1968-1990)**
- 4 Papers on the EPR paradox (1970 – 2001)**
- 5 Papers on relativistic physics (1990 – 2009)**
- 6 Talks in high schools, etc. (1968 - 2003)**

Arguments of history of physics

Questions of relativity

Foundations of quantum mechanics

A non metaphysical realism

Against obscurantist trends

1) ONE PION EXCHANGE MODEL (1960-1970) Selected papers by F. Selleri

T1. Pion cloud effects in pion production experiments, (collaboration with F. Bonsignori), **Nuovo Cim.** 15, 465-478 (1960).

T4. Role of the Peripheral Interaction in Proton-Proton Collisions, **Phys. Rev. Letters** 6, 509 (1961).

T5. Off-Shell Pion-Nucleon Scattering and Dispersion Relations (Collaboration with E. Ferrari), **Nuovo Cim.** 21, 1028 (1961).

T6. Pionic Form Factor Effects in Peripheral Nucleon-Nucleon Collisions (Collaboration with E. Ferrari), **Phys. Rev. Letters** 7, 387 (1961).

T11. Peripheral Model for Inelastic Processes (Collaboration with E. Ferrari), **Suppl. Nuovo Cim.** 24, 453 (1962).

T13. Off-Shell Pion-Pion Scattering in the $T=J=1$ State, **Phys. Letters** 3, 76 (1962).

T14. Unitarity condition and connections between pion-pion and pion-nucleon scattering (collaboration with U. Amaldi), **Nuovo Cim.** 31, 360-376 (1964).

T21. Virtual Pion-Nucleon Scattering, **Nuovo Cim.** 40, 236 (1965).

T22. Is Absorption Necessary in High-Energy Inelastic Processes?, **Nuovo Cim.** 42, 835 (1966).

T29. Azimuthal Dependence of $\pi\pi$ Scattering (Collaboration with V. Grecchi and G. Turchetti), **Nuovo Cim.** 52 A, 1306 (1967).

T33. Interference Effect in Peripheral Production of ρ -Mesons (Collaboration with V. Grecchi and G. Turchetti), **Nuovo Cim.** 59 A, 328 (1969).

**2) ELEMENTARY PARTICLES WITHOUT ANGULAR
MOMENTUM CONSERVATION (1968-1970) by F. Selleri**

T32. Relativistic Invariance without Angular Momentum Conservation, **Nuovo Cim.** 57 A, 678-688 (1968).

T34. The Physics of Spin $\frac{1}{2}$ K mesons, **Nuovo Cim.** 60 A, 291-320 (1969).

T36. The Multi-Regge Model and Strange Particle Production (collaboration with O. Murro), **Lettere al Nuovo Cim.** 1, 784-788 (1969).

T38. Two-Body Processes and Regge-Pole Model (collaboration with O. Murro), **Lettere al Nuovo Cim.** 2, 863-869 (1969).

T40. Angular Momentum Nonconservation Scheme, in HIGH ENERGY PHYSICS AND NUCLEAR STRUCTURE, 751-758, S. Devons, ed., Plenum Press, New York-London (1970).

T41. Noninvariant Vacuum and Relativistic Quantum Field Theory, University of Bari, unpublished preprint, 1-10 (1970).

3) THE QUESTION OF EMPTY WAVES (1968-1990) selected papers
by F. Selleri

T37. On the wave function of quantum mechanics, **Lettere al Nuovo Cim.** 1, 908-910 (1969).

T43. Realism and the wave-function of quantum mechanics, in FOUNDATIONS OF QUANTUM MECHANICS, 398-406, B. d'Espagnat, ed., Proc. International School of Physics "Enrico Fermi", Course IL, Academic Press, New York (1971).

T52. Is nondistributivity for microsystems empirically founded? (collaboration with G. Tarozzi), **Nuovo Cim.** 43B, 31-40 (1978).

T70. On the direct observability of quantum waves, **Found. Phys.** 12, 1087-1112 (1982) and in QUANTUM, SPACE AND TIME - THE QUEST CONTINUES, 203-228 (A.O. Barut, ed.) Cambridge Univ. Press (1984).

T74. Some possible experiments on quantum waves (collaboration with J. Andrade e Silva and J.P. Vigi er), **Lettere Nuovo Cim.** 36, 503-508 (1983).

T76. "Gespensterfelder" in THE WAVE PARTICLE DUALISM, 101-128, (S. Diner, et al. eds.) Reidel, Dordrecht (1984).

T87. Local realistic photon models and EPR-type experiments, **Phys. Letters** 108A, 197-202 (1985).

T90. Complementarity vs Causality in Space and Time, In: ADVANCES IN FUNDAMENTAL PHYSICS, pp. 381-397, (M. Barone and F. Selleri, eds.), Hadronic Press, Palm Harbor, Fl. (1995)

- T90. Bohr's complementarity in the light of modern researches in the foundations of quantum theory **Riv. Storia Scienza** 2 , 545-556 (1985).
- T97. Amplification of light from an undulatory point of view, **Phys. Letters** 120A , 371-376 (1987).
- T98. Coherence properties of photon amplifiers **Found. Phys.** 17 , 739-757 (1987).
- T102. On a possible way to detect de Broglie's waves (collaboration with J.R. Croca and A. Garuccio), **Found. Phys. Letters** 1 , 101-112 (1988).
- T103. Amplification of incoherent light behind a beamsplitter (collaboration with A. Garuccio e P. Guéret), **Found. Phys. Letters** 1 , 139-145 (1988).
- T104. Reply to "Empty waves do not induce stimulated emission in laser media", **Phys. Letters** 132A , 72-74 (1988).
- T119. Is a direct experimental detection of quantum waves possible? **Australian Physicist** 27 , 214-219 (1990).
- T121. Empty quantum waves and particle trajectories (con M. Schmidt) in IN SEARCH OF QUANTUM REALITY (R. Nair et al., ed.) Plenum, New York (1991).
- T124. Empty-wave effects on particle trajectories in triple-slit experiments (con M. Schmidt), **Found. Phys. Letters** 4 , 1-17 (1991).

4) SELECTED PAPERS ON THE EPR PARADOX (1970-2001) by F. Selleri

T48. Observable consequences from second-type state vectors of quantum mechanics (collaboration with D. Fortunato and A. Garuccio), **Int. Jour. Theor. Phys.** 16, 1-6 (1977).

T50. On local causality and the quantum-mechanical state vector (collaboration with A. Garuccio and G.C. Scalera), **Lettere Nuovo Cim.** 18, 26-28 (1977).

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ADDENDUM 2

CONFERENCES, SEMINARS, AND

ASSIGNMENTS

In this addendum I showed in three different sessions, each structured in a chronological order, 97 international conferences and seminars, 42 national conferences, and 176 seminars and workshops in schools, associations, and political circles held by Selleri during his activity and specifically selected by him. A frenetic practice, linked to the will to spread his ideas and to participate in meetings with other scholars for future contacts and collaborations. For this purpose, Selleri also used to send several copies of his books to a very large mailing list. At the end of the international conferences, I also submitted a list prepared by Selleri (updated only until 1991, though), which plainly expresses a selection of what for him were the main conferences on the foundations of quantum mechanics that he attended as inviter lecturer and that he organized; and a chart with his participations at Italian seminars, by quantifying them from 1979 to 2007, even though some of them are quite incomplete and surely I could not find several others which maybe also occurred in the missing years. He produced also a specific, unpublished book that I found in his Archive (FSPA), *Partecipazioni a conferenze*, in which he reported many conferences he experienced as well as posters and material on his talks. I decided to complete the above-mentioned list of conferences with the information presented in this book, too.

I did not include conferences AND seminars held in the schools whose information were really too vague and uncertain to be included. However, the graphs in the Summary And Conclusion section contain them, so their number is the real one in that context and higher than the one listed here. Concerning the conference C16., Selleri wrote a letter in French to complain about his participation and to send a paper for the proceedings. I reported it in a footnote with its translation in English. Selleri dealt with diverse topics, including Philosophy and History of Science, Cinema, Social responsibility of physicists, Science and Religion, Didactic of Science, etc. See for instance C32. (a conference organized by the International Laboratory of Architecture and Urban Design, quite diverse by a gathering of physicists), or several national conferences (see N3., N7., N35., N37.).

PARTICIPATIONS AT INTERNATIONAL CONFERENCES

C1. 1961, May, Geneva, Switzerland

CERN CONFERENCE ON HIGH ENERGY PHYSICS

Org. by: CERN

Talk: Quasi-elastic scattering of high-energy protons in the OPE model

C2. 1962, Birmingham, UK

CONFERENCE ON HIGH-ENERGY AND NUCLEAR PHYSICS

Talk: *on reaction $PP \rightarrow PN\pi^+$*

C3. 1964, April, Washington, D.C., U.S.A.

MEETING OF THE AMERICAN PHYSICAL SOCIETY

Org. by: American Physical Society

Talk: Deuteron production in proton-proton collisions

C4. 1964, 29 June-3 July, Boulder, CO, U.S.A.

CONFERENCE ON PARTICLES AND HIGH ENERGY PHYSICS

Org. by: Dept; of Physics and Astrophysics, University of Colorado

Talks:

πN Collisions Below 1.5 BeV/c,

On the Reaction $p + p \rightarrow D + \pi^+$.

C5. 1965, Oxford, UK

Conference on elementary particles

Talk: on absorption and form factor

C6. 1967, 20-27 September, Heidelberg, Germany

INTERNATIONAL CONFERENCE ON ELEMENTARY PARTICLES

Org. by: CERN and Institut für Hochenergiephysik Heidelberg

C7. **1969**, 25 June-1 July, Lund, Sweden

INTERNATIONAL CONFERENCE ON ELEMENTARY PARTICLES

Org. by: Institute of Physics, Lund

Talk: *On the wave function of quantum mechanics.*

C8. **1969**, 8-12 September, New York, U.S.A. Columbia University.

THIRD INTERNATIONAL CONFERENCE ON HIGH ENERGY
PHYSICS AND NUCLEAR STRUCTURE SPONSORED BY THE
INTERNATIONAL UNION OF PURE AND APPLIED PHYSICS

Talk: *Angular momentum nonconservation scheme*

C9. **1970**, 29 June-11 July, Varenna, Italy

FOUNDATIONS OF QUANTUM MECHANICS-International School of
Physics "Enrico Fermi", Course IL.

Org. by: Italian Physical Society

Talk: *Realism and the wave function of quantum mechanics*

C10. **1971**, March, London (Ontario), Canada

QUANTUM PHYSICS CONFERENCE

Org. by: Dept. of Philosophy, University of Western Ontario

Talk: *von Neumann's theorem and hidden variables*

C11. **1976**, 18-23 April, Erice, Italy

THINKSHOP ON PHYSICS: EXPERIMENTAL QUANTUM
MECHANICS

Org. by: "Ettore Majorana" Centre for Scientific Culture

Talk: *Testing Quantum Mechanics*

C12. **1977**, 3-5 December, Seelisberg, Switzerland

CONSCIOUSNESS AND NATURAL LAW

Org. by: Maharishi European University

Talk: *Difficulties of quantum mechanical treatment of correlated systems*

C13. **1979**, 6-13 June, Paris, France, Institute Henri Poincaré
 CENTENAIRE DE LA NAISSANCE D'ALBERT EINSTEIN
 Org. by: Collège de France and Fondation Louis de Broglie
 Round table 07/06: *Le paradoxe d'Einstein, Podolsky et Rosen*,
 (With the presence of O. Costa de Beauregard, J. Bell, B. d'Espagnat, M.
 Jammer, G. Lochak, M. Mugur-Schächter, F. Selleri, J. P. Vigiér).

C14. **1979**, 22-29 August, Erice, Italy
 INTERNATIONAL SCHOOL OF HISTORY OF SCIENCE: 3rd COURSE-
 some influential schools and figures in contemporary science-topic II: the
 Copenhagen school of atomic physics and the Einstein-Bohr controversy
 Org. by: "Ettore Majorana" Centre for Scientific Culture
 Talk: *Recent results on the EPR paradox*

C15. **1979**, 3-14 September, Udine, Italy
 MATHEMATICAL THEORY OF DYNAMICAL SYSTEMS AND
 MICROPHYSICS,
 Org. by: International Centre for Mechanical Sciences
 Talk: *On the consequences of Einstein locality*

C16. **1979**, 1-5 October, Cordoba, Spain
 SCIENCE ET CONSCIENCE,
 Org. by: France Culture
 Talk: *von Neumann's measurements and consciousness: a critical review*⁹²⁰

⁹²⁰ My speeches at the conference were very short and sketchy due to lack of time. On the other hand, I was the only participant who did not have the opportunity to present his ideas in detail. I would therefore be happy to send you a paper for the collective volume: the paper can be written very quickly since I already have all the material ready and it would be a critical history of all the main proposals for a role of consciousness in quantum mechanics, with a discussion of the ideas of Bohr, Heisenberg, Jordan, Pauli, von Neumann, London and Bauer, and Wigner. Obviously a reading would show that I personally disagree with this philosophy but the paper would try to give a review as objective as possible. (Mes interventions au colloque ont été très brèves et schématiques pour faute de temps. De l'autre côté j'ai été le seul participant qui n'a pas eu la possibilité de exposer un peu en détail ses idées. Je serais donc heureux de vous envoyer un papier pour le volume collectif: le papier peut être écrit très rapidement puisque j'ai déjà tout le matériel prêt et il serait une histoire critique de toutes les principales propositions d'un rôle de la conscience dans la mécanique quantique, avec une discussion des idées de Bohr, Heisenberg, Jordan, Pauli, von Neumann, London et Bauer et Wigner. Évidemment une lecture montrerait que je ne suis personnellement pas d'accord avec cette philosophie mais le papier essaierait de donner une revue autant objective que possible).

C17. **1979**, 10-12 November, Geneva, Switzerland, Institute de la Méthode

INDETERMINISME QUANTIQUE ET VARIABLES CACHEES

Org. by: Association Ferdinand Gonseth

Talk: *Is action-at-a-distance allowed in quantum mechanics?*

C18. **1980**, 1-13 September, Peyresq, France

LA PENSEE PHYSIQUE EN 1980, SCIENCE ET HUMANISME EN NOTRE TEMPS-Colloque en hommage a Nicolas-Claude Fabri de Peiresc

Org. by: Fondation Louis de Broglie

Talk 02/09: *Particules élémentaires et mécanique quantique*

C19. **1981**, 17-19 October, Cambridge, U.K. King's College

3rd ANPA MEETING

Org. by: Alternative Natural Philosophy Association

Talk: *Experiments bearing on wave-particle dualism*

C20. **1982**, 22-30 April, Perugia, Italy

THE WAVE PARTICLE DUALITY

Org. by: Fondation Louis de Broglie, Bari University, Perugia University

Talk: *Are de Broglie's waves directly observable?*

C21. **1982**, 21-23 July, Paris, France

INTERNATIONAL COLLOQUIUM ON THE HISTORY OF PARTICLE PHYSICS,

Org. by: C.N.R.S. and Société Française de Physique

C22. **1982**, 27-30 September, Athens, Greece

THE CONCEPT OF REALITY IN NATURAL SCIENCES

Org. by: Department of Physics, University of Athens

Talk: *On the reality of quantum waves*

C23. **1983**, 4-7 May, Bari, Italy

OPEN QUESTIONS IN QUANTUM PHYSICS-WORKSHOP DEVOTED
TO THE EPR PARADOX, TO THE OBSERVABILITY OF QUANTUM
WAVES TO THE FUNDAMENTAL STOCHASTIC PROCESSES

Org. by: INFN, Physics Department of the University of Bari

Talk 05/05: Three forms of the EPR paradox.

C24. **1984**, 20-21 September, Santa Flavia, Sicily, Italy

SYMPOSIUM ON NEW TRENDS IN ATOMIC COLLISION PHYSICS,

Org. by: ASI and Atomic Physics Laboratory, University of Stirling

Talk: Einstein locality and EPR type experiments

C25. **1984**, 24-28 September, Athens, Greece

SECOND INTERNATIONAL MEETING ON EPISTEMOLOGY-FORMS
OF PHYSICAL DETERMINISM

Org. by: Department of Philosophy, University of Joannina

Talk: Probabilistic causality in quantum theory

C26. **1985**, 22 February, Stirling, U.K.

QUANTUM MECHANICS AND LOCAL REALISM: RECENT
DEVELOPMENTS IN THEORY AND EXPERIMENT

Org. by: Physics Department of Stirling University

Talk: Local realistic photon models and EPR-type experiments

C27. **1985**, 16-20 June, Joensuu, Finland

SYMPOSIUM ON THE FOUNDATIONS OF MODERN PHYSICS, 50
YEARS OF THE EPR GEDANKENEXPERIMENT

Org. by: Department of Physics of Turku University

Talk: The predictive power of Einstein's elements of reality

C28. **1985**, 25 September-3 October, Urbino, Italy

MICROPHYSICAL REALITY AND QUANTUM FORMALISM-
PERSPECTIVES ON THE EINSTEIN-BOHR DEBATE FIFTY YEARS
AFTER THE EPR ARGUMENT

Org. by: Urbino University and Physics Dept. of Bari University

Talk 03/10: Photons with variable detection probability in EPR-type experiments

C29. **1985**, 25-27 November, Rome, Italy, Accademia dei Lincei

INTERNATIONAL SYMPOSIUM ON NIELS BOHR

Org. by: Physics Department, Rome University, and Academy of Denmark

Talk 26/11: Bohr's complementarity and the Einstein, Podolsky, Rosen paradox

C30. **1986**, 21-24 January, New York, U.S.A.

NEW TECHNIQUES AND IDEAS IN QUANTUM MEASUREMENT

THEORY- in honor of Eugene P. Wigner

Org. by: New York Academy of Sciences

Talk 22/01: Enhanced photon detection as an explanation of EPR experiments

C31. **1986**, 23-27 June, Bridgeport (CT), U.S.A.

NATO ADVANCED RESEARCH WORKSHOP-QUANTUM
VIOLATIONS: RECENT & FUTURE EXPERIMENTS &
INTERPRETATIONS,

Org. by: University of Bridgeport

Talk: Amplification of empty quantum waves

C32. **1986**, 11-13 August, Siena, Italy

WHERE, WHY AND HOW

Org. by: International Laboratory of Architecture and Urban Design

Talk: *Is science always rational?*

C33. **1987**, 21-25 September, Gdansk, Poland

PROBLEMS IN QUANTUM PHYSICS GDANSK 1987- RECENT AND
FUTURE EXPERIMENT AND INTERPRETATIONS

Org. by: Institutes of Physics of Gdansk University

Talk: *Is Bell's inequality violated in nature?*

C34. **1987**, 12-16 October, Delphi, Greece

THE CONCEPT OF PROBABILITY

Org. by: National Hellenic Research Foundation, Hellenic Minister of
Culture and Civilization

Talk: *Possible experiments on the nature of quantum probabilities*

C35. **1987**, 29-31 December, Calcutta, India

SYMPOSIUM ON THEORETICAL PHYSICS DEDICATED TO PROF.
NIELS BOHR & SYMPOSIUM ON MATHEMATICS (NUMBER
THEORY) DEDICATED TO DR. S. RAMANUJAN

Org. by: Calcutta Mathematical Society

Talk: *Niels Bohr from virtual fields to complementarity*

C36. **1988**, 31 May-2 June, Dubna, USSR

PHYSICAL INTERPRETATION OF QUANTUM MECHANICS AND
ALTERNATIVE THEORIES,

Org. by: Joint Institute for Nuclear Research

Talk 02/06: *Prediction of directly observable effects due to the quantum
mechanical empty wave*

C37. **1989**, 25-27 May, Brest, France

LA RATIONALITE' DES PHENOMENES QUANTIQUES

Org. by: Départ. de Philosophie, Université de Bretagne Occidentale

Talk: *Une théorie quantique causale dans l'espace-temps est-elle possible?*

C38. **1989**, 9-10 June, Heidelberg, Germany

1st PHILOSOPHY AND PHYSICS WORKSHOP

Talk: *Quantum paradoxes and physical reality*

C39. 1989, 5-14 August, Erice, Italy, Ettore Majorana Center

SIXTY TWO YEARS OF UNCERTAINTY: HISTORICAL,
PHILOSOPHICAL AND PHYSICS INQUIRIES INTO THE
FOUNDATIONS OF QUANTUM MECHANICS

Talk: *Quantum wave effects in triple-slit experiments*

C40. **1989**, 18-23 September, Gdansk, Poland

PROBLEMS IN QUANTUM PHYSICS; GDANSK 1989

Org. by: Institutes of Physics of Gdansk University

Talk: *Strong and Weak Freedman's Inequalities* (With V.L. Lepore)

C41. **1989**, 13-14 October, Rome, Italy

LA VERITA' NELLA SCIENZA,

Org. by: Accademia Nazionale dei Lincei

Talk: *Is a causal quantum physics in space and time possible?*

C42. **1989**, 15-16 December, Heidelberg, Germany

2nd PHILOSOPHY AND PHYSICS WORKSHOP-THE CONCEPT OF
NATURE AND THE MEANING OF REALITY IN MODERN PHYSICS

Org. by: FEST

Talk: *Probabilistic local realism and quantum theory*

- C43. **1989**, 28 December - 2 January 1990, New Delhi, India
 IN SEARCH OF QUANTUM REALITY- INTERNATIONAL
 CONFERENCE ON THE CONCEPTUAL FOUNDATIONS OF
 QUANTUM THEORY
 Org. by: National Institute of Science, Technology and development studies
 of India
 Talk 29/12: *EPR paradox and wave-particle duality*
- C44. **1990**, 3-8 September, London, U.K.
 PHYSICAL INTERPRETATIONS OF RELATIVITY THEORY
 Org. by: British Society for the Philosophy of Science
 Talk: *Special Relativity as a limit of ether theories*
- C45. **1991**, 6-7 June, Bremen, Germany
 SYMPOSIUM ON FOUNDATIONS OF QUANTUM THEORY
 Org. by: Physics Department of Bremen University
 Talk: *New EPR experiments with $K^0\bar{K}^0$ decays*
- C46. **1991**, 16-24 September, Bodrum, Turkey
 FUNDAMENTAL UNSOLVED PROBLEMS OF PHYSICS
 Org. by: Turkish Physical Society
 Talk: *Elementary particle physics and quantum theory*
- C47. **1991**, 7-10 October, Cesena, Italy
 BELL'S THEOREM AND THE FOUNDATIONS OF MODERN PHYSICS-
 INTERNATIONAL CONFERENCE IN MEMORY OF JOHN BELL
 Org. by: INFN, City of Cesena and Physics Department of Bari University,
 Commission of the European Communities
 Talk: *Einstein-de Broglie waves and two-photon detection*

C48. **1992**, 6-12 September, Lodz, Poland

SUMMER SCHOOL ON PROGRESS IN NEW COSMOLOGIES

Org. by: Jagellonian University of Krakow

Talk: *Special relativity and ether theories*

C49. **1992**, 24-30 September, Trani, Italy

WAVES AND PARTICLES IN LIGHT AND MATTER-WORKSHOP IN
OCCASION OF LOUIS DE BROGLIE'S 100TH ANNIVERSARY

Org. by: Physics Department of Bari University

Talk: *The role of de Broglie waves in the detection of particles*

C50. **1993**, 26-28 April, Lisbona, Portugal

Encontro regional da sociedade portuguesa de matematica

Talks: *Geometria e aplicações*

Red shift quantization

C51. **1993**, 29 August- 3 September, Oviedo, Spain

FUNDAMENTAL PROBLEMS IN QUANTUM PHYSICS

Org. by: Physics Department of Oviedo University

Talk: *Elements of reality for neutral kaons*

C52. **1993**, 27-30 September, Olympia, Greece

FRONTIERS OF FUNDAMENTAL PHYSICS

Org. by: NRC Demokritos (Athens) and Physics Dept. of Bari University

Talk: *Theories equivalent to special relativity*

C53. **1994**, 13-16 June, Helsinki, Finland

SYMPOSIUM ON THE FOUNDATIONS OF MODERN PHYSICS 1994-
70 YEARS OF MATTER WAVES

Org. by: Physics Dept. of Helsinki University

Talk: *Bohr's complementarity and quantum physics*

C54. **1994**, 18-22 June, Baltimore, U.S.A.

FUNDAMENTAL PROBLEMS IN QUANTUM THEORY- IN HONOR OF
JOHN A. WHEELER

Org. by: New York Academy of Sciences

Talk: Local propensities and quantum theory

C55. **1994**, 23 June, Montréal, Canada

Talks: The EPR paradox for neutral kaon pairs

Bohr's complementarity and de Broglie waves

C56. **1994**, 24 June, Ottawa, Canada

Talk: Theories equivalent to special relativity

C57. **1994**, 29 August-3 September, Lodz, Poland

XIV CRACOW SUMMER SCHOOL OF COSMOLOGY

Org. by: University of Lodz

Talks 03/09: Existence of theories equivalent to special relativity

C58. **1994**, 19-21 October, Fortaleza, Brazil

XII ENCONTRO DE FISICOS DO NORTE E NORDESTE

Org. by: Universidade Federal do Ceará

Talk: Problems in the Foundations of Quantum Mechanics

C59. **1994**, 24-25 October, Fortaleza, Brazil

WORKSHOP ON THE FOUNDATIONS OF QUANTUM THEORY

Org. by: Universidades de Alagoas e do Ceará

Talk: Overcoming complementarity

C60. **1995**, 1-2 July, München, Germany

MEETING ON WAVE ASPECTS OF PHYSICS AND COSMOLOGY

Org. by: Max-Planck-Institut für Astrophysik

Talk: *Inertial transformations and the Sagnac effect*

C61. **1995**, 19-25 August, Florence, Italy

10th INTERN. CONGRESS OF LOGIC, METHOD., PHILOS. OF
SCIENCE

Org. by: International Union of History and Philosophy of Science

Round Table: *Quantum Measurement, Complex Systems and Physical Reality*

C62. **1996**, 15-20 July, Goslar, Germany

XXI INT. COLLOQUIUM ON GROUP THEORETICAL METHODS IN
PHYSICS

Talk: *Locally equivalent accelerated and inertial reference frames*

C63. **1996**, 21-26 July, Oviedo, Spain

2nd SYMPOSIUM ON FUNDAM. PROBLEMS IN QUANTUM PHYSICS

Org. by: University of Oviedo

Talk 23/07: *Noninvariant one-way velocity of light and locally equivalent
reference frames*

C64. **1996**, 5-8 September, Cambridge, England

18th ANNUAL INTERN. MEETING OF THE ALTERNATIVE NATURAL
PHILOSOPHY ASSOCIATION

Talk: *The rotating platform problem*

C65. **1996**, 6-9 September, Imperial College, London, England

PHYSICAL INTERPRETATIONS OF RELATIVITY THEORY

Org. by: British Society for the Philosophy of Science

Talk: *On the necessity of a noninvariant one-way speed of light in
relativistic physics*

C66. **1996**, 23-29 September, St.-Petersburg, Russia

PROBLEMS OF SPACE, TIME AND MOTION

Org. by: Russian Academy of Sciences

Talk: *Noninvariant one-way velocity of light*

C67. **1997**, 21-24 April, Rome, Italy

PHENOMENOLOGY OF UNIFICATION FROM PRESENT TO FUTURE

Talk 21/04: *Incompatibility between Quantum Mechanics and Local Realism for K^0 Pairs*

C68. **1997**, 25-26 April, Sevilla, Spain

PROBLEMAS CONCEPTUALES EN FISICA CUANTICA Y RELATIVIDAD

Org. by: Universidad de Sevilla

Talk: *Ether theories and the relativity principle*

C69. **1997**, 25-28 June, Athens, Greece

RELATIVISTIC PHYSICS AND SOME OF ITS APPLICATIONS

Talk: *Mathematical and physical discontinuity in relativity. Consequences of its elimination*

C70. **1997**, 25-30 August, Köln, Germania

Conference "PHYSICS AS A SCIENCE"

Org. by: Society for the Advancement of Physics

Talk: *On the discontinuity between accelerated and inertial frames*

C71. **1998**, 5-9 January, Delhi, India

International Symposium "UNCERTAIN REALITY: THE QUANTUM WORLD-VIEW AT SEVENTY"

Org. by: Indian National Inst. of Science, Techn. & Development

Talk: *Some remarks concerning Heisenberg's indeterminacy relations*

C72. **1998**, 4 dicembre, Lisbona, Departamento de Fisica da Universidade de Lisboa

Org. by: Departamento de Fisica da Universidade de Lisboa

Talk: *The one-way velocity of light*

C73. **1998**, 28 December-01 January 1999, New Delhi (India)

International Symposium: FROM DUALITY TO UNITY: 75 YEARS OF WAVE-PARTICLE DUALITY.

Org. by: National Institute of Science, Technology & Development Studies.

Talk: *Wave-particle duality and EPR paradox: what they have in common*

C74. **1999**, 2-5 January, Bombay/ Mumbai (India)

International Symposium: RECENT DEVELOPMENTS IN THEOR. PHYSICS

Org. by: Tata Institute of Fundamental Research

Talk: *Open questions in relativistic physics*

C75. **1999**, 27-30 May, Puebla (Messico)

VII International Workshop: NEW TRENDS IN PHYSICS TEACHING

Org. by: Benemerita Universidad Autonoma de Puebla

Talk 30/05: *Physics evolving: the struggle against dogmatism*

C.76. **1999** 1-4 June, PUEBLA (Messico),

Curso Corto: FOUR BASIC QUESTIONS OF MODERN PHYSICS

Org. by: Benemerita Universidad Autonoma de Puebla, Instituto de Física
"Luis Rivera Terrazas

Talks:

1. *Relativity and the nature of time*
2. *Theories equivalent to special relativity*
3. *Quantum theory beyond indeterminacy*
4. *The Einstein, Podolsky, Rosen paradox*

C77. **1999**, 6-8 September, Physikzentrum Bad Hohnef (Germania)

AKTUELLE ENTWICK. IN DER GRAVITATIONSTHEORIE

Org. by: Deutsche Physikalische Gesellschaft

Talk: Rotating platforms and the definition of time in inertial systems

C78. **1999**, 17-20 September, Cesena (Italia)

REDSHIFT + GRAVITATION IN A RELATIVISTIC UNIVERSE

Org. by: Apeiron Press (Montreal) + Città di Cesena

Talk: Theories equivalent to Special Relativity

C79. **1999**, 22-28 November, Cracow (Polonia)

First Cracow - Clausthal Workshop: TUNNELLING EFFECT AND
OTHER FUNDAMENTAL PROBLEMS OF QUANTUM PHYSICS

Org. by: Inst. of Nuclear Physics Cracow + A. Sommerfeld Inst. Clausthal

Talks: Theories equivalent to Special Relativity - I

Theories equivalent to Special Relativity - II

C80. **2000**, 22 March, College Park (USA)

Org. by: University of Maryland, Baltimore, USA

Talk: Theories equivalent to Special Relativity-I

C81. **2000**, 23 March, Catonsville, Baltimore County (USA)

Org. by: University of Maryland

Talk: Theories equivalent to Special Relativity-II. A Theory with
Noninvariant One Way Speed of Light

C82. **2000**, 3-8 June, Storrs, Connecticut (USA)

THE NEW NATURAL PHILOSOPHY: AN INTRODUCTION TO 21st
CENTURY PHYSICS AND COSMOLOGY

Org. by: Natural Philosophy Association (Boston) + Univ. of Connecticut

Talk 07/06: Space and Time are better than Spacetime I and II

- C83. **2000**, 27 - 31 August, Köln, Germania
 2nd COLOGNE WORKSHOP: PHYSICS AS A SCIENCE-
 INTERNATIONAL WORKSHOP FOR THE BENEFIT OF PROGRESS IN
 FUNDAMENTAL RESEARCH
 Org. by: Society for the Advancement of Physics
 Talk: *Superluminal signals and absolute simultaneity*
- C84. **2000**, 11 - 16 September, Menorca Illa del Rei, Instituto Luis Vives
 Workshop: LA CIENCIA EN EL MUNDO: EXPECTATIVAS Y
 RESPONSABILIDADES
 Org. by: Univ. Intern. de Menorca Illa del Rei, Mahon, Spagna
 Talk 12/09: *La fisica moderna e i limiti della conoscenza scientifica* (In
 Italian)
- C85. **2000**, 15 - 18 September, Imperial College, London
 PHYSICAL INTERPRETATIONS OF RELATIVITY THEORY - VII
 Org. by: British Society for the Philosophy of Science, S.C.E.T., University
 of Sunderland
 Talk: *Superluminal signals and absolute simultaneity*
- C86. **2000**, 12 - 15 October, Crakow, Polonia
 2nd Cracow Workshop: QUANTUM INFORMATION AND OTHER
 FUNDAM. PROBLEMS OF QUANTUM PHYSICS
 Org. by: Institute of Nuclear Physics
 Talk 13/10: *The physics of space and time without space-time*
- C87. **2000**, 10 - 14 November, Vienna, Austria
 John Bell Conference QUANTUM [UN]SPEAKABLES
 Org. by: The Erwin Schrödinger Institute
 Talk 14/11: *Theories equivalent to Special Relativity*

C88. **2000**, 15 - 19 November, Vienna, Austria

Workshop: QUANTUM MEASUREMENT AND INFORMATION

Org. by: Institute of Experimental Physics, University of Vienna

Talk 16/11: Quantum mechanics versus local realism for neutral kaon pairs

C89. **2000**, 20 - 21 November, Universidad de Zaragoza, Spain

School of physics: CIEN AÑOS DE FISICA CUÁNTICA

Talk 1 20/11: The relativity principle and the nature of time

Talk 2 21/11: Recent developments on the EPR paradox

C90. **2000**, 22 - 25 November, Universidad Complutense, Madrid, Spain

100 YEARS OF QUANTUM THEORY-HISTORY, PHYSICS AND PHILOSOPHY

Talk 23/11: The Einstein-Podolsky-Rosen paradox: Truth and Fiction

C91. **2001**, 12-15 June, Pisa (Italy)

International Conference: KAON 2001 on CP Violation

Org. by: Istituto Nazionale di Fisica Nucleare

Talk: Possible EPR experiments with neutral K and B pairs

C92. **2001**, 23-29 August, Moscow State University

Xth Lomonosov Conference on elementary PP

Org by: A Studenikin et al.

Talk: The EPR Paradox in Particle Physics

C93. **2002**, 4-11 August, Bedlewo (Polonia)

IDEAS OF ALBERT ABRAHAM MICHELSON IN MATHEMATICAL PHYSICS

Org. by: Mathematical Conference Centre at Bedlewo

Talk: Superluminal signals require a modified relativity

- C94. **2002**, 11-17 August, Venice International University (Italia)
 TIME AND MATTER. COLLOQUIUM ON THE NATURE OF TIME
 Org. by: University of Notre Dame du Lac, Indiana, USA, Ludwig-
 Maximilians-Universität München, Germany
 Talk 13/08: Relativistic physics and the nature of time
- C95. **2002**, 6-9 September, Civil Engineering Department Imperial College,
 London, UK
 PHYSICAL INTERPRETATION OF RELATIVITY THEORY - VIII
 Org. by: British Society for the Philosophy of Physics
 Talk. Our absolute velocity
- C96. **2005**, 23-25 June, Monção, Portugal
 1ST CRISIS IN COSMOLOGY CONFERENCE
 SESSION 2-CONCEPTUAL DIFFICULTIES OF THE PRESENT MODEL
 Talk: Absolute simultaneity forbids the big bang
- C97. **2007**, 7-9 September, Budapest
 MATHEMATICS, PHYSICS AND PHILOSOPHY IN THE
 INTERPRETATIONS OF RELATIVITY THEORY
 Talk: The clock paradox: are special and general relativity compatible?



DIPARTIMENTO DI FISICA
UNIVERSITÀ DEGLI STUDI DI BARI

Via G. Amendola, 173
70126 BARI (Italia)

LIST OF INTERNATIONAL CONFERENCES
ON THE FOUNDATIONS OF QUANTUM MECHANICS

1. Varenna (Italy) FOUNDATIONS OF QUANTUM MECHANICS (1973)
International School of Physics "Enrico Fermi", IL Course.
2. Erice (Italy) TESTING QUANTUM MECHANICS (1976)
"Thinkshops on Physics", April 1976
3. Geneve (Switzerland) QUANTUM INDETERMINISM AND HIDDEN VARIABLES
(1979) Institut de la Méthode.
4. Athens (Greece) INTERNATIONAL MEETING IN EPISTEMOLOGY (1982)
Mostly devoted to foundations of quantum mechanics.
5. Perugia (Italy) WAVE-PARTICLE DUALITY (1982)
Fondation Louis de Broglie, Bari University, Perugia University.
6. Bari (Italy) OPEN QUESTIONS IN QUANTUM PHYSICS (1983)
Istituto Nazionale di Fisica Nucleare and Bari University.
7. Tokyo (Japan) FOUNDATIONS OF QUANTUM MECHANICS IN THE LIGHT
OF NEW TECHNOLOGY (1982) Hitachi, Tokyo.
8. Athens (Greece) INTERNATIONAL MEETING ON EPISTEMOLOGY (1984)
2nd meeting, also mostly devoted to Foundations of quantum physics.
9. Joensuu (Finland) SYMPOSIUM ON THE FOUNDATIONS OF MODERN PHYSICS
"50 years of the Einstein-Podolsky-Rosen Gedankenexperiment"(1985)
10. Urbino (Italy) MICROPHYSICAL REALITY AND QUANTUM FORMALISM (1985)
Istituto Nazionale di Fisica Nucleare and Bari University.
11. Rome (Italy) INTERNATIONAL SYMPOSIUM ON NIELS BOHR (1985)
Accademia dei Lincei, Academy of Denmark
12. Bridgeport (USA) QUANTUM VIOLATIONS: EXPERIMENTS AND INTERPRET.
NATO Advanced Research Workshop (1986)

13. New York (USA) NEW TECHNIQUES AND IDEAS IN QUANTUM MEASUREMENT THEORY (1986) New York Academy of Sciences.
14. Tokyo (Japan) FOUNDATIONS OF QUANTUM MECHANICS IN THE LIGHT OF NEW TECHNOLOGY (1986), 2nd meeting, Hitachi, Tokyo.
15. Gdansk (Poland) PROBLEMS IN QUANTUM PHYSICS (1987) University of Gdansk.
16. Athens (Greece) THE CONCEPT OF PROBABILITY (1987) Hellenic Ministry of Culture and Civilization, devoted to q.m.
17. Paris (France) LOUIS DE BROGLIE, PHYSICIEN ET PENSEUR (1987) Université Européenne de la Recherche.
18. Calcutta (India) SYMPOSIUM DEDICATED TO N. BOHR (1987) Calcutta Mathematical Society.
19. Dubna (USSR) FOUNDATIONS OF QUANTUM MECHANICS (1988) Joint Institute for Nuclear Research.
20. Fairfax (USA) BELL'S THEOREM, QUANTUM THEORY, ETC. (1988) George Mason University.
21. Delhi (India) IN SEARCH OF QUANTUM REALITY (1988) National Institute of Science, Technology and Development Studies.
22. Rome (Italy) QUANTUM THEORY WITHOUT STATE VECTOR COLLAPSE (1989) Universities of Rome and Nice.
23. Brest (France) LA RATIONALITE' DES PHENOMENES QUANTIQUES (1988) Université de Bretagne Occidentale.
24. Erice (Italy) SIXTY-TWO YEARS OF UNCERTAINTY (1988) "Ettore Majorana" Centre.
25. Tokyo (Japan) FOUNDATIONS OF QUANTUM MECHANICS IN THE LIGHT OF NEW TECHNOLOGY (1989) 3rd Symposium, Hitachi Tokyo.
26. Joensuu (Finland) FOUNDATIONS OF MODERN PHYSICS (1990) University of Joensuu
27. Gdansk (Poland) PROBLEMS IN QUANTUM PHYSICS (1989) University of Gdansk.
28. Heidelberg (BRD) THE CONCEPT OF NATURE AND THE MEANING OF REALITY (1989) FEST, Heidelberg, mostly devoted to found. q.m.

29. Columbia (USA) FUNDAMENTAL ASPECTS OF QUANTUM THEORY (1989)
University of South Carolina.
30. Bremen (BRD) FOUNDATIONS OF QUANTUM THEORY (1991)
University of Bremen.
31. Geneva (Switzerland) SYMPOSIUM OF QUANTUM PHYSICS (1991)
CERN, in memory of John S. Bell.
32. Cesena (Italy) BELL'S THEOREM AND THE FOUNDATIONS OF MODERN
PHYSICS (1991) Istituto Nazionale di Fisica Nucleare, Città
di Cesena, Commission of the European Communities.

PARTICIPATIONS AT ITALIAN CONFERENCES

N1. **1974**, 4-6 June, Frascati, Laboratori Nazionali, Italy

MUTAMENTI E PROSPETTIVE DELLA FISICA FONDAMENTALE: I
FONDAMENTI DELLA MECCANICA QUANTISTICA

Org. by: FNL, Physics Department of Bari University, INFN

Talk: Esperimenti sui fondamenti della meccanica quantistica

N2. **1977**, Torino, Italy

Workshop: FOUNDATIONS OF QUANTUM MECHANICS

Talks:

Superamento del teorema di von Neumann

Causalità locali e considerazioni sperimentali

N3. **1983**, 3-5 November, Como, Italy

IV CONGRESSO NAZIONALE DI STORIA DELLA FISICA

Org. by: Gruppo Nazionale di Coordinamento per la Storia della Fisica del
C.N.R.

Talk: Storia recente della causalità

N4. **1984**, 4-5 May, Bari, Italy

CONVEGNO DI STUDI SU GEORGE ORWELL

Org. by: Institute of English Literature of Bari University

Talk: Scienza e potere in 1984

N5. **1984**, 25-27 October, Reggio Emilia, Italy

LEOPOLDO NOBILI E LA CULTURA SCIENTIFICA DEL SUO TEMPO

Org. by: Italian Physical Society

Talk: Nobili e l'elettromagnetismo classico

- N6. **1984**, 29 October - 1 November, Rome, Italy
 V CONGRESSO NAZIONALE DI STORIA DELLA FISICA
 Org. by: C.N.R. Gruppo Nazionale di Storia della Fisica
 Talk: *Il rapporto fra matematica e realtà nella storia della fisica teorica*
- N7. **1985**, 11-13 April, Cesena, Italy
 IL PARADOSSO DELLA REALTA' FISICA
 Org. by: City of Cesena and It. Society of Logic and Philosophy of Science
 Talk: *E' possibile una fisica causale nello spazio e nel tempo?*
- N8. **1987**, 19-21 November, Rome, Italy
 THE CONSTRUCTION OF NATURE AND THE RESPONSIBILITY OF
 SCIENTIFIC KNOWLEDGE,
 Org. by: 2nd University of Rome
 Talk: *The social responsibility of the theoretical physicist*
- N9. **1988**, 24-25 October, Bologna, Italy
 INTERNATIONAL INTERDISCIPLINARY SYMPOSIUM IN HONOUR
 OF GIAMPIETRO PUPPI,
 Org. by: Physics Dept. of Bologna University
 Talk: *The unsatisfactory foundations*
- N10. **1988**, 31 October - 3 November, Camerino, Italy
 I FONDAMENTI DELLA MECCANICA QUANTISTICA; ANALISI
 STORICA E PROBLEMI APERTI
 Org. by: Department of Mathematics and Physics of Camerino University
 Talk: *I fenomeni quantistici nello spazio-tempo*
- N11. **1989**, 12-16 May, Rome, Italy
 SCIENTIFIC MODELS OF PHYSICAL REALITY
 Org. by: 2nd University of Rome
 Talk: *Is Bell's inequality violated in nature?*

N12. **1989**, 15-16 May, Bari, Italy

I MODELLI SCIENTIFICI DELLA REALTA' FISICA

Org. by: Physics Department of Bari University

Talk: *I due modelli della realtà*

N13. **1989**, 27-29 September, Perugia, Italy

FOUNDATIONS OF MATHEMATICS AND PHYSICS

Org. by: Mathematics Department of Perugia University

Talk: *On the possibility of a rationalistic approach to microphysics*

N14. **1992**, 1-4 June, Bari, Italy

QUANTA, PROBABILITY AND MEASUREMENTS

Org. by: Physics Department of Bari University

Talk: *Variable detection probability explains recent Mandel experiments*

N15. **1993**, 19-20 March, Cesena, Italy, Centro S. Biagio

FORMALISMO MATEMATICO E REALTA' FISICA

Talk: *La relatività e l'etere*

N16. **1993**, 5-9 October, Lecce, Italy

I FONDAMENTI DELLA MECCANICA QUANTISTICA. ANALISI
STORICA E PROBLEMI APERTI,

Org. by: Physics Dept. of Lecce University

Paper: *Three main forms of the quantum measurement theory*

N17. **1993**, 19-20 November, Milano, Italy

QUALE FUTURO PER LA SCUOLA ITALIANA?

Org. by: Istituto L. Geymonat e Museo Civico di Storia Naturale

Talk: *La fisica e la sua storia*

N18. **1994**, 26-27 September, Cesena, Italy, Centro di Epistemologia

CONVEGNO DI FONDAMENTI E FILOSOFIA DELLA FISICA

Org. by: Bologna and Urbino Universities

Talk: Considerazioni sulla teoria della relatività

N19. **1994**, 27-30 October, Cesena, Italy, Palazzo del Ridotto

L'EPISTEMOLOGIA DI KARL POPPER

Org. by: Università Bologna, Università di Urbino e Comune di Cesena

Talk: Le propensioni di Popper e il probabilismo nella fisica moderna

N20. **1994**, 19 August, Cesena, Italy

Conference on Prigogine: QUANTUM MEASUREMENTS COMPLEX
SYSTEMS PHYSICAL REALITY

Org. by: Università Bologna, Università di Urbino e Comune di Cesena

Talk: La natura del tempo e la velocità della luce sulla Terra

N21. **1996**, 4-7 September, Perugia, Italy

CONVEGNO SU CARTESIO E LA SCIENZA

Org. by: Mathematics Department of Perugia University

Talk: Le teorie equivalenti alla relatività e la natura del tempo

N22. **1996**, 21-23 September, Cesena-Urbino, Italy

SIMMETRIE TEMPORALI E IRREVERSIBILITA' DELL' ESPERIENZA

Org. by: Società italiana di logica e filosofia della scienza

Talk: Irreversibilità temporale e principio di relatività

N23. **1997**, 17-19 April, Trieste, Italy

1° CONGRESSO SIFF-FONDAMENTI DELLA FISICA E FRONTIERE
DELLA CONOSCENZA

Org. by: Società Italiana di Fondamenti della Fisica

Talk.: Discontinuità in relatività

N24. **1997**, 20-25 October, Bologna, Cesena, Urbino, Italy

LA FILOSOFIA DELLA FISICA IN ITALIA (1897-1997)-L'INDAGINE
SUI FONDAMENTI DAL DIBATTITO STORICO ALLE NUOVE
PROSPETTIVE DI RICERCA

Org. by: Società italiana di logica e filosofia della scienza

Talk: *Il realismo come guida nei problemi della fisica moderna*

N25. **1998**, 5-6 May, Pisa, Domus Galileiana

INTERPRETAZIONI REALISTICHE DELLA MECCANICA
QUANTISTICA: SONO ANCORA ATTUALI?

Org. by: Centro Interdisciplinare per lo studio dei fenomeni complessi

Talk: *Il paradosso di Einstein, Podolsky e Rosen: sviluppi recenti*

N26. **1998**, 15-16 May, Como, Centro Volta, Villa Olmo

XVIII CONGRESSO NAZIONALE DI STORIA DELLA FISICA E
DELL'ASTRONOMIA

Org. by: Gruppo del CNR di Storia della Fisica

Talk: *Nota sul principio di continuità fra sistemi inerziali e sistemi
accelerati-esistenza di una discontinuità matematica e fisica nella teoria
della relatività*

N27. **1998, CESENA**, 14-18 settembre 1998, Centro di Epistemologia

SCUOLA ESTIVA DI FILOSOFIA DELLA SCIENZA

Org. by: Università di Bologna, Università di Urbino e Comune di Cesena

Corso di FS: "Fondamenti della meccanica quantistica"

Talks:

1. *I disaccordi fra i padri fondatori*
2. *I paradossi della completezza*
3. *Superamento delle relazioni di indeterminazione*
4. *Onde, corpuscoli, dualità e dualismo*
5. *Il paradosso di Einstein, Podolsky e Rosen*

- N28. **1998**, 19 September, Cesena, Palazzo del Ridotto
Workshop: PHILOSOPHY OF QUANTUM MECHANICS
Org. by: SILFS (Società Italiana di Logica e Filosofia della Scienza),
Università di Bologna, Università di Urbino e Comune di Cesena
Talk: *From de Broglie waves to relativity: a necessary step*
- N29. **1998**, 25-26 September, Reggio Calabria, Accademia delle Belle Arti
CONVEGNO SULLA STORIA DELL'ASTRONOMIA
Org. by: Società Astronomica Italiana
Talk: *Da uno spunto di J. Jeans alcuni sviluppi attuali di fisica relativistica*
- N30. **1998**, 13-16 October, Lecce, Hotel President
I FONDAMENTI DELLA MECCANICA QUANTISTICA. ANALISI
STORICA E PROBLEMI APERTI
Org. by: Dipartimento di Fisica dell'Università di Lecce
Talk: *E' la velocità della luce di sola andata misurabile?*
- N31. **1999**, 15 February, Cesena, Centro di Epistemologia, Palazzo del
Ridotto
NUOVE RISPOSTE AI PROBLEMI DELLA RELATIVITA'
Org. by: Università di Bari e Comune di Cesena
Talk: *L'esperimento di Miller e la misurabilità della velocità della luce di
sola andata*
- N32. **1999**, 13 April, Cesena, Centro di Epistemologia
IL PROBLEMA DELLA CONOSCENZA
Org. by: Università di Urbino e Comune di Cesena
Talk: *La fisica del 900 e le sconfitte della conoscenza*

N33. **1999**, 14 May, Pisa, Domus Galileiana

I PARADOSSI DELLA FISICA MODERNA: I PARADIGMI DELLA
FISICA DEL XX SECOLO SONO ANCORA SUFFICIENTI?

Org. by: Centro Interdisciplinare per lo studio dei fenomeni complessi

Talk: Le teorie equivalenti alla relatività speciale

N34. **1999**, 13-17 September, Cesena, Centro di Epistemologia

II SCUOLA ESTIVA DI FILOSOFIA DELLA FISICA

Org. by: Università di Bologna, Università di Urbino e Comune di Cesena

Corso di FS: “Nuove tendenze in fisica relativistica”

Talks:

1. La relatività secondo Einstein e secondo Lorentz

2. Relatività e relativismo

3. Le teorie equivalenti alla relatività speciale

4. Piattaforme ruotanti e trasformazioni inerziali

5. La situazione sperimentale

N35. **1999**, 17-20 September, Cesena, Palazzo del Ridotto

REDSHIFT AND GRAVITATION IN A RELATIVISTIC UNIVERSE

Talk: Theories equivalent to relativity

N36. **1999**, 25-27 September, Cassano (BA)

CONFERENZA “ARTE E SCIENZA”

Org. by: Centro di Ontosofia Psicosomatica

Talk: Antagonismo e compromesso fra scienza e religione: cenni storici

N37. **1999**, 12-16 December, Bari

XI CONGRESSO NAZIONALE di DIDATTICA CHIMICA

Org. by: Società Chimica Italiana-Divisione di didattica

Talk: Le oggettive difficoltà del giornalismo scientifico

Round table: La divulgazione scientifica

N38. **2000**, 28 March, Cassano (BA)

CONFERENZA “ARTE E SCIENZA”

Org. by: Centro di Ontosofia Psicosomatica

Talk: Antagonismo e compromesso fra scienza e religione: cenni storici

N39. **2000**, 5 May, Pisa, Domus Galileiana

SCIENZE E FILOSOFIA DELLA SCIENZA: È POSSIBILE UN
DIALOGO?

Org. by: Centro Interdisciplinare per lo studio dei fenomeni complessi.
Dipartimento di Matematica dell'Università di Pisa

Round table: Sui pericoli insiti nelle idee filosofiche estratte dalle teorie
fisiche

N40. **2000**, 22 - 23 September, Cesena, Centro di epistemologia,

CONSEGUENZE FILOSOFICHE DELLA FISICA QUANTISTICA

Org. by: Università di Urbino

Talk: Il realismo locale in meccanica quantistica e in relatività

N41. **2000**, 7-8 October, Cassano (BA)

CONGRESSO INTERNAZIONALE: COSMO, SOGNO, EROS E PSICHE

Org. by: Associazione di Ontosofia Psicosomatica

Talk: Universo in lenta evoluzione, ossia, cosmologia senza il big bang

N42. **2011**, 12-17 September Cesena, Centro Culturale S. Biagio

OLTRE LA FISICA NORMALE

Org. by: Università di Bologna, Università di Urbino e Comune di CesenaFS:

Talk: Una teoria alternativa alla relatività speciale einsteiniana

PARTICIPATIONS AT SEMINARS IN SCHOOLS, ASSOCIATIONS, POLITICAL CIRCLES-ORAZIONI⁹²¹

1975-2004

In this section Selleri submitted some comment in his documents. I left the titles of talks in the original Italian language. This is the field in which Selleri has most varied the nature of the seminars, passing from philosophy, history, and also teaching and updating of teachers. All seminars organized by the Department of Physics are arranged by Selleri himself. At the end of some conference I translated Selleri's personal notes (reason why they are expressed in the first person).

1975 - 1979

S1. BARLETTA, 10/5/75, Sala della biblioteca comunale

Org. by: Associazione Ricreativa Culturale Italiana

Talk: Scienza e tecnologia per lo sviluppo del Mezzogiorno

S2. OSTUNI, 8/5/79, Palazzo di Città

Talk: Albert Einstein, scienziato e filosofo

S3. BARI, 79, Scuola media "G. Galilei"

Talk: Politica dell'energia

S4. BARI, 11/12/79, Mathesis, Institute of Matematica, Palazzo Ateneo

Talk: Energia dal punto di vista scientifico

S5. BARI, 14/12/79, Mathesis, Institute of Matematica, Palazzo Ateneo.

Talk: Il problema dell'energia e la società attuale

⁹²¹ Selleri called them "orazioni", to ironically introduce an emphatic definition for conferences.

1980 - 1984

S6. RUVO, 24/1/80, Liceo scientifico "O. Tedone"

Talk: *Il concetto di energia nella scienza contemporanea*

S7. POLICORO, 5/2/80, Liceo scientifico.

Talk: *Albert Einstein scienziato e filosofo*

S8. MONTALBANO JONICO, 28/2/80, Liceo

Talk: *Albert Einstein scienziato e filosofo*

S9. FOGGIA, 20/3/80, Liceo scientifico "A. Volta"

Talk: *Albert Einstein scienziato e filosofo*

S10. GIOIA DEL COLLE, 27/3/80, Liceo Classico

Talk: *Il concetto di energia nella scienza contemporanea*

S11. GROTTAGLIE, 28/3/80, Liceo scientifico

Talk: *Albert Einstein scienziato e filosofo*

S12. MANDURIA, 29/3/80, Liceo classico "Francesco De Sanctis"

Talk: *Albert Einstein scienziato e filosofo*

The headmaster is Prof. Attilio Scandiffio. He made the second World War in Russia, he is a Socialist and an expert on Giacomo Leopardi. My talk has been broadcast by R.T.M. the day 12/4/80. Then he sent me a copy of *Il Messapico*, local magazine which tells about my speech.

S13. IRSINA (MT), 10/4/80, Liceo scientifico

Talk: *Albert Einstein scienziato e filosofo*

S14. BITETTO, 11/4/80, Liceo scientifico sperimentale “A Scacchi

Talk: *Il concetto di energia nella scienza contemporanea*

S15. BARI, 15/4/80, Liceo Salvemini

Talk: *Albert Einstein scienziato e filosofo*

S16. BARLETTA, 16/4/80, Liceo scientifico

Talk: *Albert Einstein e il concetto di energia*

S17. SAN SEVERO, 17/4/80

Talk: *Albert Einstein scienziato e filosofo*

S18. TRICARICO, 2/5/80, Liceo scientifico

Talk: *Albert Einstein scienziato e filosofo*

S19. MOLA, 8/5/80, Liceo scientifico

Talk: *Albert Einstein scienziato e filosofo*

S20. BARI, 27/5/80, Liceo scientifico

Talk: *Il concetto di energia nella scienza contemporanea*

S21. BARI, 19/6/80, CIDI c/o scuola media G. Laterza

Talk: *L'insegnamento delle scienze fra scientismo e irrazionalismo*

S22. FOGGIA, 1981, Liceo scientifico

Talk: *Scienza e filosofia*

S23. BARLETTA, ?/2/81, Liceo scientifico

Talk: *Scienza e filosofia*

S24. MATERA, 27/2/81, Hotel President

Conference (maybe organized by Selleri): NUOVI PROGRAMMI PER LA SCUOLA MEDIA (D.M. FAB. 79)

Talk: Alcune proposte circa l'insegnamento delle materie scientifiche nella scuola media

S25. BARI, 11/3/81, Università degli Studi di Bari-Aula IV, Facoltà di Lettere. Org. by: CIDI

Talk: Il modello della razionalità scientifica da Galileo a Newton

S26. MANDURIA, 9/5/81, Liceo classico "F. De Sanctis"

Talk: Alcune considerazioni sul rapporto fra scienza e filosofia

S27. BARI, 28/10/81, Circolo Culturale "Terzo Occhio",

Talk: Galileo Galilei

S28. TARANTO, 15/4/82, Centro Vaccarella (Italsider)

Org. by: Consoli (PCI)

Talk: Il nucleare nel piano energetico nazionale

S29. BARI, 4/5/82, IRRSAE, Aula I di Giurisprudenza.

L'INSEGNAMENTO SCIENTIFICO NELLA SCUOLA SECONDARIA SUPERIORE

Talk: La fisica oggi

S30. BARI, 1982, Liceo classico "O. Flacco"

Talk: Teoria dei quanti

S31. NAPOLI, 21/5/82, CIDI, presso il CPE

Talk. La razionalità scientifica nelle scienze della natura

S32. BARI, 8/2/83, Facoltà di Magistero, Aula Levi, Palazzo Ateneo

Org. by: Società Filosofica Italiana-Sezione di Bari

Round table: Le nuove frontiere del sapere: Fisica e Filosofia

.

S33. TARANTO, 18/2/83, Consorzio Universitario Jonico-15-20 teachers

Talk: Che cos'è l'energia

S34. BARI, 13/5/83, Liceo Socrate

Talk: Einstein e la relatività

S35. ANDRIA, 14/5/83, Liceo Scientifico

Talk: Università di Bari e sbocchi professionali

S36. BARI, 19/5/83, Liceo classico "O. Flacco"

Talk: Einstein e la relatività

S37. GIOIA DEL COLLE, 21/5/83, Comitato per la pace, presso CRSEC.

Talk against the nuclear war

S38. BARI, 5/83, C.I.D.I.

Talk in favour of nuclear energy for civil use

S39. BARI, 5/83, Istituto Tecnico Statale per Geometri. Centro Studi Polivalente.

Talk in favour of nuclear energy for civil use

S40. POLICORO, 26/5/83, Prolusion to the cinema for the naming of the Scientific High School to Enrico Fermi

Talk: Enrico Fermi: la Fisica e la Società

S41. FIGLINE VALDARNO (FI), 7/10/83, Biblioteca comunale Marsilio Ficino.

Talk: Che cos'è l'energia (presentation of the eponymous book)

S42. BARI, 10/83, Liceo classico “Socrate”

Talk: Rischio di guerra nucleare

S43. PALERMO, 24-27/10/83, Istituto Gramsci siciliano-Aula Magna della
Facoltà di Ingegneria

International Conference on MARX E LE SCIENZE

Talk: Forse si può uscire dal tunnel: spunti per una rifondazione del
materialismo

(other speakers: Bellone, Bitsakis, Cini, Tagliagambe)

S44. CANOSA, 11/83, Liceo scientifico

Talk: *I rischi di un conflitto nucleare*

I sent the fee back to high school because I didn't want to get paid for a Peace
conference.

S45. ADELFA, 20/12/83, Sala comunale. Demonstration-debate for peace.

My conclusions.

S46. SAMMICHELE, 4/1/84, Peace debate at the local Castle, organized by
the local section of the PCI, with a movie on Hiroshima. I made the
introduction and the conclusions.

S47. MODUGNO, 14/1/84, Galleria L' Arcaccio.

Org. By: Nuovi Orientamenti

Round table: La paura nucleare come scelta strategica

S48. BITETTO, 24/1/84, Liceo scientifico sperimentale “A Scacchi”

Talk: Fermi, la fisica e la società

S49. GROTTAGLIE, 15/3/84, Liceo scientifico “Giuseppe Moscati”.

Talk: Scienza e filosofia

S50. ORTANOVA, 30/3/84, Comitato per la Pace.

Talk on USPID slides

S51. CASTELLANA, 12/4/84, Istituto Tecnico Industriale “Dell’Erba”

Conference on THE ENERGY ISSUES.

The young headmaster is pro-nuclear, the teacher who sent me is instead antinuclear but with spaces for dialogue.

S52. BARI, 3/5/84, Libreria De Gemmis

Round table: *Scienza, Società, Linguaggio nell’utopia di Orwell*

S53. BARI, 4/5/84, Liceo scientifico “G. Salvemini”

Talk: *Sviluppi della fisica nucleare negli anni ‘30*

S54. BARI, 9/5/84, Liceo “Socrate”

Talk on Galileo, Keplero, and Newton

S55. GIOIA DEL COLLE, 10/5/84, Liceo Classico “Virgilio”.

Talk against nuclear weapons

S56. BARI, 25/5/84, Hotel Jolly

Debate on regional energy issues

S57. BITONTO, 6/6/84, Circolo cult. FGCI-European election-50 people-many speeches, real interest

Talk against nuclear weapons

S58. ANDRIA, 7/6/84, Saletta Albergo dei Pini- PCI rally for European election- about 50 people. 30 old men who leave halfway through. 10-20 young people interested

Talk against nuclear weapons

S59. NOCI, 8/6/84, PCI rally for European election- Lots of people standing attentive. Good.

Talk against nuclear weapons

S60. BISCEGLIE, 9/6/84, PCI rally in the public square-European election-about 100 people standing, alert, but very few young people.

Talk against nuclear weapons

S61. MONOPOLI, 11/6/84, PCI rally in the public square-European election-more than 100 people, real interest

Talk on Berlinguer and against nuclear weapons

I sent information for buying slides [USPID?] on peace

S62. TRANI, 12/6/84, PCI rally in the public square-European election-about 100 people initially partly distracted. Party itself not so motivated

Talk against nuclear weapons

S63. MOLFETTA, 16/11/84, Casa per la pace

Talk on peace

S64. RUVO, 18/12/84, Liceo scient. "O. Tedone".

Talk against nuclear weapons

1985-1989

S65. BARI, 18/1/85, Presence here of students by Istituto Tecnico Commerciale per Geometri "Luigi Pinto"-Castellana.

Talk against nuclear weapons

S66. BITRITTO, 16/2/85, Associazione Culturale l'Incontro-nice debate-about 50 people

Talk against nuclear weapons

S67. ROMA, 2/3/85, Palazzo Barberini-very nice debate-few people

Org. by: Centro Culturale Interstampa. Biocca

Round table: La pace e l'uso irrazionale del sapere scientifico

S68. RUVO, 12/3/85, Libreria cooperativa "Agorà"-few people

INVITO ALLE SCIENZE.

Talk: Una scienza per l'uomo. Tecnologie, comunicazione, uso sociale del sapere (with Fistetti)

S69. MOLA, 23/3/85, Liceo scientifico

Two-voice crowded meeting with Beppe Nardulli and me, against and in favour of the nuclear energy, respectively.

S70. BARI, 28/3/85, Department of Physics

Org. by: USPID

Talk: Sottomarini con armamento nucleare

(with Fieschi, Nardulli, De Marzo, Cufaro, Maestro, Cotta Ramusino).

S71. ROMA, 4/5/85, Campidoglio, Sala della Protomoteca

International workshop: DIRITTO ALLA PACE

Talk: La militarizzazione dello spazio e gli aspetti scientifici e umani

S72. TARANTO, 9/5/85, Subfor Assemblea di 350 studenti di una scuola media superiore-about 350 students

Talk in favour of nuclear energy

S73. RUVO, 29/10/85, Liceo scientifico "O. Tedone"

Talk: L'insegnamento delle scienze fra storicismo e scientismo

S74. BARI, 10/12/85, Liceo classico "O. Flacco"

TALK: Matematica e realtà

S75. CASTELLANA, 18/3/86, Istituto Tecnico Commerciale per Geometri “Luigi Pinto”-full theater, about 300 people, very attentive, but little debate

Talk: Star Wars

S76. MONOPOLI, 3/4/86, Liceo scientifico “M. Curie” -120 students in the afternoon. Very successful.

Talk: Che cos'è l'energia

S77. MOLA, 5/4/86, Associazione turistica Pro Loco-about 30 people very attentive

Org. by: Comitato Antinucleare di Mola

Talk: Pace e nucleare: energia nucleare tra civile e militare

A bit cold environment. A few questions. Probably anti-nuclear majority. I am not unhappy.

S78. BARI, 9/4/86, Facoltà di Giurisprudenza, University of Bari.

Round table: La società post-industriale in Europa fra dialogo e conflitto

S79. PADOVA, 12/4/86, Department of Biology-about 300 people

Org. by AIMPGN.

Talk: Evoluzione della tecnologia delle armi nucleari e delle relative strategie militari

S80. PADOVA, 13/4/86-About 80 people

Round table: Rischio di guerra nucleare in Italia: quale prevenzione?

S81. BARI, 7/5/86, Istituto Tecnico Statale per Geometri. Centro Studi Polivalente- a lot of people, it went very well.

Talk: Einstein, la sua influenza sulla fisica moderna e il pensiero scientifico contemporaneo

S82. MATERA, 15/5/86, Liceo classico “E. Duni”.

Talk: *L'infinito in fisica*

It went very well. A lot of questions.

S83. GIOIA DEL COLLE, luglio 86,

speech/debate to 20 executives of the PCdI on nuclear energy after Chernobyl. It also went well for quality interventions. Tough people are the ones of Gioia.

S84. GENZANO, 19/5/87, Liceo scientifico

Long talk on Energy. First part on the concept, second part on ecological concerns. One hundred kind people. It went well.

S85. MOLFETTA, 11/12/87, Università Popolare Molfettese

Talk: *Il ruolo di Karl Popper nella fisica contemporanea*

S86. TRICARICO, 26/5/88. Liceo scientifico

Talk: *Global ecological challenges on the threshold of 2000*

S87. RIMINI, 1988 Meeting dell'amicizia fra i popoli

Round table on Halton Arp

S88. BARI, 27/10/88, Istituto Tecnico per Geom. Pitagora

Org. by: IRRSAE + UCIIM.

Talk: *Nuovo statuto epistemologico della fisica*

25 people, training course for headmasters and upper secondary school teachers. Interest, debate.

S89. TRIESTE, 22/11/88, Centro culturale “G. La Pira”-Aula Venezian a Giurisprudenza.

Talk: *Scienza ed oggettività: scelte ritardate fra amici ed elettroni*

S90. VERONA, 23/11/88, Centro Culturale S. Adalberto e Associazione Universitaria Rosmini

Talk: *I paradossi della fisica moderna*

S91. PADOVA, 31/1/89, Centro Culturale G. Sacchetti, Palazzo del Bò

Talk: *I paradossi della fisica*

S92. BARI, 11/11/89, Associazione Barese Astrofili “Guido Horn-D’Arturo”

Talk: *Il big bang esiste veramente?*

S93. CATANIA, 16/11/89, Università di Catania-Dipartimento di Fisica

Talk: *Il paradosso di EPR ancora irrisolto*

S94. BARI, 24/11/89, Facoltà di Giurisprudenza-Aula Magna “A. Moro”

Round table: *L’immagine del filosofo nella città*

S95. LECCE, 5/12/89, Liceo Scientifico “G. Banzi Bazoli”

Org. by: Centro Studi Lucio Lombardo Radice

Talk: *Fisica senza dogma*

S96. LECCE, 5/12/89, Centro Culturale della Regione Puglia

Org. by: Centro Studi Lucio Lombardo Radice

Round table: *Mezzogiorno d’Italia: quale futuro per le tecnologie militari?*

S97. TARANTO, 20/10/89, Associazione di Cultura Scientifica “Lazzaro Spallanzani”

Conference: NUOVI PROGRAMMI PER LA SCUOLA MEDIA (D.M. fab. 79)

Talk: *Alcune proposte circa l’insegnamento delle materie scientifiche nella scuola media*

1990-1994

S98. POTENZA 7-8/5/92, Università di Potenza-Dipartimento di Matematica

Talks:

La relatività e l'etere

Onde e corpuscoli di luce e materia

S99. NAPOLI, 5/12/92, Palazzo Serra di Cassano

Org. by: Nuove Ricerche Metodologiche

Talk: I fondamenti della fisica e la filosofia

S100. BOLOGNA, 28/01/93-University of Bologna

Talk: Fisica quantistica di processi causali nello spazio e nel tempo

S101. BARI, 3/3/93, Liceo "A. Scacchi"-about 200 people, much debate

Talk: La cosmologia e l'astrofisica extragalattica

S102. OSTUNI, 12/3/93, Liceo classico

Talk: Il triangolo scienza, potere e magia da Copernico a Newton

S103. DESIO, 29/4/93, Liceo scientifico "Ettore Majorana"

Talk: La scienza del XX secolo

S104. VARESE, 30/4/93, Liceo scientifico "Galileo Ferraris"

Talk: Il significato della relatività speciale

S105. LECCE, 5-8/10/93

THE FOUNDATIONS OF QUANTUM MECHANICS. HISTORICAL
ANALYSIS AND OPEN QUESTION

Talk on measurement problem (with Afriat)

S106. OSTUNI, 17/2/94 (uncertain year), Liceo Classico

Talk: Teorie scientifiche intorno all'origine dell'universo

S107. MISANO ADRIATICO (RN), 18/3/94, Aula Magna Istituto San Pellegrino

Series of workshops L'ERESIA: I VOLI DELLA MENTE NELLA STORIA DEL PENSIERO

Org. by: Biblioteca Comunale di Misano Adriatico

Talk: I quanti, la relatività e l'esistenza degli oggetti microscopici

S108. CESENA, 26/9/94, Centro di epistemologia

Talk: Considerazioni sulla teoria della relatività

S109. CESENA, 30/10/94, Palazzo del Ridotto.

Conference L'EPISTEMOLOGIA DI KARL POPPER

Talk: Le propensioni di Popper e il probabilismo nella fisica moderna

1995-1999

S110. LECCE, 22/2/95,

Workshop LOGICA E EPISTEMOLOGIA

Talk: Einstein, la relatività e l'etere

S111. ACQUAVIVA DELLE FONTI, 16/6/95, Salone del Municipio-about 300 people

Org. by: Associazione Acquavivese Astrofili

Talk: Dubbi sul big bang. E' mai avvenuto?

S112. ACQUAVIVA DELLE FONTI, 10/11/95, Salone del Municipio-about 300 people

Org. by: Associazione Acquavivese Astrofili

Talk: Albert Einstein fra gli atomi e il cosmo

S113. BARI, 9/2/96, Aula magna dell'Università

Org. by: Accademia Pugliese delle Scienze, CIDI

Round table on time problems

Deadening with closed atmosphere. Very vapid.

S114. LECCE, 24/4/96, Dipartimento Filologia classica e medioevale

Talk: Nozione di tempo nella fisica moderna

S115. CERIGNOLA, 15/5/96, Liceo scientifico "A. Einstein"

Talk: Il principio di relatività

S116. URBINO, 27/6/96

Talk: La fisica e la natura del tempo

About 50 listeners forced to follow because it was a master's course.

S117. CESENA, 22/9/96

Convegno in favour of Prigogine

Talk: La natura del tempo e la velocità della luce sulla terra

S118. ACQUAVIVA DELLE FONTI, 25/10/96, Salune del Municipio.

Org. by: Associazione Acquavivese Astrofili.

Round table: Origine dell'universo Fra scienza e fede

Full hall, 200 people. This time it went badly.

S119. ROMA, 18/11/96, Liceo classico "T. Tasso"

Org. by: CIDI, Circolo Bateson

Talk: Bohr, la complementarità e la meccanica quantistica

Cini got crazy against me. He is old, he can no longer hold the debate. Then, in reality, he has always been a poor physicist.

S120. BARI, 19/3/97, Department of Physics

Talk: Relatività, etere e problema della sincronizzazione

S121. UGGIANO MONTEFUSCO, 3/4/97 and 17/4/97, Osservatorio
Astronomico

CORSO DI CULTURA SCIENTIFICA PER DOCENTI

Org. by: Associazione Culturale "I. Newton"

Talk: Fisica e filosofia

S122. BARI, 13 and 20/5/97, Department of Physics

CORSO DI PERFEZIONAMENTO IN DIDATTICA DELLA FISICA

Talk: Introduzione all'Epistemologia della Fisica

S123. ROMA, 23/5/97, Liceo Classico "B. Russell"

Talk: Fisica e oggettività

S124. BARI, 31/5/97, Associazione della Stampa

Org. by: Associazione Ontosofia Psicosomatica

Round table: Verità scientifica, Conoscenza e Ontosofia

S125. NAPOLI, 29/9/97, Facoltà di Ingegneria

Org. by: Dipartimento di Ingegneria Elettrica

Talk: Alcuni problemi della relatività speciale e loro superamento

S126. BOLOGNA, 20/10/97, Accademia delle Scienze

Org by: SILFS

Talk: Il realismo come guida nei problemi della fisica moderna

S127. UGGIANO MONTEFUSCO, 28/11/97, Osservatorio Astronomico

CORSO DI CULTURA SCIENTIFICA PER DOCENTI

Org. by: Dipartimento di Fisica Università Di Lecce, Associazione Culturale
“I. Newton”.

Talk: La teoria della relatività e la natura del tempo

S128. BARI, 18 and 25/2/98, Department of Physics

CORSO DI PERFEZIONAMENTO IN DIDATTICA DELLE FISICA

Talk: Introduzione all'Epistemologia della Fisica

S129. CESENA, 13/3/98

Talk: La fisica del Novecento e le sconfitte della conoscenza

S130. MANDURIA (TA), 25/3/98, Liceo scientifico “G. Galilei”

Talk: La fisica moderna fra fisica e filosofia

S131. ACQUAVIVA DELLE FONTI, 31/3/98, Salone del Municipio

INCONTRI 98

Org. by: Associazione Acquavivese Astrofili

Talk: Come nacque la scienza: i grandi pensatori greci

Usual full Hall. 200 people.

S132. BARI, 7/4/98, Department of Physics

Talk: C'è un serio problema logico nella teoria della relatività

S133. CESENA, 13/4/98, Centro di Epistemologia

IL PROBLEMA DELLA CONOSCENZA

Org. da: Università di Urbino, Comune di Cesena

Talk: La fisica del 900 e le sconfitte della conoscenza

S134. MARGHERITA DI SAVOIA, 16/4/98

Talk: La relatività: aspetti scientifici e filosofici

S135. CONVERSANO, 20-22/4/98, Liceo Scientifico

Workshop: LE NUOVE FRONTIERE DELLA FISICA E LE TEORIE CONTEMPORANEE

Org. by: Istituto Italiano di Studi Filosofici, I.R.R.S.A.E., Liceo Scientifico Conversano, Liceo Scientifico Putignano, Liceo Classico Putignano, Istituto Professionale Industriale Artigianale Putignano, Liceo Scientifico Noci

Talks::

1. Epistemologia e fisica
2. Big bang e cosmologia
3. Fondamenti della fisica moderna

S136. CASSANO, 29/4/98, Sala Consiliare

Conference: L'ALIENAZIONE NELLE SCIENZE NATURALI E UMANE

Org. by: Associazione Ontosofia Psicosomatica

Talk: L'alienazione nelle scienze naturali

Unsuffled atmosphere, only they were present.

S137. CESENA, 14-18/9/98, Centro di Epistemologia

SCUOLA ESTIVA DI FILOSOFIA DELLA SCIENZA

Org. by: Univerità di Bologna, Università di Urbino, Comune di Cesena

Course: FONDAMENTI DELLA MECCANICA QUANTISTICA

Talks:

1. I disaccordi fra i padri fondatori
2. I paradossi della completezza
3. Superamento delle relazioni di indeterminazione
4. Onde, corpuscoli, dualità e dualismo
5. Il paradosso di Einstein, Podolsky e Rosen

S138. CESENA, 19/9/98, Palazzo del Ridotto

Org. by: SILFS

Talk: De Broglie e la relatività

S139. MELFI, 19-23/10/98, Liceo Scientifico “Federico II”

SCUOLA ESTIVA FOR TEACHERS AND STUDENTS OF MELFI

Org. by: Istituto Italiano di Studi Filosofici

Course: FONDAMENTI DELLA FISICA MODERNA

Talks:

1. Galileo, Keplero e Newton e l'unità di cielo e terra

2. Fisica moderna e filosofia

3. Relatività e relativismo

4. Al di là delle relazioni di indeterminazione

5. Il realismo locale e il paradosso di EPR

S140. LECCE, 1998

Conference: THE FOUNDATIONS OF QUANTUM MECHANICS.
HISTORICAL ANALYSIS AND OPEN QUESTIONS.

Talk: The physical meaning of Bell Type Inequalities

S141. CESENA, 15/2/99, Palazzo del Ridotto

Org by: Selleri for SILFS

Talk on relativity

S142. BARI, 24/2/99 and 3/3/99, Department of Physics

CORSO DI PERFEZIONAMENTO IN DIDATTICA DELLA FISICA

Course: INTRODUZIONE ALL' EPISTEMOLOGIA DELLA FISICA

Talks:

1. Il positivismo e la relatività
2. Empirismo logico e esistenzialismo
3. Popper e la rivolta antiempirista
4. Kuhn, Lakatos e Feyerabend
5. La terza dimensione di G. Holton

S143. PUTIGNANO 15-16/3/99, Liceo Scientifico

Two talks:

La fisica del XX secolo: per un bilancio critico

Very well. 40 people nailed to chairs

S144. MOLFETTA, 22/3/99, Teatro Odeon-about 400 high school students

NOVECENTO ADDIO

Org. by: Città di Molfetta

Talk: La fisica del Novecento: per un bilancio critico

S145. UGGIANO MONTEFUSCO (TA), 16/4/99, Osservatorio Astronomico

CORSO DI CULTURA SCIENTIFICA PER DOCENTI

Org. by: Associazione Culturale "I. Newton"

Talk: I grandi pensatori greci

S146. CESENA, 26-30/4/99, Centro di Epistemologia

SCUOLA SUPERIORE DI STUDI IN FONDAMENTI E FILOSOFIA
DELLA FISICA

Org. by: Univerità di Bologna, Università di Urbino, Comune di Cesena

Course: FONDAMENTI DELLA MECCANICA QUANTISTICA

Talks:

1. *I disaccordi fra i padri fondatori*
2. *I paradossi della completezza*
3. *Superamento delle relazioni di indeterminazione*
4. *Onde, corpuscoli, dualità e dualismo*
5. *Il paradosso di Einstein, Podolsky e Rosen*

S147. CESENA, 13-17/9/99, Centro di Epistemologia

II SCUOLA ESTIVA DI FILOSOFIA DELLA FISICA

Org. by: Univerità di Bologna, Università di Urbino, Comune di Cesena

Course: NUOVE TENDENZE IN FISICA RELATIVISTICA

Talks:

1. *La relatività secondo Einstein e secondo Lorentz*
2. *Relatività e relativismo*
3. *Le teorie equivalenti alla relatività speciale*
4. *Piattaforme ruotanti e trasformazioni inerziali*
5. *La situazione sperimentale*

S148. NAPOLI, 4-8/10/99, Auditorium dell'Osservatorio Astronomico di Capodimonte

SCUOLA ESTIVA DI ALTA FORMAZIONE

Org. da: Istituto Italiano di Studi Filosofici, Osservatorio Astronomico di Capodimonte

Course: PER UN BILANCIO CRITICO DELLA FISICA DEL 900

Talks:

1. Galileo, Keplero e Newton e l'unità di cielo e terra
2. Relatività e relativismo
3. Le teorie equivalenti alla relatività speciale
4. Onde, corpuscoli, dualità e dualismo
5. Il paradosso di Einstein, Podolsky e Rosen/Le teorie equivalenti alla relatività speciale

S149. BARI, 18-25/10/99, Department of Physics

CORSO DI PREPARAZIONE AL CONCORSO A CATTEDRA PER L'INSEGNAMENTO DELLA MATEMATICA E DELLA FISICA

Org. by: Università di Bari, Dipartimento Interateneo di Fisica

Course: Sistemi di riferimento e relatività

Talks:

1. Relatività, etere e propagazione della luce
2. Le trasformazioni di Lorentz
3. Energia e quantità di moto relativistiche
4. Perchè quella della luce è la velocità limite
5. Elettrodinamica relativistica

S150. UGGIANO MONTEFUSCO (TA), 4/11/99, Osservatorio Astronomico

CORSO DI CULTURA SCIENTIFICA PER DOCENTI

Org. by: Università di Lecce-Dip. Di Fisica, Associazione Culturale "I. Newton"

Talk: Per un bilancio critico della fisica del 900

2000-2007

S151. CESENA, 14/3/2000, Centro di Epistemologia

Talk: Relatività e relativismo

S152. BARI, 17/3/2000, Liceo scientifico “G. Salvemini”

Talk: Galileo, Keplero, Newton e la fisica moderna

Classroom with 150 people. Very little debate. Fault of the headmaster and mine.

S153. BARI, 29/3/2000, Liceo scientifico “G. Salvemini”

Talk: Relatività e relativismo

Half-full hall, 80 people. Much better than the 17/3. I sparked an initial debate and then there was also a final one.

S154. CESENA, 10-13/4/2000, Centro di Epistemologia

SCUOLA SUPERIORE DI STUDI IN FONDAMENTI E FILOSOFIA
DELLA FISICA

Talk: Dualità onda-corpuscolo

S155. URBINO, 13/4/2000, Dipartimento di Filosofia

Talk: Nuovi sviluppi nei fondamenti delle teorie relativistiche

Seminar without transparencies and with few formulas. Long debate, real interest.

S156. Cassano (BA), 7-8/10/2000, Sala del Comune

WORKSHOP: COSMO, SOGNO, EROS E PSICHE

Org. by: Associazione di Ontosofia Psicosomatica

Talk: L'Universo in lenta evoluzione, ossia, la cosmologia senza il big bang

S157. CASERTA, 26/10/2000, University of Caserta-Dep of Mathematics

Talk: Teorie “equivalenti” alla relatività

S158. MATERA, 3/12/2000, Centro Carlo Levi

SCIENZA E UMANESIMO NEL MONDO DELLA COMPLESSITÀ

Talk: Semplicità apparente e complessità reale della fisica odierna

S159. TARANTO, 4/12/2000, Terrarte, c/o biblioteca comunale di Taranto

Talk: Il principio di relatività e la natura del tempo

S160. TARANTO, 6/12/2000, Terrarte, c/o biblioteca comunale di Taranto

Talk: I sistemi atomici e la meccanica quantistica

S161. MATERA, 16/12/2000, Nuova Accademia Olimpia

Talk: Cento anni di fisica quantistica

S162. GIUGLIANO (NA), 26-28/3/2001, Istituto Italiano per gli. Studi Europei, Palazzo Palumbo

L'IMMAGINE DELLA SCIENZA. FILOSOFIA, SCIENZA E TECNICA:
UNA RINNOVATA ALLEANZA

Course: POSSIBILI MUTAMENTI DELLE BASI DELLA FISICA
MODERNA.

Talks:

La meccanica quantistica e il problema della realtà oggettiva

La relatività e la natura di spazio e tempo

La cosmologia dell'evoluzione lenta

S163 BARI, 13/3/2001 Parrocchia S. Giuseppe

Talk: Uranio impoverito e paesi impoveriti

S164. CASSANO, 20-21/04/2001 Comune di Cassano

Talks:

Fisica e filosofia

La fisica del Novecento: per un bilancio critico

La teoria della relatività e la natura del tempo

La velocità della luce

Le forme dell'energia

Come nacque la scienza: i grandi pensatori greci

Uranio impoverito e paesi impoveriti

La cosmologia con o senza Big Bang

S165. MOLA DI BARI, 6/3/2003, Liceo Scientifico "E. Majorana"

Training course: LA TEORIA DEI QUANTI: RIFLESSIONI SU ASPETTI SCIENTIFICI, STORICO - FILOSOFICI, DIDATTICI

Talk: Il problema del realismo nella teoria dei quanti. Analisi critica degli aspetti scientifici e filosofici più problematici della meccanica quantistica

S166. BARI, 13/05/2003, University of Bari-Faoltà di Scienze della Formazione

Talk: Le epistemologie contemporanee e la pedagogia: dal discorso aperto all'euristica del progetto

S167. NOLA (NA), 17/05/2003, Parco letterario "Giordano Bruno"

Org. by: Istituto Italiano per gli Studi Filosofici, Sezione del C.I.D.I. di Nola, Associazione culturale Meridies di Nola, Libreria Nuovo Millennio di Nola

Talk. La fisica e la natura del tempo

It went great. Packed hall, 120 people. Lots of focus, lots of questions.

S168. RUVO DI PUGLIA (BA), 02/03/2004, Liceo Scientifico

Talk: La fisica relativistica e la natura del tempo

S169. BARLETTA, 02/04/2004, Liceo scientifico

Talk: Le basi filosofiche della teoria dei quanti

S170. UGGIANO MONTEFUSCO (TA), 26 November 2004,
Osservatorio didattico "I. Newton"

Talk: *La relatività 100 anni dopo*

S171. UDINE, 26-28/09/2004

FRONTIERS OF FUNDAMENTAL PHYSICS

Talk: Space and time physics with the Lorentz ether

S172. ROMA, 4/5/2004, Istituto Nazionale di Geofisica e Vulcanologia

Talk: Spiegazione dell'effetto Sagnac: sue conseguenze per la fisica satellitare

S173. L'AQUILA, 6-11/9/2004-Associazione per l'insegnaento della Fisica
FISICA E FILOSOFIA NELLA SCUOLA. L'INFLUSSO DELL'OPERA DI
ALBERT EINSTEIN NEL PENSIERO SCIENTIFICO E FILOSOFICO
DEL '900 E L'INSEGNAMENTO DELLA FISICA E DELLA FILOSOFIA
NELLA SCUOLA SECONDARIA SUPERIORE

Talk. La relatività e la sincronizzazione degli orologi

S174. MONOPOLI, 10-16/07/2004

24th INTERNATIONAL SYMPOSIUM ON RAREFIED GAS DYNAMICS

Talk: Heisenberg inequalities and the realistic interpretation of quantum theory

S175. Altamura, 22/05/2007

Talk: Albert Einstein filosofo, scienziato e uomo di pace

S176. Bari 09/11/2007-01/02/2008-Department of Physics

SEMINARS ON FOUNDATIONS OF PHYSICS

Talks:

1. Galileo, Keplero e Newton e la natura della fisica (9 Novembre 2007)

2. Che cos'è l'energia (23 Novembre 2007)

3. Il paradosso degli orologi e la teoria Relatività Speciale (7 Dicembre 2007)

4. Le relazioni di indeterminazione di Heisenberg e il rapporto fra fisica e filosofia (21 Dicembre 2007)

5. La natura delle onde quantistiche (18 Gennaio 2008)

6. Il paradosso di Einstein, Podolsky e Rosen e gli stati "entangled" (1 Febbraio 2008)

Assignments sound interesting because Selleri put his meetings with several scholars and some comments on, showing unknown paths of his research as well as allowing cross-referencing to confirm conferences, seminars, research lines and so on. Like, for instance, a mission to Milano in January 1985 for an experiment he proposed. Which one? The following chart was found in AGAB-FS

MANDATE	PERIOD	REASON
PARIGI	25/3/81-3/4/81	
GINEVRA-ROMA	21-29/6/81	Meeting-research
VIENNA-BERNA	4-8/10/81	research-meet with Rietschmann-Rauch
LONDRA-PARIGI	16-23/10/81	Conference-research
BOLOGNA	14-15/1/82	
ROMA	20-21/1/82	
CATANIA, PALERMO	3-8/2/82	Rapisarda-Cavalieri
MONACO, HELSINKI, KLAUSTKAL GOTEBORG, AMSTERDAM	16/11-02/12/81	Work

HELSINKI, GOTEORG	17-24/11/1981	Conferences
VIENNA	20/10-11/11/80	
CATANIA	24-25/02/82	
BOLOGNA,PISA ,ROMA	6-11/09/82	Debate with Tarozzi-Gozzini-Beneventano
LONDRA	5-15/10/82	Bohm-Imperial College
PARIGI	15-24/7/82	Fondazione Louis de Broglie
GINEVRA	8-16/6/82	Bell, Pinon, research
BOLOGNA	1-3/6/82	Tarozzi
PARIGI	15/10-15/12/82	Lectures at Ecole Normale Superieure
PARIGI	10-15/10/82	Diner
IRAKLION	21/5-6/6/83	Sordalis-Kyprianidis
COMO	3-6/11/83	CNR Congress
BOLOGNA	27-29/10/83	Congress: Le frontiere della fisica
ROMA	14-16/7/83	De martini
BOLOGNA,PARMA,PISA	12-16/9/83	Tarozzi, Fieschi, Gozzini
PISA	17-18/8/83	Gozzini
BOLOGNA	30/09-08/10/83	Tarozzi
PALERMO	20-22/09/83	Cavaliere
VIENNA	27/11-09/12/83	Ranch and Sexl
GINEVRA	6-13/9/84	Conference EP Wigner
ROMA, PISA, PARMA	27/8-03/09/84	Cini, Gozzini, Giovannelli
MILANO	23-25/7/84	Giovannelli, Panizza: experiment plan
CAPRI	20-21/5/84	Conference on solid state
SANTANDER	23-30/4/84	Collaborations Bari-Manchester-Santander
VIENNA	2-7/04/84	Sexl-2° book edition “Die Debatte”
URBINO	13-15/11/84	Organizing conference
REGGIO EMILIA	25-28/10/84	Workshop on Nobili Organizing congress
ROMA	29/10-02/11/84	Congress HoS
URBINO,MILANO	8-14/10/84	Organizing international conference

PALERMO	18-22/09/84	Cavaliere-Ferrante
PARIGI	25/3-3/4/81	Research
URBINO	28-29/11/84	Organizing international conference
PARIGI,GINEVRA	9-14/02/81	
ROMA	23/4/85	Cabibbo on Urbino conference
CESENA	10-14/4/85	Conference
BOLOGNA	1-3/02/85	Organizing Urbino conference
MANCHESTER	18-27/2/85	Collaborazione Bari-Manchester-Santander
VIENNA	23-28/1/85	Rauch, conference, book translation
MILANO	11-12/1/85	Scientific debate at CISE on my experiment
URBINO,GLASGOW	10-16/9/85	Organizing conference- debate on Stirling
MADRID	10-20/7/85	Marshall-Santos
HELSINKI, AMSTERDAM	15-21/6/85	International conference Contacts with Riedel publisher

ADDENDUM 3

CORRESPONDENCE AND

CONTRIBUTIONS

I decided to submit here the reproduction of some documents whose excerpts have been used within the text, in order not to further burden the thesis with other notes. They include letters from Selleri's correspondence as well as speeches and statements by other characters, and the Italian original text of several statements which I translated.

Chapters, sections, themes, and pages containing every specific excerpt are given for each document here submitted.

INTRODUCTORY NOTES

SECTION I.5-P. 33: THE ORIGINAL ITALIAN TEXT BY THE MATHEMATICS, PHYSICS AND NATURAL SCIENCES FACULTY BOARD OF 17 OCTOBER 1980

Il prof. Franco Selleri ha svolto la sua attività scientifica occupandosi di:

- a) fenomenologia delle interazioni forti delle particelle elementari; di grande importanza sono i contributi rilevanti nella formulazione e applicazione del modello periferico;
- b) fondamenti della meccanica quantistica, con particolare rilievo dei risultati concernenti l'estensione della disuguaglianza di Bell.

L'attività scientifica è testimoniata dall'elevatissimo numero di pubblicazioni prodotte su riviste internazionali.

Notevolissima l'attività didattica svolta dal prof. F. Selleri in vari insegnamenti del corso di laurea in Fisica della Facoltà di Scienze dell'Università di Bari; di notevole interesse l'intensa attività seminariale tenuta anche all'estero e i corsi tenuti alla Scuola di Perfezionamento in Fisica.

[...] Il Consiglio di Facoltà [...]unanime delibera di chiamare il prof. Franco Selleri a riprire dal 1/1/80 il posto di professore di ruolo di IFT messo a concorso nel gruppo n. 236 (prima disciplina IFT) [...] con le seguenti motivazioni:

'Per la più cospicua e varia produzione scientifica e per l'attività didattica più estesa e più pertinente alla disciplina messa a concorso, il Consiglio di Facoltà è ben lieto di chiamare il prof. Franco Selleri a ricoprire la cattedra di IFT messa a concorso' (AGAB-FS).

SECTION I.5-PP. 34-35: THE ORIGINAL ITALIAN TEXT BY THE BACHELOR OF PHYSICS DEGREE PROGRAM AND THE DEPARTMENT OF PHYSICS BOARD HELD ON 18 JANUARY 1984

Il prof. Franco Selleri, vincitore del concorso n. 236 (prima disciplina: IFT) è stato chiamato a ricoprire la cattedra di IFT presso la Facoltà di Scienze dell'Università di Bari, a decorrere dal 1/1/1981; quale compito didattico istituzionale gli è stato affidato l'insegnamento di "Istituzioni di Fisica Teorica B" per gli indirizzi applicativo e didattico del Corso di Laurea in Fisica. Negli AA.AA. 1980/81, 81/82 e 82/83 gli è stato inoltre affidato a titolo gratuito l'insegnamento di "Teorie Quantistiche" per il corso di laurea in Fisica.

Il prof. Selleri è stato relatore di numerose tesi di laurea in Fisica nel campo dei Fondamenti della Meccanica Quantistica e nel campo della Didattica della Fisica.

Allo svolgimento della attività didattica il prof. Selleri si è dedicato sempre con grande impegno e dedizione, redigendo le dispense dell'intero corso istituzionale e valendosi della sua lunga esperienza di ricercatore in vari campi della fisica teorica (dalle Particelle Elementari ai Fondamenti della Meccanica Quantistica) per tenere corsi sempre aggiornati e di ottimo livello.

Il prof. Selleri è stato per due volte membro delle commissioni giudicatrici per concorsi liberi a posti di ricercatore universitario per il gruppo n. 86 (Fisica Teorica), rispettivamente all'Università di Bari ed all'Università di Parma.

Egli ha inoltre svolto una vasta ed intensa attività seminariale, sia come relatore sia come organizzatore. In qualità di relatore il prof. Selleri ha tenuto oltre venti seminari scientifici sui Fondamenti della Meccanica Quantistica presso Università e Centri Scientifici in Italia ed all'estero (Austria, Belgio, Francia, Germania, Gran Bretagna, Olanda) ed oltre venti seminari a carattere divulgativo su problemi scientifici e filosofici presso licei e circoli culturali in Italia.

Egli ha inoltre organizzato numerosi seminari scientifici presso l'Istituto (ora Dipartimento) di Fisica dell'Università di Bari con interventi di relatori italiani e stranieri, nonché due Convegni Internazionali: uno a Perugia sul dualismo onda-particella (22-30/4/82) ed uno a Bari, presso il Dipartimento di Fisica su: "Open questions in Quantum Physics" (4-7/5/83).

Nell'autunno del 1983 il prof. Selleri ha elaborato un progetto volto a stabilire contatti con circa 100 ditte italiane (o rappresentanti italiane di ditte estere) allo scopo di favorire (mediante borse di studio, tesi di laurea o altro) l'inserimento di giovani laureati in Fisica dell'Università di Bari nel mondo del lavoro. L'iniziativa è stata approvata dal Consiglio di Corso di Laurea in Fisica e dalla Giunta del Dipartimento di Fisica e di essa il prof. Selleri si è fatto pienamente carico.

Il C.C.d.L. in Fisica e il C.d.D. di Fisica esprimono vivo apprezzamento ed un giudizio altamente positivo per l'intensa attività didattica, organizzativa e culturale del prof. Selleri e per il modo in cui egli ha adempiuto in genere ai suoi doveri accademici nel periodo di straordinario (AGAB).

SECTION I.5-P. 35: THE ORIGINAL ITALIAN TEXT BY THE FACULTY BOARD COMMISSION

Il Prof. Franco Selleri ha preso servizio come straordinario di IFT presso la Facoltà di Scienze MFN dell'Università di Bari in data 1/1/81. La Facoltà ha espresso parere favorevole sull'attività didattica e organizzativa del Prof. Selleri.

La sua attività di ricerca, concretatasi in 11 pubblicazioni, si è sviluppata sui fondamenti della Meccanica Quantistica, in particolare sul paradosso di Einstein-Rosen e Podolski. È parere di questa Commissione che l'approccio di Selleri a tale problematica trascuri talvolta l'aspetto fisico a favore di quello ideologico. Tuttavia, la Commissione ne propone la nomina a Professore Ordinario (AGAB-FS).

CHAPTER 1

SECTION 1.2-P. 53: THE ORIGINAL ITALIAN TEXT BY SELLERI'S DEGREE THESIS

Scopo di questo lavoro è dare un quadro, sia pure parziale, della situazione attuale delle ricerche sull'interazione pione-nucleone. Nel primo capitolo verrà ripreso un argomento già ben noto: quello dello spin isotopico. Esso è stato considerato da un punto di vista più formale e meno dipendente da pesanti analogie con i momenti angolari. La traccia logica della trattazione è quella di Wick, ma gran parte dei calcoli e la discussione fisica di vari punti sono indipendenti. Nel secondo capitolo verrà trattata l'interazione pione-nucleone a bassa energia e saranno mostrate diverse ragioni sperimentali e teoriche che fanno credere all'esistenza di una risonanza nello stato $T=J=3/2$. Nel capitolo seguente sarà fatta l'analisi dell'interazione pione-nucleone ad alta energia con particolare riguardo al modello isobarico. Per mezzo di quest'ultimo verrà fatto un paragone con l'esperienza a 810 e 950 MeV sfruttando i dati sperimentali di Bagget e di V. Borelli, A. Minguzzi-Ranzi, P. Waloscheck e V. Zoboli (Voglio qui ringraziare i sunnominati Autori della esperienza a 950 MeV che mi hanno fornito i risultati parziali del loro lavoro, prima della pubblicazione). Saranno pure fatte alcune previsioni, suscettibili di verifica sperimentale, che dovranno in futuro confermare o meno le ipotesi del modello isobarico. Infine nel quarto capitolo verrà considerata l'interazione pione-pione, sia in base al modello di Piccioni, Dyson e Takeda, sia in base alla recente ricerca di Chew e Low (Selleri 1958, pagg 1-2).

SECTION 1.3-PP.58-59: THE ORIGINAL ITALIAN TEXT BY FURIO COLOMBO AT ENI TO SELLERI

Egregio Dottore, abbiamo rilevato il Suo nominativo dagli elenchi della Università in cui Ella ha conseguito la laurea. Ci rivolgiamo a Lei per prospectarle un programma di inserimento di neo-laureati nella nostra azienda, che potrebbe interessarLa e riguardarLa. E facendo riferimento al tipo di laurea da Lei ottenuto, siamo in grado di esporLe i dati obbiettivi del programma. Le mansioni concernono: problemi di matematica applicata agli studi di laboratorio elettronico. Studi e ricerche in campo elettronico (solo per laureati in Fisica). Servizi tecnico-commerciali di programmazione sui progetti di meccanizzazione della contabilità per mezzo d'impianti a schede perforate o di calcolatrici elettroniche. Lo scopo prevalente della nostra ricerca è quello di immettere giovani a livello universitario nei suddetti settori specializzati. [...] Le proponiamo pertanto un incontro che potrebbe avere luogo presso i nostri Uffici di Milano, per uno scambio di informazioni, senza reciproco impegno. Avremo così occasione di conoscere i Suoi orientamenti e le Sue aspirazioni professionali, e potremo, a nostra volta, illustrarLe dettagliatamente le possibilità di carriera riservate, nella nostra organizzazione, al personale laureato. [...] questo invito è confermato anche nel caso che Lei debba ancora compiere il Servizio Militare perché ci sembra che, in ogni caso, potrebbe essere utile o reciprocamente interessante una preliminare presa di contatto. [...] La preghiamo infine di voler considerare a nostro carico eventuali spese di viaggio [...]. Ci è gradito inviarLe i nostri migliori saluti" (Furio Colombo-Ing. Olivetti & C. S.p.A.- Segreteria per la selezione del personale laureato to Selleri, March 10th, 1959-FSPA).

SECTION 1.3-P.59: THE ORIGINAL ITALIAN TEXT BY TRECCANI

Una delle imprese private italiane più innovative fu appunto la Olivetti, che mostrò una spiccata attenzione alle scienze sociali e ai modelli di organizzazione industriale sviluppati negli Stati Uniti. Fondata come piccola officina nei primi anni del Novecento, si specializzò nella realizzazione prima di macchine per scrivere e poi, dagli anni Quaranta, di calcolatrici. Nel 1948 produsse la Divisumma 14, la prima calcolatrice scrivente al mondo in grado di eseguire le quattro operazioni. Nonostante la dura concorrenza dei colossi dell'elettronica giapponesi e statunitensi, nel 1965 presentò a New York il primo calcolatore elettronico commerciale del mondo, la P 101, che raccolse un successo strepitoso (Treccani 2013).

SECTION 1.3-P.60: THE ORIGINAL ITALIAN TEXT BY TRECCANI

Assai originale fu anche la vicenda dell'ENI, creata nel 1953 da Mattei, che nell'estate 1945 aveva ricevuto il compito di liquidare [una] creazione del fascismo, l'AGIP (*Azienda Generale Italiana Petroli*). Mattei si oppose però a questa liquidazione, che [invece] era vista con particolare favore dalle cosiddette sette sorelle, le maggiori compagnie petrolifere statunitensi e britanniche, interessate a impiantare le loro reti di distribuzione in Italia. [...] Mattei rilanciò l'AGIP, non solo aprendo nuovi distributori di benzina, ma anche inaugurando una catena di motel e le prime stazioni di servizio. In seguito, promosse una campagna di trivellazioni nella pianura padana, che portò al ritrovamento di giacimenti di metano e nel giro di pochi anni fece dell'ENI una holding a capo di un variegato impero industriale.

Abile imprenditore, ma anche appassionato mecenate, Mattei seppe circondarsi di intellettuali e giornalisti [...].

Abituato a muoversi con spregiudicatezza e con pochi scrupoli, egli strinse contratti con l'Iran, con l'Egitto e persino con l'Unione Sovietica, ma la sera del 27 ottobre 1962 il piccolo aereo su cui stava viaggiando si disintegrò nel cielo di Pavia. Nel 1997 la Procura di Pavia ha riaperto le indagini su questo incidente, e nel 2005 l'analisi di un suo perito, il professor Donato Firrao del Politecnico di Torino, ha dimostrato che sull'aereo si verificò una deflagrazione provocata da un ordigno.

[...] Con l'uscita di scena di Mattei si chiuse definitivamente l'epoca di quegli imprenditori indipendenti che furono gli artefici della ricostruzione e i pionieri dello sviluppo economico (Treccani 2013).

SECTION 1.4-PP. 61-62: THE ORIGINAL ITALIAN TEXT BY SELLERI UNPUBLISHED DOCUMENTS 1961

Ho ottenuto la laurea in fisica a Bologna nel dicembre 1958, presentando una tesi sull'applicazione del modello isobarico nelle interazioni anelastiche pione-nucleone. Relatore e suggeritore di questa tesi era G. Puppi che mi insegnò una cosa molto importante: che la fisica uno può capirla anche senza bisogno di troppa matematica; che "fare della fisica" significa prendere dei dati sperimentali e cercare di capire, dalle loro caratteristiche, cosa è successo al momento dell'interazione; e che soltanto quando si crede di aver capito ciò, vale la pena di mettersi a fare dei calcoli. Sui meriti del relatore della mia tesi non mi dilungo oltre perché è presidente di commissione per questo esame.

L'anno successivo era speso nel vano tentativo di farmi una piccola cultura teorica e nella resistenza passiva ai suggerimenti di lavorare in un gruppo sperimentale. Il successo di tale resistenza fu decretato da P. Bassi che mi portò seco a Ginevra per qualche giorno allo scopo di farmi discutere con S. Fubini su alcune oscure faccende di modello isobarico. Il fatto che le auspiccate discussioni non abbiano praticamente avuto luogo (grazie alla ben nota abilità di Fubini nell'evitare di parlare di quello che non gli interessa) non cambia l'importanza decisiva che questo viaggio ebbe per me. Fu Fubini stesso che mi propose di andare a Ginevra (sovvenzionato dall'Italia) un paio di mesi per lavorare al CERN. Cominciai così a lavorare ad un'applicazione numerica delle relazioni di dispersione, suggeritemi da D. Amati, che avrebbe dovuto portare alla determinazione del mesone [...]. [Esse erano sì] molto vantaggiose per la loro insensibilità ai contributi della regione non fisica e per la loro indipendenza dal segno delle ampiezze di transizione ma si rivelarono fortemente dipendenti dalla sezione d'urto $K^+ P$ a bassa energia [...] dove i dati sperimentali mancavano [...]. Prima che questo lavoro fosse terminato ci fu una provvidenziale crisi economica dell'INFN che ebbe come conseguenza il mio richiamo alla base. A questo punto Amati e Fubini mi suggerirono di far domanda per avere un posto come borsista alla divisione teorica del CERN, affinché potessi restare a Ginevra e terminare il lavoro iniziato. Per questa ragione ho chiamato provvidenziale la crisi dell'INFN. Sono certo che la permanenza per due anni al CERN è stata molto formativa per me, nel senso che ho imparato molto di più di quello che avrei imparato in un altro ambiente meno vivo. Sono molto riconoscente ad Amati e Fubini di avermi dato la possibilità di restare al CERN (Selleri unpublished document, 1961, pagg. 1-2-FSPA).

SECTION 1.5-PP. 75-77: THE ORIGINAL ITALIAN TEXT BY SELLERI UNPUBLISHED DOCUMENTS 1961

Cominciai a lavorare con F. Bonsignori a una possibile applicazione del metodo estrapolativo di Chew e Low ai risultati sperimentali ottenuti a Bologna sulle reazioni $p + p \rightarrow p + n + \pi^+$ (1) e $\pi^- + p \rightarrow \pi^- + \pi^+ + n$ (2)

Questa applicazione del metodo estrapolativo era stata suggerita da A. Minguzzi ma si rivelò ben presto impossibile con la limitata statistica a disposizione.

A questo punto ci chiedemmo se non fosse possibile trovare già nella regione fisica gli effetti del polo non fisico al quale volevamo inizialmente estrapolare.

[...] Il punto base dell'analisi di Chew e Low è proprio il fatto che il diagramma con un solo pione scambiato, OPE, diventa infinitamente grande in un punto mentre nello stesso punto altri contributi restano finiti. È pertanto chiaro che esiste un intorno di tale punto ove l'OPE è dominante. Purtroppo non sembrava possibile determinare la grandezza di questo intorno e sapere così se esso si estende alla regione fisica o meno. Ci chiedemmo pertanto quali tipici effetti uno debba aspettarsi nel caso che effettivamente l'OPE fosse dominante anche nella regione fisica per piccoli valori di $T = -\mu^2/2m$. La prima cosa che ci venne in mente fu la seguente. La reazione (2) è dominata dal formarsi dell'isobaro (3,3) nello stato finale [...]. Qualunque sia l'origine dell'isobaro, esso è certamente un contributo diverso dall'OPE. Perciò l'OPE non è dominante nell'insieme di tutte le energie per la reazione (2).

[...] la sezione d'urto differenziale in T predetta dall'OPE ha un massimo per bassi T e decresce piuttosto rapidamente all'aumentare di T . Quindi l'OPE predice un numero maggiore di eventi a bassi valori di T di quanti se ne avrebbero con le sole predizioni dello spazio delle fasi (teoria statistica). Esperimenti successivi [Derado 1960; Rushbrooke and Radojicic 1960; Pickup, Ayer and Salant 1960; Pickup, Robinson and Salant 1961; Erwin, March, Walker and West 1961, tutti su *Physical Review Letters*] mostrarono in maniera ancor più convincente questa caratteristica e provarono che nei processi (1) e (2), per energie attorno ad 1 GeV, l'interazione pione-pione è di primaria importanza (Selleri unpublished document, 1961, pagg. 3-5-FSPA).

[...] In una lettera successiva al Nuovo Cimento ["K-Meson Parity from Dispersion Relations", (Selleri 1960a)] discussi ancora in modo qualitativo le interazioni anelastiche $\pi n \rightarrow 2\pi n$ ad alta energia, avanzando la seguente spiegazione.

L'interazione primaria che origina la produzione singola [nelle 2 reazioni suddette] è sempre l'interazione del pione incidente con un pione virtuale della nuvola del nucleone urtato. Come passo successivo i due pioni uscenti possono ancora interagire col nucleone. Questa interazione sarà ora di bassa energia dato che i pioni uscenti sono più lenti di quello entrante.

[...] Tale interazione nello stato finale distrugge gli effetti tipici dell'OPE [...] La seconda [reazione] appare come la migliore per rivelare gli effetti tipici dell'OPE (Selleri unpublished document, 1961, pag. 5-FSPA).

[...] Nel lavoro [Bonsignori, Selleri 1960] notai che i dati sperimentali della reazione $p + p \rightarrow p + n + \pi^+$ (*) mostravano un caratteristico accentrarsi dei neutroni a basse energie cinetiche nel laboratorio, evidenza qualitativa di un importante contributo dell'OPE al processo. Questa reazione presenta [...] due vantaggi molto importanti sulle [suddette] reazioni (1) e (2) con un pione iniziale:

a) È improbabile che vi siano importanti interazioni nello stato finale oltre a quelle che sono già automaticamente incluse nell'OPE.

b) L'interazione πN che entra in uno dei vertici è ben nota sperimentalmente.

Perciò [...], mi buttai a capofitto nei calcoli per verificare l'esattezza dell'idea di Bonsignori e mia. Dovetti ben presto rendermi conto che il metodo tradizionale con spinori quadridimensionali portava ad una estrema complicazione nei calcoli anche quando si teneva conto (come feci io) di soli 2 dei 4 diagrammi di tipo OPE. Fortunatamente scoprii che l'uso di spinori bidimensionali semplificava notevolmente la complessità dei calcoli e questo mi permise di arrivare al risultato.

[...] L'accordo è buono come andamento generale ma illusorio come normalizzazione, dato che il risultato ottenuto e pubblicato ["Role of the Peripheral Interaction in Proton-Proton Collisions" (Selleri 1961)] è sbagliato di un fattore due come mi fu fatto notare prima da A. Stanghellini in discussioni private, poi da G.C. Wick e F. Salsman per lettera.

L'origine di questo errore stava fondamentalmente in una mia scarsa comprensione degli effetti matematici del principio di Pauli e secondariamente nel fatto che la stessa normalizzazione sbagliata è spacciata per buona nel libro di Bogoliubov e Shirkov [*Introduction to the theory of quantized fields*, New York: Interscience Publishers 1959].

Fortunatamente prima che mi rendessi conto dell'errore giunsero nuovi dati da Yale riguardanti la reazione (*) a energia più alta e mostranti in maniera inequivocabile la presenza del picco a bassa energia. I calcoli completi [...] furono fatti da G. Da Prato.

[...] Il risultato fu che una corretta normalizzazione portava a predire in modo sorprendentemente corretto sistema, posizione e normalizzazione assoluta del picco di bassa energia, nello spettro del laboratorio dei neutroni dalla reazione (*).

[...] La situazione si presentava come segue:

- ottimo accordo tra modello dell'OPE e dati sperimentali a bassi momenti trasferiti.
- Discrepanza via via crescente al crescere del momento trasferito.

[...] Poiché il lavoro di Da Prato aveva definitivamente chiarito che l'OPE gioca un ruolo importante nella reazione (*), era giunto il momento di spingere l'analisi oltre per arrivare a comprendere le limitazioni del modello con un solo pione scambiato e rivelare eventualmente i contributi con più particelle. Su questa linea di pensiero cominciai a lavorare con Ezio Ferrari che era giunto al CERN nell'Ottobre 1960. La collaborazione si presentava vantaggiosa per via della complementarità dei nostri caratteri: da una parte lui ha su di me il vantaggio di essere molto più esatto e ordinato; mentre dall'altra io ho rispetto a lui lo svantaggio di avere molto meno voglia di lavorare! (Selleri unpublished document, 1961, pagg. 8-10-FSPA).

[...] L'idea di determinare il fattore di forma pionico del nucleone col metodo discusso precedentemente l'ho presentata [con Ferrari] alla Conferenza del CERN sui fenomeni di alta energia ["Quasi-Elastic Scattering of High-Energy Protons in the One-Pion-Exchange Model"] (Selleri unpublished document, 1961, pagg. 14-FSPA).

SECTION 1.5-PP. 77-79: THE ORIGINAL ITALIAN TEXT BY SELLERI UNPUBLISHED DOCUMENTS 1961

1) Radiochimica periferica: Il gruppo dei radiochimici del CERN... [aveva] trovato [un anno fa] una sezione d'urto molto più grande del previsto per reazioni nucleari indotte da protoni di altissima energia... in cui il numero atomico del nucleo urtato cambia di un'unità, mentre il suo peso atomico resta inalterato. [...] G. Cocconi ci suggerì che un'interazione di tipo periferico poteva generare questi fenomeni. T. Ericson, R. Van de Walle e io elaborammo la teoria dell'interazione elementare con uno dei nucleoni del nucleo e del suo successivo riassorbimento. I risultati sono in buon accordo con i dati sperimentali. 2) Fotoproduzione doppia: In una discussione avuta passeggiando sotto i portici di Bologna, D. Boccaletti ed io varammo il programma ambizioso di "spiegare tutto" nei processi di fotoproduzione doppia, in particolare nella reazione $\gamma + p \rightarrow p + \pi^+ + \pi^-$ (**). La nostra idea era la seguente: Poiché S.D. Drell aveva suggerito che il diagramma da lui considerato ad altissima energia (decine di GeV) poteva essere molto importante anche a basse energie (0,5-1,5 GeV) e poiché ci si aspettava un contributo importante da parte del processo $\gamma\pi \rightarrow \pi\pi$ essendo il π iniziale uno dei pioni virtuali del nucleone, pensammo di mettere assieme le due cose e calcolare distribuzioni angolari, spettri energetici ecc. Purtroppo l'ambizioso programma iniziale si dimostrò irrealizzabile a causa del fatto che il diagramma di Drell dava un contributo molto minore del

previsto. [...] veniva a dimostrarsi falsa l'idea di Drell che esistesse evidenza a favore del suo effetto nei dati sperimentali a bassa energia. Bisogna però dire che l'importanza dei lavori di Drell nel campo delle interazioni indotte da fotoni di altissima energia non viene minimamente sminuita da queste considerazioni. [I nostri risultati] sono stati raccolti in una lettera [...] al *Nuovo Cimento*. 3) Diffusione elastica p-p ad alta energia: Accadde un giorno al CERN che un giovane sperimentale dal cognome famoso venisse ad offrire i suoi servigi (allo scopo di far pratica nella fisica teorica, diceva lui) ad E. Ferrari ed a me. Gli demmo da calcolare il processo di fotoproduzione di pioni [...] e con la certezza sprezzante che non l'avremmo mai più rivisto. Riapparve invece di lì a due giorni con i calcoli finiti ed esatti. Perciò decisi che U. Amaldi [Jr.] era un tipo molto in gamba e che valeva la pena di lavorare con lui. Cominciammo a lavorare assieme ad una applicazione della condizione di unitarietà che mi era stata suggerita tempo prima da Fubini. L'idea base è la seguente. La diffusione elastica P-P ad alte energie attorno al GeV appare essere dominata dalla diffrazione causata dalla grande sezione d'urto anelastica. D'altra parte l'elemento di matrice dei processi anelastici è noto in parte teoricamente, in parte fenomeno logicamente, dal lavoro di E. Ferrari e mio, indipendentemente dal fatto che uno creda o meno alla totale dominanza dell'OPE. Perciò dalla semplice condizione di unitarietà si può arrivare a calcolare quella parte dell'elemento di matrice elastico che viene dalla diffrazione (parte che noi crediamo dominante).

Il fine ultimo di questi calcoli è il paragone delle sezioni d'urto angolari così calcolate con quelle sperimentali (Selleri unpublished document, 1961, pagg. 15-17-FSPA).

[...] Voglio discutere brevemente quali sviluppi ci si può ragionevolmente attendere dai risultati presentemente acquisiti. Un problema [teorico] di grande importanza pratica è a mio avviso la soluzione completa e generale delle interazioni involventi particelle virtuali.

[...] visti i successi del modello dell'OPE a energie superiori a 900 MeV viene naturale chiedersi quale sia il limite inferiore in energia per la validità di tale modello. Purtroppo il modello è certamente non valido a bassa energia, nella sua versione attuale. Ciò è dovuto alla presenza accertata di una forte interazione nello stato finale portante talora alla formazione del deutrone. Perciò sarebbe interessante modificare il modello, tenendo conto di tale interazione, e paragonare poi i risultati ottenuti colle sezioni d'urto sperimentali. Un altro problema, a mio avviso interessante, è quello della diffusione elastica protone-protone a energie sotto la soglia per le reazioni anelastiche. [...] I termini che si possono calcolare sono: lo scambio di un solo pione. [...] lo scambio di due pioni contribuente a interazioni con grande parametro d'urto. Dato che la risonanza pione-pione è risultata essere a energia molto alta, lo scambio di un bipione risonante tra due nucleoni urtantisi dà luogo ad una forza di raggio molto piccolo. Perciò un contributo di grande raggio d'azione viene dallo scambio di due mesoni non interagenti tra loro.

Tale scambio può d'altra parte essere calcolato se si tiene conto che in ciascuno dei due vertici si ha sostanzialmente a che fare con una diffusione pione-nucleone in cui tanto il pione entrante che quello uscente sono virtuali. Perciò c'è la speranza di riprodurre i dati sperimentali di bassa energia tenendo conto dei due termini precedenti oltre che di un parametro da determinarsi sperimentalmente, che rappresenta tutti i contributi di corto raggio (bipione, tripione, ecc). [...]

Tra gli sviluppi futuri non deve dimenticarsi una ovvia estensione ad altri processi fisici del modello dell'OPE.

[...] Concludendo credo che già i soli problemi elencati qui possano dare da lavorare per un anno a una persona di buona volontà. E mi sembra che loro soluzione permetta di pubblicare lavori decorosi, anche se certamente non di importanza sublime (Selleri unpublished document, 1961, pagg. 18-20-FSPA).

SECTION 1.10-P. 116: THE ORIGINAL ITALIAN TEXT OF THE LETTER SENT BY SELLERI TO SEGRÈ

Caro Prof. Segrè, Le scrivo per chiederle informazioni circa l'esperimento $K^+ \rightarrow \pi^+ \gamma$ di cui parliamo a Vienna. Questo esperimento sarà decisivo per la vita o per la morte della mia teoria (più probabilmente per la seconda!) e naturalmente ne aspetto con ansia il risultato.

Decadimenti del tipo $K^+ \rightarrow \pi^+ \gamma$ sono stati cercati qui a Bari da Sergio Natali nel suo film dell'esperimento X2. Sulla base di $\Gamma(K^+ \rightarrow \pi^+ \gamma) \approx 5 \cdot 10^{-4}$ aspettava di trovare un evento e mezzo e non ne ha trovato nessuno. Chiaramente il risultato è poco significativo.

Circa il valore di $\Gamma(K^+ \rightarrow \pi^+ \gamma)$ ho avuto una discussione epistolare con Min Chen, ma non credo di essere riuscito a convincerlo che la mia stima è corretta.

Il suo argomento si basa su un diagramma perturbativo con un K virtuale e soffre di tre gravi debolezze:

- 1) Non è gauge invariante;
- 2) Necessita della conoscenza a priori della costante d'accoppiamento $K \pi$ che risulta notevolmente diversa a seconda che la si estragga da $K \rightarrow 3\pi$, da $\Lambda \rightarrow p\pi$ o da altri processi;
- 3) È perturbativo nel senso pre-dispersivo e quindi, comunque, fortemente dubbio (pensi per esempio alle predizioni perturbative per l'interazione πN di bassa energia).

La mia stima attribuisce $K^+ \rightarrow \pi^+ \gamma$ alla stessa basica transizione che fa avvenire $K^+ \rightarrow \pi + \pi^0$ ed assume denominanza di un mesone ρ . Pertanto, è in regola con le relazioni di dispersione ed è gauge-invariante.

Anch'io ho bisogno di una costante d'accoppiamento (quella relativa a $P \rightarrow \pi \gamma$), ma questa è stimabile teoricamente in modo preciso (Weinberg-Proceedings di Vienna) perché dipende da $P \rightarrow \pi + w$ (ottenibile da $w + \pi \pi \pi$) seguito da $w + \gamma$ (ottenibile da $w \rightarrow l^+ l^-$ o dai fattori di forma elettromagnetici del nucleone).

La mia teoria se la cava assai bene in campi molto diversi, quali lo sdoppiamento della A_2 anche nel canale KK_{bar} e la produzione sempre più probabile di coppie KK_{bar} ad alte energie e nei raggi cosmici. Inoltre, restano i successi [già] discussi nel mio lavoro.

Infine, data la situazione generale della fisica delle particelle elementari, mi sembra di poter dire che una rivoluzione è auspicabile e probabile. Se non sarà questa sarà un'altra, ma qualcosa di grosso dovrà pur succedere!

Le sarei infinitamente grato se potesse scrivermi due righe circa lo stato attuale dell'esperimento $K^+ \rightarrow \pi^+ \gamma$ (Selleri to Segrè, January, 8th, 1969-FSPA).

SECTION 1.10-PP. 117-118: THE ORIGINAL ITALIAN TEXT SENT BY FRANCESCO GUERRA TO 2017 SISFA CONGRESS

Franco Selleri è innanzitutto un fisico teorico di grande classe, capace di dare contributi di altissimo livello nei settori più avanzati della fisica teorica delle particelle elementari, fin dagli anni giovanili. Il suo modello “periferico”, che interpreta una parte rilevante dell’interazione protone-protone come dovuta allo scambio di un singolo mesone, trovò immediatamente molteplici applicazioni, in particolare sulla produzione di mesoni nell’urto e sulla diffusione diffrattiva, in una serie di lavori svolti in prestigiose collaborazioni, con Ugo Amaldi, Francesco Bonsignori, Ezio Ferrari. [...] Nella seconda metà degli anni Sessanta, la sua proposta di non conservazione del momento angolare avrebbe portato ad una migliore comprensione di alcuni aspetti peculiari della fisica di un insieme di particelle elementari “strane”. La proposta fu inizialmente accolta con grande interesse. Ricordo che a Napoli ne discutemmo con grande interesse. [Uno dei miei studenti decise addirittura di lasciarmi e andarea fare la tesi con Selleri, per questa questione della nonconservazione del momento angolare]. La proposta di Selleri era fisicamente ben fondata. Il momento angolare mancante sarebbe stato assorbito da una nuova forma di eccitazione del vuoto, detta spurione, privo di energia e momento, ma dotato di momento angolare. Lo spurione aveva una origine nobile, proposto addirittura da Heisenberg. Purtroppo la Natura aveva deciso di non utilizzare questa possibilità. La non conservazione del momento angolare si rivelò contraria all’evidenza sperimentale, e la proposta cadde (Guerra F., “Franco Selleri: the courage of ideas”, paper presented at the 37° SISFA National Congress, Bari: 28 September 2017).

SECTION 1.10-PP. 119-120: THE ORIGINAL ITALIAN TEXT BY (JACOB, MAIANI 2001)

Tra le scoperte di Fermi [...] elenchiamo [...] lo studio teorico e sperimentale della diffusione pione-nucleone con la convalida dell’indipendenza dalla carica, o invarianza di isospin, [e] il manifestarsi della famosa risonanza 3-3. [...] Verso la fine degli anni ’50 era già chiaro che il nucleone era un oggetto esteso (con dimensioni di circa 10^{-15} m) e con qualche struttura più interna, ancora da esplorare. Le molte particelle nuove scoperte nei raggi cosmici dimostravano anche che la lista di particelle fondamentali non poteva essere limitata alle poche coinvolte nella struttura dell’atomo. [...] [A fine anni ’40] i teorici si trovarono in grande difficoltà nel formulare previsioni verificabili per le interazioni forti. [...] la teoria delle perturbazioni [...] falliva miseramente con la costante di accoppiamento forte delle teorie del mesone.

[...] Nel loro sforzo per ridurre il numero di particelle fondamentali, Fermi e Yang furono portati ad assumere una interazione puntiforme (del tipo di Fermi) tra i costituenti del mesone π evitando così l’introduzione dei quanti di un campo inter-nucleone.

Questa ipotesi fu abbandonata in seguito, da Gell-Mann, in favore di una forza vettoriale. I quanti elementari di questi campi sono stati chiamati “gluoni” per il loro ruolo di “incollare” assieme i costituenti elementari. Sappiamo oggi che le interazioni dei costituenti mostrano una simmetria “nascosta”, chiamata colore, che gioca un ruolo fondamentale nel vietare gli stati legati fermione-fermione e permettere quelli fermione-antifermione. Di conseguenza, esiste un intero otetto di gluoni che sono in relazione tra loro per mezzo della simmetria.

Sakata introdusse nel quadro l'iperone Λ , che portava l'unità negativa della stranezza, ed estese il modello di Fermi-Yang alle particelle strane. Ciò che non funzionò nel modello di Sakata, e che ha compromesso molto l'affermazione di questa idea nei tardi anni '50, fu la struttura dei barioni strani, particelle di spin $1/2$, che decadono debolmente in un nucleone e un pione, scoperte nel frattempo (a volte chiamate anche iperoni) [...] Un passo cruciale in avanti fu lo schema dell'"ottuplice via" (eightfold way) di Gell-Mann e Ne'eman, basato sulla simmetria SU_3 , le trasformazioni unitarie in uno spazio tridimensionale complesso. Questa simmetria estende la simmetria di isospin di Heisenberg (SU_2) alle particelle strane. Nel modello di Fermi-Yang-Sakata questa simmetria è naturale perché ci sono tre costituenti base, e le trasformazioni SU_3 non sono altro che le trasformazioni unitarie di P, N, e Λ fra loro (i fermioni sono rappresentati da campi complessi, ed il loro spazio tridimensionale è conseguentemente complesso). Tuttavia l'idea fu di porre la simmetria su una base più astratta: assumere la sua esistenza e cercare di classificare le particelle secondo multipletti di SU_3 [...].

Sorprendentemente, si trovò che i mesoni si adattavano alle rappresentazioni di otetto e singoletto di SU_3 (come nel modello di tripletto) e che i barioni noti [e questo accadeva solo con la eightfold way] si potevano sistemare in ottetti e decupletti: quest'ultima è una rappresentazione di SU_3 mai considerata prima.

[...] Il decupletto di risonanze di spin $3/2$ era incompleto quando fu proposta la eightfold way. La scoperta della particella mancante, la Ω , che possedeva esattamente le proprietà predette, convinse i fisici che erano sulla buona strada!

[...] Per quanto comprovata con successo, la eightfold way non dava risposta alla domanda fondamentale: queste particelle, barioni e mesoni, devono essere tutte considerate elementari? Il passo successivo è dovuto a Gell-Mann e Zweig, che in maniera indipendente scoprirono che la struttura dei mesoni e dei barioni poteva essere riprodotta assumendoli come stati legati di un tripletto fondamentale di fermioni di spin $1/2$, i quark, chiamati u, d, e s. I mesoni sono stati legati quark-antiquark mentre i barioni sono fatti di 3 quark: la prima affermazione richiede che la carica di u sia maggiore di una unità della carica elettrica di d ed s (similmente al tripletto di Fermi-Yang-Sakata). Tuttavia, per riprodurre i barioni, i quark devono avere cariche elettriche frazionarie [...], con $Q(u) = 2/3$, $Q(d) = Q(s) = -1/3$.

[...] A metà degli anni '60, l'idea dei quark come costituenti degli adroni noti stava guadagnando credito. Tuttavia, e come appena detto, è solo con l'avvento del modello a quark che la teoria di Fermi trovò il suo completamento. Un π^+ è fatto di un quark u e di un quark anti d. Fare calcoli a livello dei quark con un buon livello di accuratezza è ancora oggi frequentemente impossibile (Jacob, Maiani 2001, pag. 259)

SECTION 1.10-P. 123: THE ORIGINAL ITALIAN TEXT OF THE LETTER SENT BY PIAZZOLI TO SELLERI

Caro Selleri,

ogni tanto ridiscutiamo fra noi la fattibilità dell'esperimento sulla fluorescenza.

Abbiamo raccolto qualche dato tecnico e sperimentale ed abbiamo deciso di scriverti per avere maggiori dettagli sulla tua proposta di esperimento. Non intendiamo dettagli teorici (per ora) ma sperimentali, o meglio informazioni che riguardino la stesura di un primo progetto di esperienza. Trattandosi di un esperimento di tipo completamente nuovo, non ci è neppure possibile formularti delle domande molto precise. Comunque provo a porne qualcuna:

- a) supposto che l'esperimento sia negativo, fino a che punto ha senso spingere il limite superiore della variazione percentuale della vita media?
- b) ritieni più conveniente la misura di una vita media lunga o corta?
- d) hai citato a caso il fenomeno della fluorescenza? Pensi che si possa usare anche il tempo di diseccitazione di un laser, per esempio?

Vorremmo insomma capire le esigenze imprescindibili dell'esperimento, anche perché avremmo trovato un elettronico esperto in misure accuratissime di larghezza di righe spettroscopiche e cose del genere. Se sei così gentile da scriverci su questo argomento, penso che potremo tentare di fare un progetto di esperienze (Adalberto Piazzoli to Selleri, 1 April 1970-FSPA).

SECTION 1.10-P. 124: THE ORIGINAL ITALIAN TEXT OF THE EMAIL SENT BY PIAZZOLI TO THE AUTHOR OF THE PRESENT DISSERTATION

Selleri era un eretico, era risaputo, e aveva pensato a questo esperimento che però è rimasto solo a livello di dialogo e nulla più, non è stato mai sviluppato nulla. Selleri dava una valore anche alla Ψ , non solo alla Ψ^2 . un esempio che mi viene in mente per descrivere la proposta di Selleri è quello di voler scuotere un tappeto sotto un edificio atomico. A Pavia abbiamo una tradizione di fisici delle particelle e di solidisti, esperti in fluorescenza. E c'è anche l'unico reattore funzionante in Italia che ovviamente produce solo fasci di neutrini. L'idea era di misurare il tempo di fluorescenza e poi rimisurarla in sala reattore, con valori dell'ordine di 10^{16} neutrini/s cm^2 . Se si fosse visto un effetto, non sarebbe stato da attribuire alla Ψ^2 , al neutrino, la cui sezione d'urto è 10^{-44} ma alla Ψ . Come detto, non si è fatto nulla, non posso confrontare luoghi con e senza neutrini. E poi, perché usare il reattore quando già il Sole invia $6 \cdot 10^{10}$ neutrini/s cm^2 ? Di Selleri ricordo un seminario a Frascati, abbastanza teatrale. Lui dav i valori degli spin sbagliati e l'uditorio era un po' in imbarazzo. In realtà la situazione strana era legata alla sua proposta della teoria della non conservazione del momento angolare. Tra l'altro, egli spiegava come la nonconservazione della parità portasse alla nonconservazione del momento angolare. Mi sembrava un po' tirata per i capelli (Prof. Adalberto Piazzoli-Personal communication to the author, 2 September 2020. My underline).

SECTION 1.10-PP.124-125: THE ORIGINAL ITALIAN TEXT BY THE EMAIL MICHELE BARONE SENT TO HIS SON

Franco nel 1968 aveva fatto un modello per spiegare la fisica dei mesoni K a spin $\frac{1}{2}$ basandosi sulla violazione della conservazione del momento angolare nel decadimento dei K per via debole. Questo modello, che entrava in competizione con SU(3), spiegava meglio di SU(3) i mesoni strani prodotti fino ad allora. Una delle prove sperimentali che in più avrebbero provato la validità del modello consisteva nella osservazione del comportamento anisotropo del decadimento del K^+ in $\pi^+\pi^0$.

Ciò doveva essere misurato osservando una asimmetria del 5-10% nel decadimento del $\pi^+ \pi^-$ di K^0 polarizzati.

Quindi l'esperimento proposto può dare ragione o torto a Franco.

Ho gli articoli a cui Franco si riferisce:

Relativistic invariance without angular momentum conservation 1968

The physics of spin $\frac{1}{2}$ K mesons 1969

(Michele Barone to Gaetano Barone, date unknown, maybe after 26 January 2011).

SECTION 1.10-P.125: THE ORIGINAL ITALIAN TEXT BY THE NOTE BY MICHELE BARONE

Poiché π^+ ha spin zero non dovrebbe essere sensibile all'influenza di un campo magnetico esterno, per cui la distribuzione di μ^+ dovrebbe essere simmetrica. Invece molti esperimenti rilevano una anisotropia dovuta ad un eccesso di muoni nella direzione opposta a quella del protone incidente che ha prodotto il pione.

[...] Poiché in passato molti altri esperimenti avevano rilevato una certa asimmetria, fu proposto [anche in seguito ai risultati ottenuti da Gaetano Barone] di fare al Paul Scherrer Institute un esperimento ad hoc che fu rifiutato [adducendo la scusa degli errori sistematici e anche del fascio non garantito monocromatico].

Dal punto di vista teorico se questa asimmetria fosse confermata, il pione non sarebbe più considerato come una quantità pseudoscalare che non possiede informazione sulla direzione di propagazione ma vettoriale, così come l'aveva ipotizzata Yukawa.

Franco nel 1968 aveva ipotizzato la violazione della conservazione del momento angolare (spin) nel caso del decadimento del mesone K (con una distribuzione asimmetrica $K \rightarrow \pi^+ \pi^0$) e degli iperoni Λ , Σ , Ξ ed inquadrava questa violazione in un modello delle particelle elementari alternativo a quello dei quarks e basato sulla similitudine con gli atomi eccitati in cui alte masse corrispondono ad alti valori del momento angolare.

[...] In Franco la reazione $p + p \rightarrow d + \pi^+$ ha misurato che π^+ ha $S=0$ ma poiché $p + p \rightarrow d + \pi^+ \rightarrow \mu + \nu$ (il cui S totale è supposto essere zero dal teorema V-A) allora la somma T+J (momento isotopico+momento angolare) è zero, quindi poiché essa non è conservata, lo spin del neutrino dovrebbe essere l'opposto di quello che si osserva (+ 1/2) perché si abbia la conservazione.

[...] Nel modello di Franco si risolve questa contraddizione [e in più si risolve anche il] problema della CP violation nel decadimento del K (Michele Barone notes, 16 April 2014-MBPA).

SECTION 1.12-PP. 135-139: THE FULL ORIGINAL ITALIAN TEXT BY THE LETTER OF SELLERI "LA PIRAMIDE AZTECA DELLA FISICA TEORICA DELLE PARTICELLE ELEMENTARI"

Una cosa che ha da tempo preoccupato lo scrivente è come potessero conciliarsi il numero assai elevato di fisici teorici delle particelle elementari con la pochezza dei risultati raggiunti, soprattutto in considerazione del fatto che in molti Paesi "sviluppati" gli elementi migliori di ogni annata si siano dedicati a tale attività.

Negli ultimi 10-15 anni alcuni argomenti di maggior interesse sono stati i seguenti, in ordine cronologico:

- il metodo di Tamm e Dancoff
- il modello statico
- le relazioni di dispersione
- la rappresentazione di Mandelstam
- i poli di Regge
- le simmetrie (SU_3 ...)
- il modello di Veneziano.

Ciascuno di questi e di tanti altri argomenti, una volta sparito, ha lasciato poco o nulla da ricordare. Gli ultimi tre sono tuttora in discussione, ma almeno per due di essi (poli di Regge e simmetrie) gran parte delle speranze sono già andate deluse. Come risultato il campo scientifico al quale sono stati dedicati più sforzi ha portato ad una comprensione quasi nulla dei fenomeni elementari. Come è stato possibile ciò? Attorno a questo problema principale ruotano tutta una serie di problemi secondari.

- 1) Che cosa determina l'improvviso apparire fiammeggiante di un nuovo argomento nel cielo della Fisica, se poi invariabilmente, si riduce ad un mucchietto di cenere tiepida?
- 2) Perché i rispettati e riveriti capi della Fisica mondiale sono sempre gli stessi od al più aumentano di un'unità solo per diretta cooptazione da parte dei capi già esistenti?
- 3) Perché la ricerca di tipo deduttivo (assiomatica, fisica matematica) è mantenuta in gran conto mentre quella di tipo induttivo (fenomenologia) è disprezzata?
- 4) Perché nessuno è disposto ad ammettere lo stato fallimentare di questa ricerca e viceversa molti parlano di continuo progresso della Scienza?

Queste ed altre domande trovano risposta naturale se paragoniamo l'insieme dei fisici teorici delle alte energie con una piramide azteca e se assumiamo che tale struttura tenda ad ingrandirsi ed a conservare nelle migliori condizioni il suo gradino superiore (quello dei grandi capi). La tendenza ad ingrandire è realizzata annualmente dall'entrata tra i ricercatori dei giovani laureati italiani, PhD americani ecc, e siccome il numero degli studenti universitari ed il numero degli istituti di fisica sono in continua crescita ne segue che l'entrata tra i ranghi teorici dei giovani laureati sarà crescente se il fascino esercitato dalle particelle elementari non diminuirà.

Con questo meccanismo ogni anno un nuovo gradino, più grande del precedente, si aggiungerà alla nostra piramide azteca. È chiaro che ad un certo punto questa struttura diventerà così grande che i grandi capi (quelli del primo scalino) ricoperti di prestigio, di denaro e di responsabilità non avranno più tempo per pensare. Non resterà loro che il tempo di applicare il principio del pescaggio: guardando nella folta schiera dei loro seguaci potranno decidere di volta in volta quale lavoro innalzare agli onori della grande cronaca in modo da:

- a) creare una nuova moda e quindi controllare il pensiero di coloro che stanno nei gradini successivi della struttura;
- b) condividere con l'autore il merito della nuova "scoperta".

Naturalmente se un grande fisico, mettiamo californiano, innalza così al suo livello un oscuro fisico, mettiamo inglese, per via di una, mettiamo, rappresentazione bidimensionale delle ampiezze di diffusione poco importerà se in seguito tale rappresentazione si dimostrerà falsa ed inutile.

Entrambi siederanno d'ora in poi al vertice della struttura, la loro posizione essendo giustificata dall'importante fatto di aver avuto successo, di aver rubato la scena per un certo periodo.

Evidentemente abbiamo ora la risposta alla domanda 1) (ed in parte a 2)) posta all'inizio. Un ulteriore elemento che aiuta a comprendere il punto 2) consiste nel fatto che scoperte di potenziale importanza fatte da elementi della piramide non interamente condizionati vengono semplicemente ignorate ai vertici superiori (e quindi dall'intera struttura). Gli esempi su ciò abbondano, ma ne citerò solo uno. SU_3 è stata scoperta la prima volta da tre giapponesi, ma non è certo a costoro che va il credito maggiore per SU_3 . Nonostante che queste osservazioni siano importanti per capire il perché di 2), non ritengo che esse risolvano completamente tale problema. Tornerò quindi a discuterlo più avanti.

Per quanto riguarda 3) è ovvio che il disprezzo del processo induttivo è funzionale per la stabilità della struttura in quanto uno scarso sforzo in tale settore renderà improbabile la scoperta di nuove regolarità da parte di persone non direttamente controllate dai capi e quindi renderà improbabile la nascita di nuovi capi e la conseguente ridistribuzione del potere scientifico. Probabilmente la validità di 3) è da ascrivere in larga misura all'affermarsi della meccanica quantistica nella formulazione della Scuola di Copenhagen. Tale teoria ha portato a drastiche conclusioni di portata metafisica, quali l'impossibilità di postulare l'esistenza separata di due sistemi che abbiano interagito nel passato. Evidentemente un ragionamento induttivo che parta dalle proprietà degli oggetti elementari è molto difficile quando non si possano pensare questi oggetti come esistenti, mentre uno deduttivo che parta dagli assiomi della meccanica quantistica e della relatività è molto più sicuro. Concludendo circa il punto 3), esso è fondamentale alla stabilità della struttura piramidale della fisica teorica delle alte energie e la sua validità è imposta dalla meccanica quantistica. Quest'ultima teoria dovrebbe pure essere analizzata a fondo, ma non è questo il luogo per farlo.

L'ultimo problema da discutere prima di venire alla questione principale è quello indicato con 4) all'inizio di questo scritto. È ovvio che se la piramide deve ingrandirsi di continuo occorre che i giovani siano attirati dall'argomento e quindi occorre che si dica loro che si fanno progressi e che il campo è affascinante. Forse è ora il momento di dire che non è necessario pensare i fisici teorici delle alte energie come una casta di mistificatori che vogliono aumentare i loro privilegi ed il loro potere e che mentono sistematicamente per ottenere questo risultato. Non ho dubbi circa il fatto che la stragrande maggioranza dei capi e di tutti i fisici dei vari gradini sia in perfetta buona fede. Quindi, se la presente analisi è corretta, si deve trattare di un fenomeno culturale che porta alle deviazioni discusse, dello stesso tipo dell'impostazione medioevale secondo cui la terra era al centro dell'universo. Tornando a 4) è chiaro che l'affermare il continuo progresso della fisica teorica serve ai teorici per giustificare a sé e agli altri la loro stessa esistenza. I ragionamenti che vengono fatti a supporto di tale progresso sono almeno di due tipi. In primo luogo si dice che, per esempio, anche se il modello statico non ha portato a nulla di buono in sé, esso ha tuttavia permesso di scoprire le relazioni di dispersione e così via.

In secondo luogo si afferma che anche qualora un campo non dia risultati è sempre importante averlo studiato e scartato perché così non si ripeterà l'errore in futuro. È ovvia la debolezza di tale impostazione.

Se la teoria A non ha portato a risultati validi ma ha permesso la scoperta della teoria B, non si può portare B a parziale giustificazione di A se nemmeno B ha portato a risultati validi. Inoltre l'aver passato quindici anni a dimostrare false tutte le teorie proposte A,B,C,D,... non sembra un buon modo di fare della ricerca.

A questo punto molte cose cominciano a chiarirsi, ma resta tuttavia un grosso dubbio. Come può il controllo del vertice essere così forte da permettere senza gravi rischi di trascurare le scoperte fatte da elementi non al vertice della struttura e non pescati? Certo i controlli ci sono e sono forti. Essi vanno dal bisogno di aver successo (e quindi di richiamare l'attenzione su di sé) che si traduce in un rapido adeguamento alla moda imposta dall'alto, al linguaggio oggettivamente mistificatorio (esempio: la meccanica quantistica non vale per certe osservabili quali la carica elettrica, ma nessuno dice così, si dice invece che esistono delle "regole di superselezione"), alla necessità di essere modesti e riconoscere la propria inferiorità rispetto ai grandi capi, per potere affermare la propria superiorità su quelli dei gradini inferiori. Ciononostante rimane difficile comprendere perché proprio nessuno abbia avuto l'idea buona ed abbia saputo dimostrare che quanto si fa ora è sbagliato e che la strada da scegliere è un'altra.

Per chiarire questo punto torniamo alle ipotesi di partenza. Ho detto che i soli criteri che accetto per spiegare i problemi posti all'inizio sono quelli dell'ingrandimento continuo della piramide e della stabilità del gradino superiore.

Come si vede non accetto il punto di vista che l'interpretazione razionale dei fenomeni naturali sia il criterio basilare che determina l'evoluzione della fisica (almeno di quella teorica delle alte energie). Possiamo allora raggiungere una comprensione globale della situazione se assumiamo che all'inizio ci si sia messi sulla strada sbagliata e che la si sia seguita fino ad ora. Evidentemente è ora molto difficile accorgersi di essere su un terreno falso in quanto ci si è troppo allontanati dalla strada giusta (che assumo esistente in qualche modo). Questa analisi si conclude quindi con la visione apocalittica dell'intera piramide azteca, sempre crescente, lanciata a galoppo sfrenato sulla strada del Nulla mentre la luce della Verità è ormai scomparsa all'orizzonte. Questa immagine permette di risolvere il problema principale del perché così poco si sia capito dei fenomeni elementari ed inoltre fornisce definitivamente la soluzione al problema n. 2). Le "scoperte" provenienti dagli strati più bassi della piramide possono essere trascurate dai capi, quando non faccia loro comodo il contrario, semplicemente perché nessuna di esse può essere oggettivamente importante, essendo esse ottenute da una cultura fondamentalmente falsa.

AUTOCRITICA

Naturalmente credo che la descrizione precedente sia basicamente corretta. È chiaro comunque che la descrizione data è esagerata e limitata e che molti altri punti vanno analizzati con cura. Questo scritto ha come sola ragione di essere la possibilità di riflessione generata dal contrasto dialettico tra l'ottimismo imperante e la pessimistica visione presentata.

IMPOSSIBILITÀ DI GENERALIZZAZIONE

L'analisi precedente non può essere applicata alla fisica sperimentale delle alte energie.

Certamente gli esperimenti sono stati condizionati dal pensiero teorico, perché uno sperimentale che si rispetti deve oggi essere al corrente delle ultime mode teoriche e sulla base di queste deve decidere quali ricerche compiere. Ma i risultati ottenuti hanno una loro validità oggettiva che trascende le idee teoriche che ne hanno determinato la nascita. Inoltre una fisica sperimentale, di tecnologia avanzata, come quella delle alte energie, comporta l'acquisizione di metodologie e di conoscenze collaterali (calcolatori, tecniche strumentali ecc) ed una ricaduta tecnologica probabilmente benefica sul piano economico.

Non conosco abbastanza altri campi della ricerca da poter giudicare se le idee presentate si possono applicare pure ad essi. Ritengo che qualcosa di questo tipo possa essere generalmente valido, ma che la fisica teorica delle alte energie sia un caso limite.

SUGGERIMENTO COSTRUTTIVO

Se l'analisi presentata è corretta l'unico modo di ritrovare la strada giusta sembra essere un ritorno alle origini della fisica delle particelle elementari. Sembra quindi opportuno uno studio delle idee e degli esperimenti dell'immediato dopoguerra e, soprattutto, la ricerca di punti di partenza diversi da quelli storicamente scelti (Selleri, *La piramide azteca della fisica teorica delle particelle elementari*, 155° meeting of the SIF Executive Board, March 2nd, 1970, SIF Bulletin n. 75, pagg. 13-17).

SECTION 1.12-PP. 140-142: THE FULL ORIGINAL ITALIAN TEXT BY THE LETTER OF ELIO FABRI

La lettera di Selleri vuole stimolare una riflessione e magari una discussione dello stato della fisica teorica delle particelle elementari. Personalmente credo che lo stimolo vada accolto e vorrei portare il mio contributo alla discussione.

I due fenomeni di base che Selleri rivela (continua crescita della piramide, ricerche dettate da mode di breve vita) credo non possano essere negati. Probabilmente l'affermazione sullo stato fallimentare della ricerca non troverà molte adesioni e forse ha bisogno di essere articolata meglio. Non si può trascurare che molte cose sono più chiare ora che non 15 anni fa, e non solo sul piano negativo. Tuttavia il giudizio di fondo mi sembra accettabile, considerando che:

- nessuna idea di teoria generale veramente valida è sorta dopo QED, ormai vecchia di 20 anni. E questa è tutt'altro che soddisfacente nelle sue basi, anche se dà previsioni di incredibile esattezza;
- tutti i tentativi di fondare una teoria delle particelle elementari su basi generali o si rivelano sterili o vanno incontro a difficoltà gravissime, o portano solo a teoremi negativi, di non esistenza, impossibilità ecc;
- in pratica gran parte della ricerca vive sullo sviluppo di idee non ben fondate e che non formano un quadro coerente; situazione questa accettabile in una fase euristica ma insostenibile a lunga scadenza.

L'atteggiamento di molti teorici può essere definito "testa nella sabbia": si conosce la situazione ma si continua a lavorare nel solito modo, sempre sperando che a qualcuno venga l'idea buona. Questo

però non elimina il senso di frustrazione e l'esigenza di giustificarsi agli occhi degli altri fisici: di qui, almeno in parte, deriva l'ottimismo ufficiale. Del resto questo senso di inferiorità è evidente anche nel discorso di Selleri che ritiene stranamente immune dalle sue critiche la fisica sperimentale.

Per gli aspetti sociologici, la mia analisi delle cause è diversa da quella di Selleri. Non mi sembra essenziale il principio della stabilità del gradino superiore. Lo vedo più come un effetto. È tutta la piramide che cerca un equilibrio che ne assicuri la persistenza. Ed è la società nel suo insieme che promuove o blocca una crescita.

La quantità non può sostituire la qualità. Il numero di problemi importanti che arrivano a soluzione non è proporzionale al numero di ricercatori. Un nome noto è un fattore che pesa nella scelta e già ciò spiega la stabilità del gradino superiore. È favorito anche il conservatorismo delle idee. Sempre dalla crescita continua si ha la necessità fisiologica, per chi voglia far conoscere il proprio lavoro, di ricorrere a tecniche di propaganda, anche a sacrificio dell'austera regola scientifica del tempo che fu. Uso dei congressi più come tribuna di pubblicità che come palestra di discussione; la proliferazione di lettere che non sono tali ma solo articoli che vogliono arrivare prima; pioggia di preprint ecc.

C'è poi la specializzazione, figlia della necessità di selezione. Non potendo stare dietro a tutto il ricercatore si difende specializzandosi. È evidente che anche questo va nel senso del conservatorismo delle idee: il super specializzato non si azzarda ad uscire dal proprio campo e non osa giudicare ciò che non può capire. Perciò, cooptazione dall'alto.

La crescita continua la crescita continua comporta infine un pesante condizionamento sul metodo di lavoro. Se il ricercatore ha scelto un campo molto studiato non può lavorare con calma, sotto pena di vedersi battere nel tempo. Se si rifugia in un capo poco esplorato ha vita difficile e rischia di restare ignorato anche se ha detto qualcosa di nuovo. È comprensibile che i molti, che non si sentono degli Einstein, scelgano la prima alternativa. Finirà per emergere non chi ha maggiore profondità ma chi è più pronto e ha più fiuto.

Motivazioni per l'atteggiamento di un sistema sociale verso la ricerca avanzata. La ricerca può avere, per una società capitalistica evoluta come quella USA (la sola che conti esaminare, data l'evidente subordinazione della nostra ricerca a quella statunitense) alcune funzioni utili: prestigio, preparazione a sviluppi tecnologici, copertura ideologica. Ma può anche comportare il pericolo della nascita di idee culturalmente rivoluzionarie.

È perciò importante salvaguardarsi da questo rischio; e ciò si ottiene, assai meglio che con un'ottusa ideologia, con l'incoraggiare nel ricercatore schemi di comportamento che ne promuovono la identificazione con la struttura sociale: corsa al successo, estrema divisione dei compiti, lavoro di gruppo inteso come rinuncia alla responsabilità individuale ecc. Il meccanismo di interazione ricerca-società premia quei ricercatori che meglio si conformano allo schema, conferendogli potere. Il comportamento dei capi verso il resto della piramide è perciò determinato: sono stati scelti in quanto davano garanzie di comportarsi nel modo voluto.

Quanto detto non riguarda specificamente la fisica teorica delle particelle elementari. È un punto in cui divergo decisamente da Selleri: non vedo niente che renda migliore la situazione della fisica sperimentale. Non ritengo corretto l'argomento della validità oggettiva dei risultati sperimentali. A me pare che la ricerca sperimentale sia spesso portata da una logica del sempre più grande.

L'argomento della ricaduta tecnologica è nullo. Anzi, è spesso la tecnologia a condizionare fortemente la ricerca. Anche nell'ambito della grandi macchine della ricerca in particelle elementari, l'esperimento grosso mangia il piccolo. Anche la condizione del ricercatore sperimentale non mi pare affatto migliore di quella del teorico. La trovo anzi più alienante. Il teorico sarà magari frustrato ma spesso è consapevole; lo sperimentale può invece trovar soddisfazione negli aspetti esteriori del suo lavoro e così dimenticare lo scopo che la retorica ufficiale ancora gli assegnerebbe: la Conoscenza della Realtà. Fino ad invertire i valori; fino a battersi in difesa del sistema che lo ha inghiottito.

Che fare. La battaglia non può essere che politica. Interventi di tipo scientifico o di politica scientifica. Aspetti scientifici. Mi sembra che Selleri (lezioni di Frascati 1969) individui nella MQ l'origine prima dei nostri guai presenti: nel suo carattere dogmatico e nella scarsa considerazione che hanno ricevuto proposte alternative. Sono abbastanza d'accordo sui fatti (meno sulla filosofia) ma non li ritengo molto rilevanti alla questione. Mi sembra che non siano ancora emersi risultati sperimentali che abbiano veramente creato serie difficoltà alla MQ. Ma non è tanto la MQ che va rivista con occhi nuovi, quanto tutti i fatti fondamentali della fisica delle particelle, dalla scoperta del μ in poi. Nello stesso tempo si dovrebbe mettere sotto processo non la MQ in generale ma la seconda quantizzazione e la teoria dei campi, le cui difficoltà e contraddizioni ci sono così ben note che ormai abbiamo imparato "to live with them".

Confesso però che non mi faccio troppe illusioni. Certo, un nuovo Heisenberg sarebbe accolto a braccia aperte (a vittoria raggiunta); ma intanto, quanti possibili Heisenberg sono stati dissuasi dallo studiare argomenti difficili ed impopolari e dirottati verso traguardi più conformistici? Ognuno di noi ha piccole occasioni di potere (argomenti di tesi, recensioni di lavori, assegnazione di borse) nelle quali inevitabilmente prende partito pro o contro la tendenza in atto. Se tutti quelli che condividono almeno in parte queste considerazioni vorranno usare il loro potere per sostenere la ricerca "difficile", forse potremo ancora vedere qualcosa di nuovo sotto il sole.

SECTION 1.12-P. 143: THE FULL ORIGINAL ITALIAN TEXT BY THE LETTER OF ANGELO BARACCA AND SILVIO BERGIA

Le lettere di Selleri (bollettino 75) e Fabri (bollettino 77) (sulla piramide azteca) hanno dato l'avvio ad un dibattito che merita di essere ripreso, approfondito e allargato, anche in relazione alle iniziative che la SIF ha preso ed ha in programma per il prossimo futuro.

Insoddisfazione che nuclei di ricercatori anche consistenti mostrano per alcune caratteristiche della ricerca attuale e in presenza della superspecializzazione di Fabri, riteniamo che fisici delle diverse discipline e scienziati operanti in campi diversi di ricerca comincino a comunicare fra loro.

Selleri e Fabri a nostro avviso hanno trovato il punto giusto da cui cominciare. Selleri ha cercato di dare una prima impostazione politico-sociologica di una situazione che giudica fallimentare; Fabri ha fatto un passo avanti nell'analisi della logica interna che regge e condiziona la ricerca. Noi abbiamo contribuito con osservazioni al metodo della ricerca odierna nella fisica delle particelle elementari.

Siamo convinti che nella scienza non tutto si riduca a questioni metodologiche ma siano presenti presupposti di diversa natura da mettere in rilievo. Trascureremo volutamente tutti i risvolti economici e sociali. Ci pare infatti che i fisici del settore, adagiati in una visione scienziata senza incrinature, abbiano dedicato ben poca attenzione a questioni metodologiche ed epistemologiche, che d'altra parte possono interessare strati più larghi di ricercatori. La proliferazione non ha fatto compiere dei reali progressi. In mancanza di una teoria, la fisica delle particelle procede oggi per modelli. Sono presentati o come tecniche di approssimazione "tratte dalla teoria" o come tentativi di estensione relativistica di tecniche sviluppate in teoria di potenziale. I presupposti del modello cadono ben presto nel limbo dei problemi poco eccitanti; si rinuncia quasi subito nella pratica a dimostrare qualsiasi cosa per fare ciò che sovente gli stessi esperti delle regole chiamano della "cucina", aggiungendo qualche polo o taglio o modificando qua e là la trattazione (aggiungendo qualche parametro). Mancando una teoria, spesso non si sa letteralmente di che cosa si stia parlando. Ma la conseguenza più grave sul piano metodologico investe il significato stesso della sperimentazione. L'esperimento tipico o è uno di routine o è una domanda precisa rivolta però solo a verificare una particolare predizione di questo o quel modello. Anziché risolvere dei problemi reali, molto spesso ne vengono così posti dei nuovi, fittizi, quando la previsione non è verificata, eventualità a cui il modello stesso si affretta a dare una risposta "spiegando" il risultato in definitiva con l'aggiunta di qualche parametro. È stato così allentato a dismisura il principio stesso di verificabilità, alla base del metodo delle scienze della natura. Una misura precisa di ciò si ha se si ricorda che la fase di verifica si riduce quasi sempre alla tecnica di "best fit" imposta dalla presenza di parametri non accessibili ad una determinazione diretta. Principio di multi dualità: tutti i risultati sono spiegati da tutte le teorie (D.R.O. Morrison).

Presupposto che le energie non ancora raggiunte spiegano i punti deboli di un modello. Tutto sarà chiaro nella regione asintotica. Non si arriverà mai all'utopica teoria delle particelle elementari.

Senza riandare ai condizionamenti esterni di una tale organizzazione della ricerca, ci sia una logica interna; si individuino gli schemi generali di pensiero e le opzioni di fondo che ne costituiscono i presupposti.

Un paradigma, per Kuhn, non deriva dall'applicazione del metodo scientifico ma da situazioni contingenti e da condizionamenti esterni, e il suo mantenimento ha anche precise funzioni di organizzazione del consenso e di affermazione sociale del gruppo di scienziati in questione, i quali si basano sulla estrema specializzazione per erigersi ad unici giudici della validità scientifica del loro lavoro. Il paradigma limita drasticamente il quadro dei problemi di cui la ricerca si occupa e diventa, prima o poi, un intralcio per il progresso della scienza. Come affermano D. Bohm e J. Bub (Rev. Mod. Phys, 38, 453, 1966):

it is not generally realized that in the present language we can only ask questions and consider experiments that do not go outside the structure of this language. As long as we use this language, no experiment is ever likely to be devised that could conceivably refute the basic postulates of quantum mechanics and thus in principle provide a test of these postulates. We shall always search for energy levels, scattering probabilities, magnetic moments, etc. And if they are not as predicted we can always suppose that a change of the forces between atoms, the introduction of particles with new properties, new groups such as SU_3 , SU_6 , and other such orders, etc, will eventually bring experiments back into agreement with the theory, without changing the basic postulates given above, except in the way of enriching and adding to them; we need never take a step that contradicts them.

Esiste in effetti da 6 anni una predizione ben precisa (Teorema di Bell) nella quale una teoria locale con variabili nascoste differisce dalla MQ; mentre per tutti gli esperimenti compiuti fino ad oggi le sue teorie coincidono, esiste almeno questa precisa possibilità che i limiti della teoria attuale non vadano ricercati nella “regione asintotica” ma in esperienze praticamente realizzabili oggi. Il fatto che lo sforzo in questa direzione sia insufficiente e non conclusivo, mentre molti gruppi sperimentali sono simultaneamente a caccia del primo “picco” insolito che viene segnalato, ci sembra rispecchi una “logica” distorta.

Proposte a SIF. Rileviamo ancora una volta la necessità che la scuola di SDS abbia il più ampio respiro, affronti un ampio spettro di problemi, garantisca l’espressione di tutte le correnti di pensiero e sia aperta a studiosi che si occupano di varie discipline. Per quanto riguarda il campo di SDS suggeriamo alla SIF di occuparsi del problema di creare possibilità di lavoro per i giovani intenzionati a dedicarsi.

SECTION 1.12-P. 147: THE ORIGINAL ITALIAN TEXT BY FRANCESCO CALOGERO, TAKEN FROM BONOLIS, MELCHIONNI 2003

[...] a un certo punto ho cominciato ad avere una specie di rigetto per la fisica delle particelle come veniva fatta allora, sulla base delle regole di dispersione, poi con la cosiddetta rappresentazione di Veneziano. Sempre di più avevo l’impressione che non esistesse un quadro teorico solido e che le cose che erano considerate interessanti venissero in qualche misura determinate dalle mode più che da una reale corrispondenza con la realtà sperimentale o con la reale esistenza di un quadro teorico solido. A un certo punto decisi, quindi, di lasciare la fisica delle particelle. Penso che sia stato alla fine degli anni ‘60, quando scrissi un lavoro insieme ad altri colleghi proprio sul modello di Veneziano, e lo trovai assolutamente futile” (Bonolis, Melchionni 2003, pag. 78).

SECTION 1.12-P. 148: THE ORIGINAL ITALIAN TEXT BY FRANCESCO CALOGERO, TAKEN FROM BONOLIS, MELCHIONNI 2003

“[...] Quello che non mi piaceva, in quella certa fase della fisica delle particelle elementari era che non valesse più il criterio che una cosa potesse essere vera o falsa, il criterio più importante era se era alla moda o se non lo era. Questo atteggiamento sussiste ancora in parte per la fisica delle particelle elementari teorica; in parte è accoppiato al fatto che, siccome la fisica delle particelle

elementari sperimentale richiede enormi fondi, la ricerca è accompagnata da un *battage* pubblicitario un po' ridicolo, per cui in ogni momento ci viene detto che siamo sull'orlo della scoperta in cui si capirà tutto, laddove ne siamo invece lontanissimi.

Viene presentato come una grandissima scoperta qualsiasi piccolo sviluppo, [...] piccolo rispetto all'architettura complessiva e al fatto che ancora ci sono un grandissimo numero di interrogativi e di questioni irrisolte, anzi di questioni che non si sa ancora come affrontare. In questo quadro preferisco non esserci, perché questo aspetto di continua pubblicità, propagandistico, mi disturba; così come mi disturba il fatto che le cose che sono considerate importanti lo siano più perché "di moda" che non perché effettivamente legate alla comprensione della natura, cioè allo sviluppo della fisica. [...] Non c'è niente di male nella ricerca della teoria finale, quello che c'è di male è nel far credere che si sia vicini alla teoria finale, quando se ne è lontanissimi. [...] si è veramente molto lontani dall'avere un quadro coerente della fisica delle particelle elementari. Ci sono dei grandissimi problemi aperti: [...] ci sono tante particelle elementari di cui non si ha la minima idea del perché ci siano. Si ha qualche ricetta per metterle insieme, delle formule che in qualche misura possono connetterle; ma non esiste una vera teoria per risolvere moltissimi problemi connessi alla fenomenologia delle particelle elementari (Bonolis, Melchionni 2003, pagg. 84-85. My underline).

SECTION 1.12-PP. 148-149: THE ORIGINAL ITALIAN TEXT BY FRANCESCO CALOGERO, TAKEN FROM BONOLIS, MELCHIONNI 2003

[...] per me a un certo punto è arrivata una fase di saturazione nei confronti della fisica delle particelle, che del resto proprio in quegli anni era in crisi. Però nella fisica delle particelle, per ragioni sociologiche, non si riconosce mai che c'è un periodo di crisi, ma al contrario c'è sempre una grande fanfara, legata al fatto che questa fisica per la parte sperimentale è molto costosa, e quindi ha bisogno di apparire sempre sull'orlo di grandi sviluppi per giustificare le enormi spese. In quel periodo, come adesso molti ammettono, c'era una fase di stanca e di vera e propria crisi della teoria. Io me ne sono completamente distaccato. Quello che a me allora, e ancora adesso, dà più fastidio è questo aspetto legato alla moda, di una fisica in cui in larga misura l'interesse degli argomenti teorici è determinato dalla moda. Intorno a questo fatto c'è poi tutta una sociologia di chi manipola la moda, di chi promuove se stesso e i propri amici sulla base di un miscuglio di aspetti scientifici, sociologici e di potere. [...] Mescolare il marketing con la fisica teorica mi sembra un po' una profanazione; vedevo, però, che veniva fatto, e mi sembra che venga fatto ancora oggi, più nella fisica delle particelle che in altri settori (Bonolis, Melchionni 2003, pag. 102. My underline).

SECTION 1.12-P. 149: THE ORIGINAL ITALIAN TEXT BY CARLO BERNARDINI, TAKEN FROM BONOLIS 2008

[...] I K, la stranezza e il mixing tenevano banco ma non vi era energia sufficiente. La teoria, SU(3), quark e regole di selezione a parte aveva preso strade poco eccitanti. Uno stuolo di specialisti si estenuava su problemi legati alla rappresentazione di Regge, all'analiticità delle ampiezze, alle relazioni di dispersione, alle regole di somma, all'assiomatica dei campi, al bootstrap, al problema di Pomeranchuk, ai modelli di 'evasione e cospirazione', alle ampiezze di Veneziano, alla 'democrazia nucleare' di Chew ecc. Francamente, visto a posteriori, il tutto appariva un brancolamento in una giungla di matematiche un po' gratuite (Bonolis 2008, pag. 371. My underline).

CHAPTER 2

SECTION 2.2-P. 176: THE ORIGINAL ITALIAN TEXT BY TAGLIAGAMBE

La differenza sostanziale tra la complementarità e la teoria degli insiemi quantistici consiste nel fatto che, secondo il concetto di complementarità, la causa del carattere statistico della maggioranza delle affermazioni della meccanica quantistica è la alterazione, incontrollabile per principio, dello stato dell'oggetto; mentre invece, secondo la teoria degli insiemi, la moderna meccanica quantistica aveva, sin dall'inizio, a che fare con veri e propri insiemi statistici.

[...] la teoria degli insiemi statistici ritiene di poter liquidare la concezione, da essa definita idealistica, della funzione d'onda come caratterizzazione dello stato di una particella atomica individuale: la funzione d'onda si conserva, in tale teoria, come caratteristica statistica, e il quadrato del modulo di questa funzione caratterizza la probabilità con cui si può trovare la coordinata della particella. L'errore dei cosiddetti "idealisti fisici", errore che conduce al tentativo di distruggere la teoria materialistica della causalità, starebbe proprio nel fatto che essi confondono illegittimamente le leggi statistiche con quelle dei singoli fenomeni (Tagliagambe 1972, pagg. 735-736).

SECTION 2.4.1-P. 228: THE ORIGINAL ITALIAN TEXT BY TRECCANI

[...] In Gran Bretagna, la *International history of the twentieth century and beyond* (2004) di Anthony Best, Jussi M. Hanhimäki, Joseph A. Maiolo e Kirsten E. Schulze, docenti della London School of Economics e del King's College di Londra, parla di uno «straordinario successo», che ebbe profonde ripercussioni non solo sull'economia e la società italiane, ma anche sulla politica del vecchio continente, dal momento che contribuì a fare dell'Italia una delle nazioni che più contribuirono al processo di creazione della Comunità economica europea e di integrazione fra gli Stati.

In Francia, la *Histoire économique et sociale du XX^e siècle* (2002), una raccolta di saggi di vari autori, curata da Jean-François Muracciole, dell'Università di Montpellier, riferendosi alla situazione italiana nel secondo dopoguerra sottolinea il pragmatismo che accomunò i protagonisti del processo di ricostruzione, e si sofferma in particolare sulle figure di Luigi Einaudi, promotore nel 1947 di un'importante riforma monetaria, e di Enrico Mattei [abile imprenditore, ma anche appassionato mecenate], che tra il 1945 e il 1962 portò avanti strategie industriali e politiche [...] innovative.

Negli Stati Uniti, Iván Tibor Berend, direttore del Center for European and Eurasian studies nella facoltà di Storia della UCLA, [...] nel suo libro *An economic history of twentieth-century Europe: economic regimes from laissez-faire to globalization* (2006) individua i settori trainanti dello sviluppo italiano nelle industrie che fabbricavano automobili, elettrodomestici, mobili e macchine per ufficio, tutti prodotti che in breve tempo divennero un punto di riferimento a livello mondiale sia per le innovative soluzioni tecnologiche sia per la qualità della progettazione e del lavoro di design (Treccani 2013).

SECTION 2.4.1-P. 229: THE ORIGINAL ITALIAN TEXT BY TRECCANI

[...] La vittoria della DC sotto la guida di Alcide De Gasperi (1881-1954) nelle elezioni del 18 aprile 1948 ebbe come effetto la definitiva collocazione dell'Italia [sconfita nella WWII] nel cosiddetto blocco occidentale e il suo inserimento nell'European Recovery Program, meglio noto come *piano Marshall*, lanciato dal governo degli Stati Uniti per sostenere gli alleati europei e tentare di riorganizzare l'intero sistema economico mondiale in contrapposizione al 'blocco comunista'. [...] Grazie al piano Marshall, attivo dal 1948 al 1952, l'Italia ricevette aiuti per circa 1400 milioni di dollari, che utilizzò per completare la ricostruzione [post bellica] e per rilanciare i settori dei trasporti, dell'agricoltura, dei lavori pubblici e delle industrie (Treccani 2013).

SECTION 2.4.1-PP. 229-231: THE ORIGINAL ITALIAN TEXT BY TRECCANI

A parte i timidi interventi legislativi del governo De Gasperi, la crescita si verificò senza una programmazione politica forte e netta, e seguì piuttosto le logiche del mercato, piegandosi all'iniziativa dei grandi gruppi industriali. Un primo significativo fattore di squilibrio riguardò il costo del lavoro: le aziende riuscirono a mantenere a lungo competitivi i prezzi delle merci sfruttando la manodopera e ostacolando le attività sindacali. Ancora più impressionante risulta il numero dei lavoratori che furono coinvolti nella dura repressione con cui le forze di polizia, guidate dal ministro degli Interni Mario Scelba, contrastarono gli scioperi. Per aver partecipato a manifestazioni sindacali, tra il 1948 e il 1954 vennero uccisi 75 operai, 5104 furono feriti e circa 150.000 arrestati (di questi ultimi, 61.200 vennero condannati). Si era in piena guerra fredda, e le pressioni degli Stati Uniti sul governo democristiano affinché limitasse l'iniziativa dei sindacati furono forti e insistite. Nel 1954 l'ambasciatrice statunitense in Italia, Clare Boothe Luce, incontrando il presidente della FIAT Vittorio Valletta gli disse che il suo governo avrebbe troncato i rapporti con l'azienda torinese se questa non fosse riuscita a ridurre il peso che tra gli operai aveva la CGIL. Il timore di incrinare i rapporti con gli Stati Uniti spinse allora i dirigenti della FIAT ad avviare ondate di licenziamenti e di trasferimenti interni dei sindacalisti più combattivi, che vennero mandati nei reparti dove il lavoro era più duro. Addirittura, l'azienda concesse alle forze dell'ordine di utilizzare gli elenchi dei dipendenti per avviare una schedatura di quelli sospettati di essere simpatizzanti comunisti: qualcosa di molto simile al maccartismo allora diffuso nelle istituzioni e nella società statunitensi. [...] Dopo cinque-sei anni di relativa 'pace sociale', nuove proteste scoppiarono nel 1962, quando la FIOM (*Federazione Impiegati Operai Metallurgici*, facente capo alla CGIL), nel corso delle trattative per il contratto dei metalmeccanici, chiese una riduzione dell'orario lavorativo da 44 a 40 ore settimanali, distribuite su cinque giorni invece che su sei. In luglio, a Torino gli operai di Fiat, Lancia e Michelin incrociarono le braccia, rivendicando aumenti di salario e il riconoscimento della terza settimana di ferie pagate. La loro mobilitazione durò un mese, durante il quale gli operai ricevettero il sostegno degli abitanti del quartiere di Borgo San Paolo. I lavoratori della Michelin occuparono la stazione ferroviaria di Porta Nuova, mentre per la prima volta dopo parecchi anni si astennero dal lavoro anche gli operai della FIAT Mirafiori: era l'alba di una nuova e tumultuosa epoca nella storia delle relazioni industriali in Italia.

Per quattro giorni, le forze dell'ordine e gli operai, armati con bastoni e bulloni, si fronteggiarono in piazza Statuto, mentre gli automezzi della polizia caricavano la folla. I dirigenti dei partiti di sinistra e alcuni giornalisti denunciarono la presenza nella folla di 'agenti provocatori', ma i processi contro i manifestanti arrestati dimostrarono invece che si era trattato di operai, in maggioranza giovani e originari del Meridione: segno che la speranza di uno sviluppo armonioso dal punto di vista economico e sociale non si era avverata.

La frattura tra i principali sindacati impedì a lungo al movimento dei lavoratori di avere un peso sufficiente per aprire contrattazioni contrattuali con le aziende, e le condizioni di vita dentro le fabbriche migliorarono solo dopo il 1970, quando venne approvato [sulla scia delle proteste] lo Statuto dei lavoratori. Un altro fattore di squilibrio della società italiana di allora può essere identificato nelle ondate migratorie, che in meno di vent'anni modificarono profondamente la struttura della popolazione, generando un rimescolamento senza precedenti. Il fenomeno assunse aspetti ancora più radicali al Sud, dove i giovani partirono in massa, avendo come mete principali Milano e Torino. [...] Qui trovarono un diffuso atteggiamento di ostilità e disprezzo, se non di vero e proprio razzismo, che si manifestava per prima cosa negli annunci immobiliari, in cui si specificava: 'qui non si affitta a meridionali'.

[...] Gli anni del miracolo economico rappresentarono il periodo chiave di uno straordinario processo di trasformazione che toccò ogni aspetto della vita quotidiana: la cultura, la famiglia, i divertimenti, i consumi, perfino il linguaggio e le abitudini sessuali.

I giovani italiani, influenzati dai loro coetanei inglesi e statunitensi, accentuarono i comportamenti trasgressivi nell'abbigliamento, nei gusti musicali e nelle scelte di vita. Contemporaneamente crebbe il loro coinvolgimento civile e politico, che a lungo andare, durante la fase di crisi che seguì il miracolo economico, divenne la valvola di sfogo di un sentimento di delusione a lungo covato. Tra il diffondersi delle prime forme di protesta giovanile e la crescita di un rinnovato senso civico, nella fase conclusiva del miracolo economico si manifestarono dunque diversi segnali di un nuovo mutamento dell'ethos collettivo, che sarebbe poi venuto alla luce nel 1968. Lo scontro tra generazioni sui vestiti e sulla musica, sugli stili di vita e sulla lunghezza delle gonne per le ragazze celava uno scontro ben più radicale sui modi di concepire la vita e la politica (Treccani 2013).

SECTION 2.4.1-P. 237: THE ORIGINAL ITALIAN TEXT BY FRANCESCO CALOGERO, TAKEN FROM BONOLIS, MELCHIONNI 2003

Io l'ho vissuto abbastanza in prima persona. Prima c'era stato il periodo delle occupazioni, con la morte di Paolo Rossi.

Uno dei motivi per cui mio figlio si chiama Paolo è che nacque proprio in quei giorni. Io fui in parte coinvolto nell'occupazione dell'università. L'occupazione dell'Università di Roma, nel '66, era diretta contro la preponderanza fascista nell'università, sia da parte di squadracce di studenti sia anche da parte di una certa gerarchia accademica. In seguito ci fu un'altra occupazione e man mano il movimento studentesco degenerò, a mio parere, e alla fine ci fu perfino una certa contiguità col terrorismo.

Quello fu un periodo significativo per tutti e anche abbastanza duro e difficile, perché il Dipartimento di Fisica qui a Roma fu per parecchi anni graveente handicappato a causa di un gruppetto di studenti estremisti che vi si era annidato all'interno, il cosiddetto "collettivo di fisica", e che usava metodi di lotta anche alquanto violenti, tanto che qualche docente è stato addirittura aggredito personalmente (Bonolis, Melchionni 2003, pag. 89).

SECTION 2.4.1-P. 238: THE ORIGINAL ITALIAN TEXT BY GIORGIO PARISI, TAKEN FROM BONOLIS, MELCHIONNI 2003

Il '68 è stato per me un'esperienza estremamente importante, perché io venivo da una famiglia di tradizione liberale però mio padre preferiva votare per la democrazia cristiana come baluardo più solido contro il pericolo dei comunisti. Quindi per me il '68 è stato tutto un periodo di cambiamento di prospettiva. Io avevo frequentato [...] una scuola in cui molti erano monarchici o almeno di destra. In quell'ambiente, essendo liberale, avevo finito con l'essere uno di sinistra. Durante il '68 ero contrario all'occupazione, come attività illegale; ma, una volta fatta, ho partecipato a tutte le attività e piano piano questo ha provocato in me un forte spostamento a sinistra. Nel '69 ero ormai completamente d'accordo, dal punto di vista ideologico, con la posizione marxista [...] [una] forte socializzazione è stata una conseguenza importantedi quel periodo, che mi ha dato un'apertura notevole (Bonolis, Melchionni 2003, pag. 296).

SECTION 2.4.1-PP. 239-240: THE ORIGINAL ITALIAN TEXT BY ANGELO BARACCA AND SILVIO BERGIA

La grande esplosione della contestazione nel '68 e negli anni immediatamente seguenti aveva dato l'avvio ad un processo di riflessione critica sulla scienza che non trova precedenti nella memoria dei ricercatori della nostra generazione. Molti di essi avevano subito allora [...] un processo di radicalizzazione politica; altri, probabilmente i più, pur senza schierarsi decisamente con le posizioni del dissenso nei confronti dell'*establishment* scientifico [...] e delle finalità stesse della ricerca, vennero proiettati in uno stato di diffusa insoddisfazione nei confronti dei contenuti e dei metodi della ricerca in atto. Non diminuisce il valore dirompente di quel movimento la contestazione che quello stato d'animo non trovava poi sovente modo di superare il livello del puro rifiuto, per la carenza di analisi specifiche ed indicazioni di una via d'uscita. Anche chi aveva maturato una scelta politica netta si trovava poi nella stretta di un'alternativa paralizzante: quella cioè tra l'abbandono puro e semplice di un'attività di ricerca in cui non riusciva più a credere o, posto che nulla di alternativo poteva venire individuato e che l'abbandono del proprio terreno scientifico di attività era giudicato politicamente sbagliato, la scissione totale tra l'attività politica e l'impegno di lavoro. Certo, non mancavano, né erano mancate negli anni precedenti, analisi sulla scienza capaci di travalicare i limiti angusti di una visione settoriale e di fornire un solido terreno per una riflessione generale; alludiamo, in specifico, a quelle analisi, di stampo marxiano, che sottolineavano l'importanza determinante del modo di produzione nel condizionare le scelte della ricerca e nel plasmare talune caratteristiche.

Esse avevano tuttavia, a nostro avviso, il difetto di essere troppo poco specifiche, di non essere cioè calate nel vivo di un campo di ricerca definito, cosicché si riducevano spesso a petizioni di principio, non verificate sulla realtà attuale (Baracca, Bergia 1975, pagg. 5-6).

SECTION 2.4.1-PP. 248-249: THE ORIGINAL ITALIAN TEXT BY SELLERI'S LETTER TO GARUCCIO FROM USSR

[...] Oggi è una bella giornata con sole e con vento. Dalla mia finestra vedo il Volga. La prima impressione che fa l'URSS è che quasi tutto sia di qualità scadente. Tranne l'aereo [IL-62M] che regge il confronto con quelli USA e tranne la gente che mi sembra la migliore del mondo, tutto il resto è un po' raffazzonato, incompleto, vecchiotto, scadente, mal fatto. Dai vetri della finestra il mondo appare distorto perché sono disomogenei.

[...] Ho appena finito di leggere e di sintetizzare le nove lettere che mi hai mandato nel 74-75. Leggendo ho anche capito perché ne avevo sentito il bisogno: si tratta della continuità fra la tua visita in URSS e la mia. Ho ripercorso tutta la tua avventura, dal problema della lingua, inizialmente drammatico, poi via via risolto [...], alla grave situazione economica che ha costretto Enza a partire e che vi ha costretti a penosi risparmi; dall'amicizia coi cubani con il generoso aiuto che vi hanno dato, alla scoperta anche sentimentale del PCI e della sua politica; dalla traduzione degli articoli di Cernanskij ai contatti con Feinberg e alla preparazione del seminario sulla disuguaglianza di Bell; dall'isolamento iniziale, alle reazioni per le lettere dei compagni: Michele Barone, Marco Matteuzzi, ecc. Cercherò di dare un senso di continuità a questa mia visita, di usare le tue esperienze per ottimizzare le mie. E cercherò di porre le premesse perché continuino i nostri contatti con l'URSS.

[...] Io penso che tu hai fatto anche troppo, sopportando condizioni durissime e spianandomi il terreno almeno nel senso che so come non debbo parlare del nostro lavoro: non debbo dire che c'è un nesso profondo fra Mat. Dial. e l'ispirazione di certa fisica.

[...] Ps: Il Mat. Dial. sembra dunque inutile nei rapporti con la gente sia in Italia dove ti tiri addosso reazioni pesanti che qui dove il collegamento con la fisica è fantomatico. Dunque ce lo teniamo per noi come profonda motivazione, come enorme potenziamento intellettuale. Se gli altri non vogliono sapere come fare per capire il mondo e la fisica peggio per loro. Ci regalano, in esclusiva, un potente mezzo d'indagine (Selleri to Garuccio, Dubna, 18 June 1977-AGPA. My underline).

SECTION 2.4.1-PP. 250-252: THE ORIGINAL ITALIAN TEXT BY SELLERI ON *CRITICA MARXISTA*

[...] L'isolamento culturale di coloro che si occupano di scienza ha un duplice aspetto: da un lato il ricercatore spesso non sente l'esigenza di occuparsi di letteratura, arte o politica e preferisce dedicare tutte le sue energie alla ricerca spinto dal desiderio di avere successo, di "fare una grande scoperta"; dall'altro lato i non scienziati che cercano di avvicinarsi alla ricerca trovano grandi difficoltà e non solo per il motivo ovvio che essi spesso non hanno un bagaglio matematico adeguato, ma soprattutto perché il ricercatore, con tutta la buona volontà, non è in grado di spiegare loro in termini comprensibili il problema di cui si sta occupando. Questa divisione tra scienza e cultura umanistica è stata lungamente discussa e viene di solito presentata come il problema "delle due culture".

Le cause essenziali di questa situazione sono a mio parere da ricercare nella struttura stessa della cultura scientifica contemporanea e risalgono a tre gravi rinunce compiute dai fisici nel periodo 1925-1936: la rinuncia alla storia, la rinuncia alla filosofia e la rinuncia alla realtà fisica.

a) Rinuncia alla storia

L'atteggiamento dei ricercatori verso la storia della scienza è spesso di disprezzo e di derisione e tanto più di avanguardia è un laboratorio o un istituto universitario, tanto maggiore è l'insofferenza verso i richiami alla storia e l'ignoranza della medesima.

La storia della scienza è considerata un'attività per ricercatori anziani oramai troppo spremuti e quindi incapaci di "produrre" sul fronte della ricerca.

[...] Le ragioni di questa esplicita e volontaria rinuncia dei fisici e di molti altri ricercatori a conoscere le contraddizioni ed i travagli dai quali è nata la loro cultura presente non sono difficili da individuare, dato che questa rinuncia è stata esplicitamente teorizzata e messa in pratica da alcuni dei più importanti creatori della QM.

[...] La battaglia che [...] famosi fisici [come Einstein, Planck, Schrödinger, Ehrenfest, de Broglie] condussero, senza successo, contro la formulazione finale di Bohr, Heisenberg, Born, Jordan e Dirac era una battaglia a difesa dei concetti di realtà fisica, di causalità, di comprensibilità e di attacco al principio di complementarità [...].

[...] si vedrà che il materialismo dialettico accetta l'esistenza di una realtà oggettiva indipendente dall'uomo, implica che essa sia a noi comprensibile ed afferma la validità del principio di causalità.

La posizione [di Einstein, Planck, Schrödinger e de Broglie] era [...] consistente con quella del materialismo filosofico.

[...] (b) Rinuncia alla filosofia.

Lo studio dei legami tra scienziati e filosofia è importante per comprendere i contenuti storicamente determinati, spesso assai rilevanti, delle teorie scientifiche. Questo studio è complicato dal fatto che i fisici evitano quasi sempre di dichiararsi d'accordo con qualche corrente filosofica.

[...] la causalità nel mondo microscopico e la conoscibilità del medesimo portano ad una disuguaglianza tra quantità misurabili che le stesse quantità, calcolate con la QM, non soddisfano. Siamo dunque ormai al punto che la contraddizione tra QM e materialismo dialettico può essere decisa sperimentalmente.

Comunque il contenuto filosofico della QM è ignoto alla stragrande maggioranza dei ricercatori che la usano acriticamente credendo di avere a che fare con una creazione superiore dell'intelletto umano in nessun modo contaminata da pregiudizi o condizionata dalla società di cui i fisici sono una parte. C'è dunque nelle comunità scientifiche un rifiuto della filosofia che è poi un'adesione inconsapevole ad una filosofia ben precisa.

Se si considera la filosofia come una specie di cerniera fra le varie attività culturali, l'ignoranza del contenuto filosofico della scienza non può che isolare la scienza stessa dal resto della cultura umana ed impedire una comprensione delle pressioni sociali e dei condizionamenti storici che favoriscono la nascita della scienza moderna (Selleri Critica Marxista 1972, pagg. 120-133. My underline).

SECTION 2.4.1-PP. 252-254: THE ORIGINAL ITALIAN TEXT BY SELLERI ON *CRITICA MARXISTA*

(c) Rinuncia alla realtà fisica

[...] Storicamente la rinuncia alla realtà fisica e alla sua comprensione è avvenuta in diversi momenti:

1) nel 1926-1927 con i lavori di Born e von Neumann c'è stata la rinuncia al principio di causalità, che è un diretto attacco alla comprensibilità del mondo materiale [...].

2) Nel 1927 con il cosiddetto principio di complementarità di Bohr si rinuncia alla comprensibilità dei fenomeni microscopici, affermando che certe contraddizioni che i fisici avevano incontrato nello studio degli atomi nel primo quarto di secolo (principalmente la contraddizione onda-corpuscolo) sono non risolubili in linea di principio [...].

3) Attorno al 1935 con la "soluzione" dei famosi paradossi di Schrödinger e di Einstein si porta un duro colpo allo stesso concetto di realtà fisica notando che la QM implica che due oggetti che siano stati in contatto nel passato, comunque remoto, non possono più essere assunti individualmente esistenti, comunque grande sia la loro distanza al presente.

[...] Chiunque conosca gli ambienti della ricerca scientifica sa bene che [il pragmatismo] è l'atteggiamento dominante.

[...] Anche il positivismo è largamente presente nella ideologia scientifica contemporanea [...]. D'altra parte la stessa nascita della QM fu fortemente marcata in questo senso.

[...] Il positivismo fu spesso attaccato assai duramente dai materialisti. Si potrebbero ricordare significative affermazioni di Marx su Comte; e di Engels sui positivisti del periodo 1880-1890, ma forse è più importante ricordare che Lenin dedicò un libro all'attacco di tutte le forme di positivismo con particolare attenzione a quello di Mach.

[...] L'ideologia scientifica contemporanea contiene [oltre al positivismo ed al pragmatismo] pure elementi di idealismo e di esistenzialismo. L'idealismo è presente nella descrizione teorica dei processi di misura, secondo cui è un intervento della coscienza del fisico che compie la misura che determina, in ultima analisi, le proprietà del sistema misurato. Non mi è possibile documentare questa affermazione, perché non si potrebbe evitare di parlare di vettori di stato, di riduzione del pacchetto d'onda e di miscele di prima e di seconda specie.

[...] Abbiamo visto che al ricercatore moderno non resta che uno stretto corridoio culturale nel suo pensare scientifico essendo state erette spesse barriere verso la società (rinuncia alla filosofia) e verso la natura (rinuncia alla realtà oggettiva). Si tratta inoltre di un mezzo corridoio, essendo stata chiusa la porta verso il passato dalla rinuncia alla storia. L'attività scientifica restante consiste perciò solo di una serie di slanci di inventiva teorica e tecnologica con i quali si cerca di chiarire certi fatti sperimentali e di inglobarli in qualche formula matematica riassuntiva.

[...] possiamo concludere che [la ricerca fisica contemporanea] è pesantemente condizionata da elementi culturali non solo tradizionali per la borghesia (idealismo, positivismo), ma anche di tipo irrazionalistico (esistenzialismo, pragmatismo) quali la borghesia cominciò ad adottare come reazione alle lotte proletarie ed all'avvento del socialismo in Unione Sovietica.

Appare quindi tutt'altro che casuale che la QM sia stata prodotta dal '24 al '27 quando l'Europa era scossa da una profonda crisi e quando in Europa centrale si stava preparando l'avvento al potere del nazismo.

[...] [Un'altra] questione riguarda la funzionalità per il sistema capitalistico dell'imbottigliamento della ricerca fondamentale in un corridoio senza uscita.

[...] D'altra parte, un'ulteriore avanzata nella comprensione del mondo fisico avrebbe potuto riservar chissà quali sorprese e, forse, avrebbe potuto mettere in pericolo la redditività degli investimenti fatti o mettere in discussione il [...] controllo [di capitalisti ed imperialisti] della ricerca e della tecnologia. Si comprende quindi che la borghesia non abbia avuto bisogno di far uscire la ricerca fondamentale dal vicolo cieco in cui si è cacciata negli ultimi quarant'anni. Compito della classe operaia è anche di modificare questo stato di cose e di favorire lo sviluppo delle forze produttive anche nel campo della ricerca fondamentale.

[...] Come si è visto [...] lo scontro che generò la QM ebbe luogo attorno ai concetti di realtà fisica, comprensibilità e causalità. È importante allora osservare che il materialismo dialettico afferma l'esistenza di una realtà oggettiva che si evolve in modo causale ed afferma che questa realtà è comprensibile agli uomini.

[...] Circa il problema della conoscibilità la posizione del materialismo è che il mondo fisico è senz'altro comprensibile da parte degli uomini, nel senso che essi possono concepire nel loro cervello delle immagini degli oggetti studiati corrispondenti alla vera struttura di tali oggetti.

[...] È quindi evidente che per un materialista dialettico è più corretta la posizione degli oppositori della QM (Einstein, Plank, Schrodinger, de Broglie,...) di quella dei creatori (Bohr, Heisenberg, Born, Jordan,...) (Selleri Critica Marxista 1972, pagg. 133-146. My underline).

SECTION 2.4.1-PP. 257-258: THE ORIGINAL ITALIAN TEXT BY SELLERI AND GARUCCIO FOR *MATERIALISMO DIALETTICO E MECCANICA QUANTISTICA*

Questo libro [...] contiene ventinove scritti di filosofi e fisici sovietici sul rapporto tra la struttura logica e filosofica della meccanica quantistica ed il materialismo dialettico di Marx, Engels e Lenin. Indubbiamente questa raccolta giunge in Italia in un momento assai opportuno, mentre accanto al marxismo italiano, tradizionalmente storicista nel suo ruolo di considerare la scienza, si va affermando una corrente vicina al materialismo dialettico, grazie anche alla pubblicazione dei sei volumi della "Storia del pensiero filosofico e scientifico" di L. Geymonat e del Quaderno di Critica Marxista "Sul Marxismo e le scienze", il cui successo editoriale testimonia del crescente interesse per le tecniche concernenti il rapporto tra classe operaia e ricerca scientifica.

Questo interesse non è certo casuale ma va fatto risalire, in ultima analisi, alla necessità di un nuovo tipo di sviluppo economico del nostro paese, dopo l'esaurimento del tipo di sviluppo degli anni '60, ed alle concrete proposte avanzate dalle organizzazioni politiche e sindacali del Movimento operaio, proposte nelle quali è indicato un ruolo ben preciso della ricerca scientifica e tecnologica come elemento vitale. Il rinnovato interesse in Italia per il materialismo dialettico è sovuto anche agli studi di L. Geymonat e della sua scuola, che hanno portato [...] ad una comprensione delle cause storiche ed economiche di quell'uso dogmatico del materialismo dialettico che portò al caso Lysenko.

Il valore dei saggi contenuti nel libro in esame sta anche, sono parole di Geymonat, nel “...vigore con cui ribadiscono la tesi dell’inscindibilità del materialismo storico dal materialismo dialettico”.

[...] sembrano emergere con forza due dati: il primo luogo l’intensa collaborazione di ricercatori delle varie discipline tra di loro e di ricercatori e filosofi [...] in secondo luogo l’impegno costante ad arricchire il materialismo dialettico con gli sviluppi più recenti della scienza.

[...] Tutto questo può ben portare a meste considerazioni sulla situazione italiana dove non solo permane profonda la spaccatura tra le due culture, ma si verifica persino una situazione di netto isolamento reciproco dei ricercatori di diverse discipline.

[...] Un pregio fondamentale del libro di Tagliagambe consiste nel proporre [...] le idee circa la natura della meccanica quantistica di [...] fisici e filosofi sovietici.

Apprendiamo così che nel 1957 Fock ha convinto Bohr a cambiare certe sue affermazioni, che potevano essere tacciate di positivismo, senza tuttavia cambiare la sostanza delle sue posizioni. Apprendiamo anche che il famoso filosofo Omelianovskij, che era stato a lungo un oppositore della meccanica quantistica, considerando la nuova comune posizione di Bohr e Fock l’ha trovata accettabile dal punto di vista del materialismo dialettico. Apprendiamo infine che nonostante questa importante convergenza di Fock, Omelianovskij e altri fisici e filosofi, permane in URSS una forte schiera di dissenzienti (Tyapkin, Pakhomov, Isayev, Blokhintsev, Terletski e altri) i quali, pur riconoscendo i grandi successi della meccanica quantistica, la considerano incompleta o imperfetta o incompatibile col materialismo dialettico (Garuccio, Selleri 1973, pagg. 303-305).

SECTION 2.4.2-PP. 262-263: THE ORIGINAL ITALIAN TEXT BY SELLERI

È molto difficile liberarsi di una calunnia dopo che è stata ampiamente diffusa, tuttavia l’accusa rivoltami di vetero marxismo sembra aver perso interesse, sembra essere andata fuori moda, senza dubbio dopo che i miei persecutori hanno soppesato ogni pagina da me scritta. Posso tranquillamente autodefinirmi materialista e ateo, ma con Marx – vetero o non vetero - c’è un problema insormontabile, quello dello storicismo. Se l’elettrone fosse figlio della società come pretende lo storicismo, ci dovremmo aspettare l’esistenza di una sua variabilità intrinseca. Il noto filosofo della scienza Ludovico Geymonat ha sostenuto la storicità dell’elettrone, mentre la fisica ha dimostrato che l’elettrone ha una vita sicuramente maggiore di 10^{26} anni e quindi enormemente maggiore del tempo di evoluzione dell’umanità sulla Terra che è stimato essere 10^6 anni. Secondo Geymonat vi sono movimenti in seno alla società che la ristrutturano di continuo, per esempio dei movimenti che modificano lentamente la struttura in classi sociali. Inevitabilmente le attività produttive di una siffatta società saranno tutte modificate, ivi comprese la produzione di scienza e le proprietà dell’elettrone. Ma allora la stessa esistenza dell’elettrone diventa incerta e può prevalere una tendenza o quella opposta a seconda delle circostanze concrete in diversi momenti. Non c’è alcuna evidenza che quello delineato sopra sia effettivamente il comportamento della materia, anzi va sottolineato che l’elettrone trova posto in tutte le teorie moderne, dalla prima teoria quantistica di Bohr-Sommerfeld alla teoria statistica di Fermi-Dirac. L’elettrone non è una variabile, non è un optional, ma un’onnipresente proprietà della materia. E io non sono marxista (FSPA. My underline).

SECTION 2.4.2-P. 263: THE ORIGINAL ITALIAN TEXT BY SELLERI'S LETTER TO MINAZZI

Tu sai come la penso. Il marxismo reale è stato uno sfascio filosofico che ha percorso e preparato lo sfascio politico/economico/civile delle società del socialismo reale. Sarebbe ridicolo dare tutta la colpa del fatto ai filosofi, ma lo sarebbe altrettanto il dimenticare quello che è successo. Ti ho già scritto che io sono andato alla ricerca di alleanze filosofiche fra i marxisti italiani, francesi, russi ecc e che non ne ho trovato uno di realisti. Il marxismo reale è convenzionalismo ed è storicismo. È una fregatura.

Se queste cose sono dolorosamente chiare a me che sono materialista, antiimperialista, anticapitalista, simpatizzante di Rifondazione Comunista, figurati ai filosofi italiani. Essi tolgono il mio dolore e vi sostituiscono il disprezzo. Per questo credo che sia un errore grave oggi qualificarsi come marxista. Tu dirai che al di là delle etichette conta la sostanza di quello che tu scrivi. Dovrebbe in un mondo ideale. Dentro al cono d'ombra invece contano anche le etichette, eccome (Selleri to Fabio Minazzi, 1 January 1992-FSPA).

SECTION 2.4.2-P. 266: THE ORIGINAL ITALIAN TEXT BY SELLERI'S 1972 LETTER TO GEYMONAT

[...] Io sono un ex-fisico delle particelle elementari che si è saturato per dieci anni di calcoli con il momento angolare complesso, con le proprietà di analiticità della matrice S, e altre analoghe porcherie.

Poi l'influenza del movimento studentesco prima e del PCI poi mi ha portato ad una riflessione autocritica e ad una crisi. Non puoi immaginare il senso di liberazione che si prova nell'uscire da sé stessi, guardare a quello che si è e si è fatto in modo nuovo, vederne l'enorme limitatezza, e insieme la rabbia per aver speso tante energie entro uno strettissimo corridoio culturale senza luce [...]. Insomma il capitalismo deve pagare anche per questo, e mi sono impegnato negli ultimi due anni a cercare di riempire le enormi lacune culturali che ho, a causa della sterile superspecializzazione cui mi avevano e mi ero costretto. Un altro colpo di fulmine è stata la lettura (in USA!) di un trattato di marxismo-leninismo edito dall'Uniove Sovietica. Come diventava tutto chiaro alla luce della contraddizione materialismo-idealismo! Così non c'è dubbio che il materialismo dialettico mi affascina, anche se non so rispondere alle critiche [...] forse solo [per] una questione di ignoranza (Selleri to Geymonat, 3 October 1972-Archivio Ludovico Geymonat (ALG), box 75, folder 792. My underline).

SECTION 2.5-P. 269: THE ORIGINAL ITALIAN TEXT BY SIF PRESIDENT TORALDO DI FRANCIA-30 OCTOBER 1968

[...] lo sviluppo storico della società nella quale viviamo ha reso necessario porsi delle domande di carattere fondamentale. Ci si chiede precisamente se i fini tradizionali siano ancora tutti da perseguire e se lo si debba fare con modalità tradizionali. Ci si chiede inoltre se non vi siano anche fini non tradizionali che divengano oggi importanti e che comunque siano degni di doverosa considerazione. [...] Non si tratta di facile cedimento a una moda imperante. Si tratta di non nascondere la testa sotto la sabbia.

Si tratta di avvertire che in un mondo in rapida e tumultuosa trasformazione, nel quale molte strutture tradizionali vengono esaminate criticamente e spesso giustamente messe sotto accusa, la comunità dei fisici non può esimersi dal fare un esame di coscienza e dal prendersi la sua parte di responsabilità nella costruzione di una società nuova e, speriamo, migliore.

[...] come non essere assaliti da seri dubbi [...] quando si vede la produzione concorrenziale ed affannosa di nuova scienza sfociare in sempre più spaventose applicazioni militari e in un'impostazione, attuata con tutti i mezzi su masse indifese, di sempre nuovi artificiali bisogni, che si trasformano di fatto in odiosa oppressione? (SIF Bulletin n. 63 of 28 November 1968, pag. 13).

SECTION 2.5-P. 270: THE ORIGINAL ITALIAN TEXT BY SIF PRESIDENT TORALDO DI FRANCIA-30 OCTOBER 1968

un rappresentante del Movimento Studentesco chiede di poter leggere un documento formulato dal Movimento medesimo. Toraldo ricorda che [...] lo Statuto della SIF non stabilisce che la riunione sia segreta, pertanto non ha difficoltà ad ammettere che assistano non Soci. Tuttavia è chiaro che [...] i non Soci [...] possono prendere la parola solo dietro autorizzazione dell'Assemblea dei Soci. Pur essendo di massima favorevole ad un colloquio con gli studenti, prima di qualsiasi intervento estraneo all'Assemblea, ragioni statutarie lo obbligano a completare gli argomenti all'ordine del giorno approvato in apertura di seduta. Poiché il Movimento Studentesco con vivaci proteste si dichiara contrario alla dilazione, Ricci e Castagnoli propongono di sospendere per 15 minuti l'Assemblea per ascoltare la breve relazione degli studenti. Si mette ai voti la proposta che viene approvata a grande maggioranza. Trascorsi i 15 minuti il Presidente riapre la seduta, ma dopo un ulteriore intervento del Movimento Studentesco che chiede la soppressione dei premi già assegnati dalla SIF a studiosi di fisica, constatata l'impossibilità di proseguire i lavori dell'assemblea secondo la prassi e lo Statuto, aggiorna la seduta a data da destinarsi (SIF Bulletin n. 63 of 28 November 1968, pag. 10-11).

SECTION 2.5-PP. 272-273: THE ORIGINAL ITALIAN TEXT BY SIF PRESIDENCY BOARD

Morpurgo [...] ritiene che il Consiglio avrebbe dovuto avere la sensibilità di indire un referendum fra i Soci prima di introdurre elementi politici. [...] Dichiaro che voterò contro la relazione del Presidente perché ritiene che l'entrata della politica nella Fisica risulti deleteria alla SIF. Cortellessa dice che è bene [...] precisare che cosa si intende per politica. È d'accordo che la politica in senso partitico non deve entrare nella Fisica. Ma dichiara che si dimetterebbe da una Società di Fisica che rifiutasse di discutere i fatti della vita reale e non facesse nel 1968 delle scelte politiche. Così come si dimetterebbe da una SIF partitica. Potenza si associa a Cortellessa ritenendo che non si può non fare politica. Zichichi esprime preoccupazioni per il contrasto tra i due differenti punti di vista. Chiede spiegazioni per il termine "politica". A. Romano [...] desidera che gli aspetti politici siano accentuati [...]. Porsi dei problemi generali vuol dire avere un atteggiamento politico. Potenza rifiuta una politica puramente scientifica a favore di una politica di carattere più umano. A. Bettini nota che finalmente si può parlare di politica. Non farlo significa solo che le scelte politiche sono fatte senza parlarne. Invita ad una maggiore coscienza sociale della categoria.

[...] Morpurgo deplora che non si parli più di fisica nell'assemblea SIF ma di ideologie e manifesta la sensazione che almeno il 50% dei Soci ambisca a diventare Onorevole. S. Ratti suggerisce di vedere se lo Statuto permette di fare politica. In caso negativo o si cambia lo Statuto o non si fa politica [...] Selleri si dichiara d'accordo con la relazione del Presidente. [...] Zichichi teme che scelte politiche siano anteposte a considerazioni scientifiche e culturali. Cortellessa riferisce che la relazione del presidente è stata approvata all'unanimità in una riunione del suo laboratorio. Franzinetti si congratula col presidente per il realismo mostrato [...]. Castagnoli si dichiara d'accordo con Franzinetti e fa rilevare che la relazione appare politicizzata, anche perché il Consiglio ha il mandato di proporre un nuovo Statuto. L'Assemblea deve perciò rispondere se vuole una SIF asettica ed accademica, oppure impegnata. Borsellino ricorda gli aspetti storico-politici della SIF, nata intorno al *Nuovo Cimento*, erede di quel *Cimento* fondato nel 1844 da fisici che poi nel 1848 interruppero le pubblicazioni per partecipare all'azione di Curtatone e Montanara. Invita a non rinnegare un passato a cui tutti siamo legati. [...] Salvini [...] enumera i problemi politici a cui vede giustamente accostata la Fisica e viceversa altri problemi nei quali la Fisica non c'entra. Ad esempio: Fisica e Società italiana, sì; Fisica e guerra sì, Fisica e valore scientifico di Stalin no. [...] La relazione del Presidente viene approvata con 208 sì, 1 astenuto e un voto contrario (SIF Bulletin n. 66 of 28 May 1969, pagg. 6-8. My underline).

SECTION 2.5-P. 280: THE ORIGINAL ITALIAN TEXT BY SIF PRESIDENCY BOARD

Il presidente legge una lettera di Selleri il quale, avendo studiato la situazione del *Nuovo Cimento* e in particolare l'evoluzione comparativa del *Nuovo Cimento* e di *Nuclear Physics*, ha riscontrato una notevole diminuzione di lavori sperimentali. Le proposte di Selleri per cercare di sanare la situazione sono:

- 1) accettare direttamente tutti i lavori sperimentali provenienti da laboratori importanti;
- 2) triplicare il costo degli abbonamenti per i soli paesi ricchi e abolire le spese di pubblicazione.

Bassani non ritiene che siano le spese di pubblicazione (salvo per gli inglesi ed i paesi dell'Europa orientale) ad influire sul numero dei lavori sperimentali, ma soprattutto il fatto che il *Nuovo Cimento* ha acquistato prestigio nel campo dei teorici piuttosto che nel campo degli sperimentali. Toraldo è propenso a credere che il provvedimento più immediato da prendere sia quello di cercare di abbreviare il più possibile i tempi di pubblicazione. Alberigi propone di dare più libertà ai Vice-Direttori nel senso che, se essi lo ritengono opportuno, i lavori sperimentali provenienti dai grandi laboratori vengano senza'altro accettati per la pubblicazione. Secondo Alberigi un altro sensibile miglioramento dei tempi si potrebbe ottenere qualora gli autori rinunciassero alla correzione delle bozze. A conclusione della discussione, come immediati provvedimenti, si delibera 1) di dare maggiore libertà d'azione ai vice-direttori; 2) di informare i membri del Comitato Editoriale di alcuni Paesi con difficoltà di conversione valutaria che il *Nuovo Cimento* è disposto a pubblicare gratuitamente un certo numero di lavori all'anno provenienti da tali Paesi (SIF Bulletin n. 66, 28 May 1969, pag. 3).

SECTION 2.5-P. 285: THE ORIGINAL ITALIAN TEXT BY SIF PRESIDENCY BOARD

[...] la questione del *Nuovo Cimento* si pone in termini gravi dal punto di vista finanziario perché, come è noto, da varie parti sono in corso azioni per cercare di limitare l'afflusso di lavori al *Nuovo Cimento* a causa dei gravosi contributi alle spese di stampa. [...] Selleri afferma che occorre rendersi conto che la situazione del *Nuovo Cimento A* non è una situazione che si sta deteriorando, ma che è già deteriorata, sia per quanto concerne i lavori sperimentali di alta energia, che gli autori, col pretesto dei contributi alle spese di stampa troppo alti, pubblicano su altre riviste, sia dal punto di vista di alcuni fisici stranieri, che in un recente incontro con Selleri, hanno lamentato un evidente calo del *Nuovo Cimento*. Occorre pertanto una azione tempestiva e radicale per salvare una situazione drammatica. Secondo Selleri, la situazione potrebbe essere sanata aumentando l'abbonamento al *Nuovo Cimento*, che è di molto inferiore a quello di altre riviste, mettendo così in condizione la SIF di eliminare subito e per sempre i contributi alle spese di stampa. Tale azione, a giudizio di Selleri, non dovrebbe comportare eccessivi rischi, considerando che la maggior parte degli abbonamenti provengono da biblioteche di grandi istituti che non rinunceranno all'abbonamento (SIF Bulletin n.73, 28 February 1970, pagg. 5-6).

SECTION 2.5-P. 285: THE ORIGINAL ITALIAN TEXT BY SIF PRESIDENCY BOARD

Ricci ritiene che occorra obiettivamente vedere se il problema del *Nuovo Cimento* è legato unicamente ai contributi ed agli abbonamenti, oppure se non vi sia una ragione più politica. Considerando il *Nuovo Cimento* nei suoi tre settori si può constatare che, per quanto riguarda le particelle elementari, si pubblicano meno lavori da quando la North Holland ha iniziato la pubblicazione dei lavori di alta energia su *Nuclear Physics*, malgrado la divisione del *Nuovo Cimento* in *A* e *B*. Per quanto riguarda la fisica dei nuclei la causa della scarsa richiesta di pubblicazione va ricercata nel fatto che all'estero il *Nuovo Cimento* non è riuscito ad inserirsi tra i fisici dei nuclei come *Nuclear Physics* e la *Physical Review*. Di conseguenza i lavori pubblicati sul *Nuovo Cimento* vengono raramente citati nei riferimenti bibliografici. Per quanto riguarda le *Lettere*, esse hanno avuto successo perché si sono inserite in un momento in cui le altre riviste pretendono lettere troppo corte. Le *Lettere al Nuovo Cimento* hanno un carattere di maggiore completezza e per questo trovano il consenso degli autori (SIF Bulletin n.73, 28 February 1970, pag. 6).

SECTION 2.5-P. 286: THE ORIGINAL ITALIAN TEXT BY SIF PRESIDENCY BOARD

[...] dopo ampia discussione il Consiglio perviene alle seguenti deliberazioni:

[...] Intraprendere immediatamente una azione la più efficace possibile nei confronti dei principali organi di Governo allo scopo di ottenere i fondi per l'abolizione totale dei contributi alle spese di stampa.

[...] Portare da tre a quattro i fascicoli mensili delle *Lettere al Nuovo Cimento*.

[...] Reinserire la fisica dei nuclei nella parte *A* del *Nuovo Cimento*.

Selleri si dissocia da questa decisione, ritenendo che si sarebbero potuti abolire oggi stesso i contributi alle spese di stampa aumentando di due o tre volte l'abbonamento per le sole biblioteche di paesi ricchi (SIF Bulletin n.73, 28 February 1970, pag. 7).

SECTION 2.5-PP. 289-290: THE ORIGINAL ITALIAN TEXT BY SIF PRESIDENCY BOARD

Prima mozione: “L’Assemblea dei Soci della SIF fa proprio lo Statement on Vietnam, approvato a maggioranza dai partecipanti alla Scuola di Varenna 1972 sulla Storia della Fisica [...], ribadisce la condanna di quei fisici che hanno avuto, in quanto membri della divisione JASON, una diretta corresponsabilità nella sistematica applicazione di scoperte scientifiche per l’aggressione imperialista USA in Indocina e invita vivamente la SIF a escludere da ogni sua iniziativa scuole, congressi, convegni ecc tutti gli scienziati di cui sia stata provata la appartenenza alla divisione JASON”. [...] la seconda mozione è questa: “Proposta di programma di collaborazione scientifica con la Repubblica Democratica del Vietnam. [...] riteniamo che sia entro le nostre possibilità di prendere iniziative antagoniste alle attività dei fisici della guerra. Iniziative che contribuiscono a rafforzare la capacità di difesa e di ricostruzione nei paesi dell’Indocina più brutalmente colpiti dall’attacco americano, che aiutino questi paesi sin d’ora e dopo la loro vittoria a definire un programma di sviluppo tecnico e scientifico. L’assemblea dei Soci chiede quindi che la SIF prenda direttamente contatto con le istituzioni scientifiche della Repubblica Democratica del Vietnam e con la delegazione della stessa a Parigi per un programma organico di collaborazione scientifica [...].

SECTION 2.5-P. 291-292: THE ORIGINAL ITALIAN TEXT BY SIF PRESIDENCY BOARD

Permettete che prima di tutto io esprima la viva riconoscenza della SIF ai colleghi di Bari, in particolare al prof. Michelangelo Merlin e al dott. Luigi Nitti che lo ha direttamente coadiuvato, per avere accettato un compito così gravoso qual è divenuto quello di ospitare un Congresso annuale della SIF. Non sarà sfuggito a nessuno quanta cura e quanta intelligenza siano state spese per rendere l’organizzazione locale efficiente e per offrire ai Congressisti un soggiorno piacevole in questa simpatica e bella città. In molti soci è ancora vivo il ricordo del congresso tenuto in questa sede sei anni orsono. Il fatto che il prof. Merlin ed i suoi collaboratori, non ignari per precedente esperienza di che cosa li aspettava, abbiano così volenterosamente ospitato il presente congresso li raccomanda alla nostra profonda gratitudine.

[...] La società italiana e l’umanità tutta stanno vivendo un periodo acutamente travagliato sul piano ideologico. [...] Fra il compito relativamente facile di individuare e criticare strutture od istituzioni non più adatte ai nostri tempi e quello estremamente difficile di indicare in concreto che cosa sostituire ad esse, troppi cedono alla tentazione di limitarsi strettamente al primo. Ma non si può onestamente reggere una società scientifica come la nostra senza pensare molto seriamente anche al secondo.

[...] La comunità dei fisici italiani doveva, per quello che le competeva, prendere parte attiva ed assumersi le sue responsabilità nella difficile situazione evolutiva del paese, anziché limitarsi ad un’agnostica ed asettica promozione del sapere scientifico. In altre parole si riconosceva la necessità di effettuare anche scelte “politiche”, pur mantenendosi lontani, ovviamente, da posizioni “partitiche”.

[...] Naturalmente non c’è ragione che il dibattito e l’intervento politico si limitino strettamente al nostro Paese.

A questo proposito voglio ricordare soprattutto due prese di posizione in campo internazionale: quella assunta e resa pubblica dal Consiglio di Presidenza a proposito dell'allontanamento dall'Università di alcuni fisici in Brasile e quella di solidarietà con i fisici cecoslovacchi, che ho avuto occasione di assumere mediante dichiarazioni che quei valorosi colleghi hanno pubblicato sul giornale di fisica del loro paese (SIF Bulletin n.74, 13 April 1970, pagg. 1-3).

SECTION 2.5-PP. 292-293: THE ORIGINAL ITALIAN TEXT BY SIF PRESIDENCY BOARD

Alberigi, dichiara che [...] le sue dimissioni riguardano la carica di Segretario-Cassiere, e non quella di Consigliere in merito [...] Alberigi sostiene che i motivi si riferiscono puramente ad una questione di bilancio ma essenzialmente alla discriminazione in tema di finanziamenti: questo aspetto è puramente politico. Alberigi dichiara di non sentirsi più di procedere ad occuparsi dell'amministrazione della Società, dovendo talvolta attuare decisioni (quale è quella relativa al finanziamento NATO) sulle quali egli non è politicamente d'accordo. [...] Toraldo dichiara che a questo punto non rimane che prendere atto con vivo rammarico della decisione di Alberigi. Tiene a sottolineare che il dissenso politico rimane puramente a questo livello, poiché la stima reciproca è tale che nessuno la può disconoscere (SIF Bulletin n.73, 28 February 1970, pagg. 12-13. My underline).

SECTION 2.5-PP. 294-295: THE ORIGINAL ITALIAN TEXT BY SIF PRESIDENCY BOARD

[...] G. Cortellessa: [...] la Scuola di Varenna deve rimanere aperta, però possiamo rinunciare al contributo della NATO. Quindi mi associo senz'altro a De Marzo e non so se lui presenterà una mozione in questo senso, perché altrimenti la presenterò io. Devo ricordare che anche altre Società di Fisica, non sospette in alcun modo di avere delle stranissime inclinazioni politiche, hanno dato un taglio deciso ai contributi di questo tipo. [...] Quindi dico: facciamone a meno (SIF Bulletin n.74, 13 April 1970, pag. 17. My underline).

[...] G. Toraldo: [...] è vero che di diritto e di fatto oggi la NATO non ci pone nessuna limitazione. [...] dato che [...] dobbiamo scrivere sul bando che la scuola è finanziata anche dalla NATO, molti colleghi dei Paesi dell'Est che noi avremmo in animo di invitare possono avere una pesante remora ad accettare. Quindi la partecipazione può essere limitata in modo indiretto (SIF Bulletin n.74, 13 April 1970, pag. 19).

[...] [La] mozione relativa alla NATO, a firma De Marzo-Cortellessa suona così: "L'Assemblea della SIF [...] constatato che dal bilancio risulta un finanziamento da parte della NATO, delibera di rinunziare cortesemente a tale contributo, onde sia nettamente stabilita l'indipendenza della SIF da qualunque organizzazione o attività di carattere militare" (SIF Bulletin n.74, 13 April 1970, pag. 20. My underline).

[...] G. Toraldo: [Tornando alla NATO], il Consiglio di Presidenza nella sua maggioranza, anche se forse non sarà stata l'unanimità, avrebbe preso in considerazione molto volentieri la decisione di fare a meno dei fondi NATO. Ma, data l'importanza della cosa, non si sentiva di prendere un provvedimento senza aver consultato l'Assemblea.

[...] G. Bellettini: [...] Voterò con piacere a favore di questa mozione. [...] È molto meglio [...] conservare in tutti i casi una completa indipendenza in tutta la propria politica scientifica (SIF Bulletin n.74, 13 April 1970, pag. 24).

SECTION 2.5-P. 298: THE ORIGINAL ITALIAN TEXT BY SIF PRESIDENCY BOARD

[...] non possiamo nasconderci [...] il fatto che le divergenze ideologiche (in senso lato) fra i fisici non sono mai state così vivaci come al momento presente. Personalmente non mi dolgo di questo. Ritengo che l'ammettere franca e larga discussione ed accettare l'apporto di tutti, specie dei giovani, sia molto meglio che seguire compatti la guida di pochi grandi maestri illuminati (SIF Bulletin n. 75, 4 May 1970, pag. 5).

[...] Basteranno poche cose per far capire da che parte sto, a chi non lo sapesse già. [Riguardo l'Università] ritengo che la contestazione sia stata la cosa migliore che la società borghese abbia saputo esprimere negli ultimi anni. I suoi effetti sulla concezione della Scuola e dell'Università in particolare, sono benefici e spero irreversibili. L'Università deve essere democratizzata con partecipazione di tutte le componenti a tutti i livelli (SIF Bulletin n. 75, 4 May 1970, pag. 11. My underline).

[...] Non ho dubbi che la maggiore politicizzazione della SIF, avvenuta di recente, sia una cosa buona. Chiudersi nella torre d'avorio degli studi e perdere di vista la realtà e gli interessi della società italiana sarebbe pernicioso. Certamente a me non interesserebbe una SIF così fatta (SIF Bulletin n. 75, 4 May 1970, pag. 12. My underline).

SECTION 2.5-PP. 303-304: THE ORIGINAL ITALIAN TEXT BY SIF PRESIDENCY BOARD

L'Assemblea della SIF, nella sua seduta del 30 Ottobre 1973, esprime condanna del golpe provocato in Cile dalle forze reazionarie interne e internazionali che, con l'appoggio del capitale monopolistico americano hanno rovesciato il Governo legittimo del Cile, assassinato il Presidente democraticamente eletto Salvador Allende e arrestato, torturato e fucilato migliaia di lavoratori e militanti della sinistra cilena; condanna le azioni compiute dai "golpisti" contro gli studenti, la cultura e la scienza cilena, con fucilazione di centinaia di studenti e docenti democratici, con roghi di libri, documenti e quadri con l'imposizione di militari alla guida delle Università.

Invita le forze culturali e gli insegnanti a dibattere nelle loro sedi operative il significato degli avvenimenti cileni. Invita le forze democratiche italiane a trovare nell'esperienza cilena una indicazione per una decisa politica di riforma e di eliminazione degli squilibri sociali per emarginare e sconfiggere il fascismo nel nostro Paese. Chiede inoltre che il Governo italiano non riconosca la giunta militare cilena e conceda assistenza politica ai resistenti cileni. [...] Approvata a maggioranza con 1 contrari e 10 astenuti.

SECTION 2.6.2-PP. 320-322: THE FULL TEXT BY THE DOCUMENT TITLED "NOTES ON THE CONNECTIONS BETWEEN SCIENCE AND SOCIETY", WHICH APPEARED AS EXCERPTS IN THE THESIS

NOTES ON THE CONNECTIONS BETWEEN SCIENCE AND SOCIETY BY A GROUP OF PARTICIPANTS TO THE 1ST COURSE (ON FQM) OF THE INTERNATIONAL SCHOOL OF PHYSICS "E. FERMI"-TYPEWRITTEN

INTRODUCTION

A group of participants in the school has gathered together several times to discuss problems which confront us as researchers living and working in the present society.

This course on Foundations of Quantum Mechanics, at variance with others more technical one, has gathered a great number of people interested in studying the structure of physical theories, their values and limits, and who are also interested in understanding their developments and how they have acquired their particular forms.

In and out the conference room, problems of philosophical character have been touched upon, but too often in a superficial way. In several occasions it was evident how philosophy, logic, historical knowledge are scarce in the culture of the average physicist. It looks like his professional training has been done at the expenses of a broader cultural basis, one capable of putting him in vital connection with social life.

The average physicist seldom arrives at a deep insight into the origin of physical theories, the historical and cultural conditions preading to their birth. Besides, as everybody knows nowadays, the scientific community has no control whatsoever on its products.

All those problems, interlaced as they are in a complex way, gave rise to lively discussions; our aim was to look for some answer or perhaps, more realistically, to formulate some problems and to make clear some specific points.

It was our intention, then, to present these points to the other participants so as to arise their interest and to promote a discussion that could have helped in exchanging ideas and experience and, may be, in reaching some common conclusion. Unfortunately lack of time and a number of adverse circumstances have made this impossible.

Notwithstanding this we think it useful to summarize in the following pages the main themes of our discussions in the belief that answers to these problems may only come from a wider awareness and ??? disposition to act accordingly.

GENERAL CONSIDERATIONS ON THE SOCIAL RESPONSIBILITY OF THE SCIENTIST

We recognize first that an important social function of science is to organize consensus and to maintain social stratification. The myth of the neutrality of science, reinforced by the common concept of the intrinsic positive value of all technological progress, is used as a factor of social cohesion to evade confronting the real problems which lead instead to class struggles.

Examples of this relationship between science and the social order are the space research, the links with military activity (we recall that Chomsky frequently insists on this aspect, as factor of social consensus), surgical transplants.

These aspects must be inserted in the global pattern of the technologically advanced society in which the evaluation and accumulation of the capital together with the efficiency of the system dominate over human values.

In a social structure of this type, production is organized on very large dimensions, and this implies the existence of massive authoritarian social organization, whose coercive instruments are however more and more artificial, manipulated and devoted to the creation and the maintenance of a general consensus rather than to specific intervention such as police or the administration of the justice.

It is therefore not accidental that scientific progress is usually presented as a liberation from physical suffering and as the effect of an ordered and integrated social organization.

It is instead extremely important to realize that science is certainly not neutral, and that the same holds moreover for the very structure of scientific thought. One may recognize the fact that the structure of a scientific theory reproduces the categories of the culture of the dominant classes. Even in its complexity, the culture of the middle classes (bourgeoisie) is an expression of the thought and the system of value just of the dominant middle classes.

The contradictory aspects of this culture find an explanation in the context of the dialectic of class struggle in the widest sense. Modern theoretical physics may be a specific example just for the deductive nature of research, the very structure of the theory (the axiomatic setting of Quantum Mechanics), the functioning of this setting in order to generate a phenomenological research. This subatomic phenomenology is connected, for instance, with the introduction of complex technologies of economic relevance, with the hierarchic pyramidal structure existing in the research world, with the subjection to chiefs.

This setting is moreover in harmony with the pedagogical attitude of the middle classes culture, which has mainly the purpose of settling and conserving instead of criticizing and overthrowing. A typical consequence is not only the structure of research, but specifically the organization of the schools, its selective task, the standardization of the culture. All these are the instruments for the perpetuation of the social labour division. The selection is commonly considered as based on objective merit criteria, but its ideological nature is extremely clear to us.

The scientific researchers are automatically inserted in a mechanism mainly characterized by competition, individualism and the uncritical acceptance of the social structure. The last aspect is itself a specific consequence of the extreme competitiveness which completely absorbs the energies of the researchers and leaves an extreme poverty of cultural and political instruments. We have experienced this fact even in the discussions of the School of Physics "E. Fermi" on the Foundations of Quantum Mechanics: the poverty of the philosophical background pushes people toward an attempt to separate Physics from Philosophy with an evidently arbitrary and artificial procedure.

It is consequently necessary to investigate the social role of the scientist, with respects not only to his specific research activity but also with respect to his social position and his relationship to social stratification. At some point it also becomes necessary for us to take concrete political action with respect to these matters. In connection with the social and class position of the researcher one may observe that even intellectual work is becoming more and more dependent and more and more connected with the productive sectors, with the surplus value production mechanisms, and with accumulation and use of capital. In what concerns specifically research activity, one must consider the problem in an international context, taking into account the international labour division and the already mentioned penetration of American capital in foreign countries.

Leaving to a subsequent analysis applied industrial research, the most remarkable feature in the fundamental theoretical research is, as already said, the main development which has occurred in industrially related research areas (elementary particle accelerators, nuclear reactors).

This explains the reasons for the tremendous and exponentially increasing number of theoretical papers, having a very low cultural value but concretely linked to the construction of great machines.

A well known stabilizing mechanism of the capitalistic system consists in a non productive investments. These develop an enormous number of enterprises connected to the production of goods whose market never gets saturated or such that the State is the direct consumer. This is exemplified by military expenses and the great research programs, in spite of the official explanation in terms of challenge competition and social services.

Moreover the mechanism we are mentioning have a real international dimension. As we already said, the country which is able to produce the most significant and numerous technological innovations with the lowest relative costs due to the possibility of great investment, gains the largest profits and imposes the development of certain scientific branches instead of others.

Another aspect is also the drain of the most technically qualified labor force and of the scientist “brains”. It is obviously worth noticing that the mechanism we have discussed is very complicated, is not characterized by a strict determinism but has instead a dialectic nature; research activity develops in the branches which allow investments of large capital, and the latter in turn allows the development of specific research.

The last aspect we want to examine is the direct connection between research and industry and the bearing of technical innovation in the capitalistic system. Technological innovation leads to the automation of the productive process. This fact increases the profits only initially because the enlargement of automation increases competition, reduces the engagement of the labor-force and causes a gradual fall of the profit rate. Technological innovation has then the task of creating new products on the basis of artificial and unnecessary needs (for example appliances more and more sophisticated cars, dresses).

The introduction of new products prevents the saturation of the market and creates new productive processes and engagement of workers. The creation of unnecessary needs plays moreover a role of social evasion and consensus maintenance: the enlargement of the consums simulates an apparent social mobility, while it absorbs the social strains and the push of the working class. In conclusion, trough unnecessary consumptions the workers lose the advantages which they have gained in the union’s struggles.

On the other hand, socialist countries, bound to the Soviet Union, which have not had cultural revolutions have reintroduced capitalist mechanism of accumulation and appropriation of surplus value. From this point of view, in spite of the roughness of informations and also taking into account the objective differences, the Chinese attempt gives an alternative model of production organization. In all cases, for this direction of research, we need an historical investigation about the interaction between the science and the social structure.

PRE-DECISION ON THE SOCIAL ROLE OF THE SCIENTIST

In this school we have tried to analyse the possible attitudes of scientist with respect to the object of his research, that is, his philosophical position concerning nature.

It seems to us that this analysis has been unsatisfactory.

We have studied in what measure the individual consciousness of the scientist is involved in the phenomena he actually investigates. But we have never attacked this problem in a larger context than the laboratory in which scientific experiments are performed.

We have considered to some extent the role of our consciousness in photon polarization experiments, but we have not considered our conscious involvement in phenomena such as the explosion of the atomic bomb.

The limitation of the individual consciousness to laboratory activity, disregarding any judgement of the social application of the research, results in indiscriminate support for all applications of science (e.g., atomic bomb, chemical and bacteriological warfare).

The scientist's incapacity to control the product of his research facilitates its cultural manipulation and the creation of consensus.

This mystification is formalized in the powerful theory ("scientism") which assumes the intrinsic ability of science to solve all the human problems.

It is not an accident that the ruling class uses more and more the prestige coming from scientific achievements.

Whatever the role of consciousness in the measuring process, it seems to us necessary to be aware that no human activity is neutral in the social context and that this is true also for science. As a consequence the scientist should assume all the responsibilities of his behaviour.

We have also discussed the objectivity of phenomena. It has been said, for instance, that a clock is "objective" because everybody reads the same numbers on it. This attitude, which we do not want to question here in respect to science, may be dangerous. For in this way we may overlook the fact that our agreement concerning clock readings is based not only on our present concept of objectivity but also depends on a common historical inheritance from human activity in the past.

If men had chosen to live aesthetically instead of productively, could we use the same idea of objectivity?

Now we conclude that a pre-decision is strongly needed concerning the social structure in which men live and act. That is, a pre-decision on the historical and social role of the scientist, on his responsibility, on the fact that no concept or activity is neutral.

Thus the primary datum to start with is the material condition of men. Ideological mystification may be avoided only if we choose to attribute objective existence and reality to the material condition of men; we are no longer interested in the question of whether this objectivity is real or apparent. If human suffering is the ultimate measure of our activity and man is the unit of measure, then we must discuss the foundation of scientific theories being aware of the well defined limits of this task. Whether a theory can or cannot make predictions, we stress that our activity as scientists is not at all neutral and that our fundamental task as men is to act upon the social reality and to change it.

ON THE CONNECTION BETWEEN SCIENCE AND THE PRODUCTIVE SYSTEM

In thinking about the development of science during the last four centuries one can recognize a deep connection between it and the production apparatus. This statement must be understood in the following sense: the rise of the bourgeoisie as economical class, in the last four centuries or more, has produced social and political changes as well as changes in culture, ideologies, techniques, way of life and so on. Modern science is one of the products of the social and cultural revolution connected with the rise of the European capitalist.

The essential difference between modern science and the ancient one is the enormous yield in practical informations on the physical world. This huge amount of powerful knowledge has made possible the industrial society in which we live. As few examples, we may remember here how the study of thermodynamics was promoted by the development of the steam engine or more recently, how solid state physics optoelectronic and cybernetics are strongly forced to develop by the industry apparatus because of their practical applications.

In this sense we can speak of a connection of the science with the industrial apparatus and from this point of view it is easy to understand how the organization and cultural content of the scientific research reproduces the organization and culture of the leading social class.

We think that the famous problem of the two cultures finds its explanation in these considerations. In part we think that it is more correct to talk about speak of a splitting of the culture in many separate streams.

We perceive an always deeper division of the scientific work in specialistic fields and real difficulties to put in discussion the foundations and purpose of science. Actually in the scientific community it is very difficult to find the right atmosphere to discuss foundations and purpose of science. This is shown by the relatively very poor scientific production in such field. Obviously we consider this school an exception.

We see a huge production of papers in each specialistic sector of the science.

This production deals with the subject in fashion and mostly fashions change quite quickly in time. So we perceive not only a deep division of the scientific culture from the body of the humanistic culture but also we find our scientific culture deeply divided and fragmented in itself. As a matter of fact such division of culture in separate parts avoids the formation of a really strong critical movement able to reach a new cultural synthesis.

The possibility for scientists to change the situation are very few. In principle scientists have complete freedom to decide the kind of research they want to do; in practice they are inserted in a scientific organization which reproduces the structure of the society to which they belong. In every country the political establishment puts funds for researches under the control of few trusted people. Usually they are people of acknowledged scientific authority but they are part in charge of the scientific management because they guarantee a development of research not in contrast with the interests of the establishment.

As an example of the decisional power of these people we may quote the fact that careers of researchers depend in a substantial way by the scientific opinions held by the people directing the scientific community.

TECHNOLOGICAL AND SCIENTIFIC DOMAIN OF THE USA: A NEW COLONIALISM

During and after WWII a great technological jump has taken place in the more developed industrial countries. This jump may be recognized by the very great increase of their expenditures for research and development.

The prewar dominance of extractive industries has been replaced by the postwar dominance of industries of a transformative and manipulative nature. This may be seen by examining specific data, but it is also apparent in the sudden growth of the most technologically advanced industrial fields such as electronics, which are now the most important.

The connection between these facts and the military effort might be related to the fact that the Wall Street Journal, which is representative of industries based on traditional technologies, is against the war in Vietnam, which represents, on the contrary, the interests of the most technologically advanced corporations.

Another relevant aspect of the above mentioned industrial transformations is the fact that the most advanced technology has been developed mainly in the USA, while the main part of European industry rests on a traditional technology. We may observe that the Marshall financial aid of the USA to Europe has facilitated this phenomenon. The consequent technological gap between USA and European countries made the latter completely dependent on USA for the most advanced technological procedures.

The official financial aid of USA to Europe was moreover accompanied by a great increase (a factor ten in absolute value and from 15% to 25% of the total investment in the world) of private American investments. This clearly shows how enormously important interests are.

As a further remark, one may recall that an analysis of the hundred largest corporations of the world shows the enormous gap between USA and Europe mainly in the most advanced technological branches. Only in traditional branches (mechanics, cars, appliances) has been partially avoided. But the situation becomes even worse if one takes into account the multinational participation in many great corporations and the concentration of them to create monopolies in many sectors; consider for instance the recent fusion of General Electric and Honeywell, leaving to IBM the monopoly in constructing computers.

We can at first obtain from these considerations that Europe has become an enormous market for the new american products and technologies, so well justifying the military intervention of USA in Europe, which was justified, at the time, as a defence of human rights, an argument common to all the military activities of USA.

The almost complete technological dependence of Europe on the USA is consequently even greater in the most advanced fields. In some of them, like computers or nuclear reactors, Europe lacks technicians who can design and construct finished products. We can only assemble the different parts which are under American patents. Europe constitutes then, more than rest of the world, an enormous market for the American patents in advanced technologies.

It is possible to make specific examples pertaining specifically to scientific research. In Florence there is an important research center of CNR on electromagnetic waves. 95% of the direct research expenditures goes directly to the USA.

In this economic pattern, undetailed but nevertheless accurate, we think that it is possible to speak about the new colonialism of the USA in Europe and in the European culture countries (for example in the Latin American ones).

Everyone understands what is meant by old colonialism: consider, for instance, the British Empire in the last years of XIX century. Its main feature was the extraction of raw materials such as iron minerals, rubber, gold and other precious materials in the Dominions using underpaid native workers (Indian, Arabian and so on).

On the contrary USA presently make very great investments in Europe, but at the same time they gain a big market for American manufacturers; moreover Americans imposed in Europe a mechanism which prevents the saturation of the market acting through the continuous creation of fictitious needs. All this is allowed by the existing technological gap which was born as we roughly have described and this maintained by the financial dominance and by the expropriation of the European intellectual forces. The new colonialism facilitates in facts the use of a highly qualified work force at low costs.

It is well known that the more developed countries of Europe and South America (Uk, Italy, Argentine, for example) produce so a large number of highly qualified technicians that is impossible for them to employ. It is so possible for the USA to import the best of these technicians. This is an enormous social cost the European culture nations must pay in order that American technology can maintain its supremacy in the world. Some people have tried to estimate this social cost: they found that this cost evaluated in USA dollars is comparable to USA investments in Europe.

As a demonstration of this we may recall that the main scientific theories originated in Europe until WWII, while at present the scientific leadership belongs to USA. These new areas of research (such as elementary particles, with the consequent accelerator technology, solid state physics, etc) have been chosen by the USA and then imposed to Europe. We know that the Nazis forced many scientists to emigrate to USA. US scientific supremacy is in large measure a consequence of this historical happenstance. Thus, once more we see that the intervention of the USA in WWII has proved to be a good investment.

SECTION 2.6.3-PP. 329-330: THE ORIGINAL ITALIAN TEXT BY THE LETTER OF G. MATTIOLI, WINNER OF THE STANGHELLINI PRIZE AT SIF IN 1971

Credo, anche nella mia vita scientifica, che la scuola di Varenna è stata un polo singolare molto importante: sia perché, partendo dalla scuola di Varenna, io ho potuto fare un programma di studio che [...] mi ha fruttato il premio Stanghellini, ma soprattutto per quello che ha significato, nella vita scientifica mia e di molti colleghi, questa scuola. [...] [nella relazione per il concorso] bisognava dire in che modo si aveva in programma di utilizzare questa borsa di studio, e allora io dicevo gli argomenti di fisica di cui finora mi sono interessato (che sono i poli di Regge) e poi dicevo che non era possibile essere andati alla scuola di Varenna senza dire che mi appariva sicuramente impossibile riprendere ad occuparmi di quelle cose. Cioè dopo aver scoperto che uno ha vissuto per 10 anni lavorando sulla Meccanica Quantistica e ignorando assolutamente che, a monte, esistono contraddizioni e paradossi tali, per cui, non dico i poli di Regge, ma addirittura la matrice S o cose

ancora più importanti - la teoria dei campi - sono delle cose che hanno dei fondamenti quanto meno assai discutibili e assai problematici.

[...] La mia personale convinzione, avendo in mente gli “strumenti critici” che questa Scuola della Sif mi ha dato [...] è che se guardiamo con occhio obiettivo alla Fisica che stiamo facendo, credo che poche persone possano dire con serenità che si sentono di avere tutte le carte in regola dal punto di vista culturale per motivare le ricerche che stanno facendo. [...] e mi sembra che, alla scuola di Varenna, l'essenza venuta fuori dal dibattito era che, effettivamente, c'erano motivi perché un dibattito culturale di proporzioni generali sulla Meccanica Quantistica, che era vivissimo intorno al 1930, improvvisamente con la scuola di Copenhagen sparì. Sparì perché, in fondo, questa Fisica bisogna poi mandarla avanti, è un grande meccanismo che deve sfornare pubblicazioni, deve sfornare una grande mole di fisica per dare supporto e ricevere supporto da e a una grande mole di fisica sperimentale; è una grande mole di fisica sperimentale che si fa a livello di acceleratori, a energie sempre più elevate [...] quindi [è] un affare di proporzioni industriali sempre più colossali che viene selezionato e governato, fatalmente al di là delle nostre intenzioni, dal paese che ha possibilità di investirvi maggiore capitale. La scuola di Varenna mi pare che mettesse in luce anche un altro fatto. Cioè, di fronte ai problemi, al nocciolo dei problemi, che erano problemi culturali e investivano anche strumenti filosofici, veniva fuori in un modo palese che tutti noi - dall'ultimo studente al primo speaker - sul piano degli strumenti filosofici eravamo veramente gracilissimi, al punto che l'operazione che si conduceva quasi quotidianamente era dire: bene, siccome questi strumenti filosofici il fisico medio non li ha, allora facciamo un'operazione molto semplice: diciamo che fra Fisica e Filosofia non c'è nessun rapporto, la Filosofia la tagliamo perché non la conosciamo. E poi emergeva anche un altro dato di fatto: al di là della malizia politica della gente, appena i problemi investivano anche degli aspetti politici (questo rapporto tra scienza e società, questo chiedersi perché in fondo su questi problemi della Meccanica Quantistica il dibattito era rimasto al livello del 1930, quali interessi fossero in gioco), veniva fuori un'immagine del fisico che, anche sul piano politico è estremamente sprovveduto, crede ancora alla neutralità della scienza, al mito del progresso e così via. Mi sembra che non dovrebbe essere affossata questa esigenza, che se non sbaglio è esplosa l'anno scorso al congresso SIF. Sarebbe veramente una grossa miopia, un'occasione persa vederla affossare, il chiedersi finalmente: 1) qual è la validità, ma in un senso globale e culturale finalmente, della ricerca che si fa; 2) se questa validità viene messa in discussione (perché io mi permetto di dire che la fisica teorica che faccio io e moltissimi miei colleghi, è una fisica assolutamente senza valore: consiste soltanto nel raddoppiare il numero dei parametri su dei modellini più o meno straccioneschi), bene, chiediamoci allora finalmente perché siamo costretti a fare questa ricerca che non ha nessun valore culturale; e terza cosa, prendiamo anche delle decisioni operative in tal senso. [...] Non so, mi viene spontaneo di dire, ai lavoratori che la pagano poi questa università e questa ricerca [...] che, dal punto di vista dei problemi che questo paese ha, problemi effettivi [...], noi abbiamo molto poco da dire e facciamo una ricerca che vale molto poco nel senso dei problemi essenziali, per esempio dei problemi dei lavoratori.

(MINUTES OF THE 165TH STEERING SIF COMMITTEE, January 1971, pagg. 9-11 IN SIF bulletin n. 83 del 30/06/1971)

SECTION 2.7.3-PP. 360-361: THE ORIGINAL ITALIAN TEXT BY LAURORA ON BELL

La sua attenzione [di Bell] si focalizzò sull'argomento che più lo interessava: la non-località della teoria di Bohm. Da sostenitore della posizione di Einstein, egli decise di riprendere l'esperimento EPR nella versione di Bohm ritenendo di poter contribuire al progresso nella ricerca sui fondamenti della Meccanica Quantistica se fosse riuscito a stabilire in modo rigoroso una teoria locale a variabili nascoste che riproducesse le correlazioni quantistiche.

Nel suo ragionamento Bell riprende la versione di Bohm dell'argomento EPR. [...] Tuttavia esamina un esperimento più generale di quello considerato nell'argomento EPR e anche in Bohm: analizzando le misure delle componenti di spin lungo direzioni arbitrarie (e non solo le componenti lungo l'asse z o l'asse x , come era stato fatto fino a quel momento).

[...] Poi Bell ebbe l'idea di ruotare i magneti [degli Stern-Gerlach] in modo che i campi magnetici formassero un certo angolo tra essi (o anche di mantenere costante la direzione di misura da un lato dell'apparato e variare la direzione dall'altra parte). Era un'idea abbastanza semplice, ma fino ad allora nessuno ci aveva pensato. Egli assume che "se le due misure sono eseguite in luoghi distanti l'uno dall'altro, l'orientamento di un magnete non influenza il risultato ottenuto con l'altro", ammettendo quindi l'ipotesi di località. Poi osserva che, poiché è possibile prevedere il risultato di una qualsiasi componente dello spin della particella 2 misurando la stessa componente dello spin della particella 1, possiamo concludere che il risultato di una tale misura è predeterminato e che "questa predeterminazione implica la possibilità di una specificazione più completa dello stato". Questa [è] l'esistenza di variabili nascoste, è ipotizzata e inoltre realizzata per mezzo di parametri λ , che possono indicare una singola variabile oppure un insieme di variabili, essere di tipo discreto o continuo. [...] Giunge poi alla sua disuguaglianza: $1 + P(b,c) \geq |P(a,b) - P(a,c)|$.

[...] Mostra quindi come il valore di aspettazione quantistico non soddisfi tale disuguaglianza, derivata dall'ipotesi di realismo locale, dimostrando così il risultato: l'incompatibilità di una teoria a variabili nascoste con le previsioni della QM.

[...] A conclusione del suo lavoro, Bell sottolinea la possibilità che la Meccanica Quantistica sia di validità limitata, le cui previsioni possono essere applicate "solo a quegli esperimenti in cui le preparazioni degli strumenti siano fatte sufficientemente in anticipo, in modo da consentire loro di raggiungere un qualche reciproco rapporto per mezzo di uno scambio di segnali aventi velocità minore o uguale a quella della luce. In questo senso sono fondamentali gli esperimenti del tipo proposto da Bohm e Aharonov (1957), in cui le preparazioni degli apparati sperimentali sono modificate durante il tempo di volo delle particelle".

[...] Pertanto già nel 1964 Bell suggerisce l'opportunità di un esperimento che sarà poi effettivamente realizzato da Aspect, Dalibard e Roger nel 1982 (Laurora 2014, pagg. 42-46).

SECTION 2.7.4-P.369: THE ORIGINAL ITALIAN TEXT BY SELLERI DESCRIPTION OF FREEDMAN and CLAUSER experiment, April 1972

In questo esperimento la radiazione di una lampada ad arco investiva un fascio di atomi di calcio eccitandone parte allo stato $3d4p^1P_1$. Circa il 10% di questi passava al livello inferiore $4p^2\ ^1S_0$ che è lo stato iniziale della cascata (0,1,0) che genera fotoni di 551.3 nm e 422.7 nm.

Su ciascun lato della sorgente i fotoni sono raccolti e collimati da una lente, poi incidono su un filtro e su un polarizzatore (a una via) e infine eventualmente raggiungono un fotomoltiplicatore. Essi usarono polarizzatori lunghi circa 1 m e consistenti di dieci lastre di vetro parallele inclinate all'angolo di Brewster. Gli impulsi dei fotomoltiplicatori erano inviati a un circuito di coincidenza e misure di coincidenza venivano compiute durante intervalli temporali di 100 s, alternando gli intervalli nei quali le lastre erano state rimosse a intervalli in cui erano presenti. I risultati, ottenuti variando le direzioni relative degli assi ottici dei polarizzatori, furono trovati in buon accordo con le predizioni quanto-meccaniche, e in chiara violazione della diseguaglianza forte (Selleri Fisica 900, 1999, pagg. 208-209; Selleri Paradossi, 2001, pag. 155).

SECTION 2.7.4-P.370: THE ORIGINAL ITALIAN TEXT BY SELLERI DESCRIPTION OF HOLT and PIPKIN experiment, 1973

In questo esperimento furono osservati i fotoni con lunghezza d'onda 567.6 nm e 404.7 nm emessi nella cascata (1,1,0) dell'isotopo ^{198}Hg del mercurio con spin nucleare zero. Poiché il livello finale della cascata usata non è lo stato fondamentale dell'atomo, non furono necessarie precauzioni per evitare l'intrappolamento risonante dei fotoni, effetto che invece era stato osservato nell'esperimento di Freedman e Clauser e segnalato nella tesi di PhD di Freedman. Per produrre le coppie di fotoni, vapori di mercurio furono eccitati al livello 9^1P_1 da un fascio di elettroni da 100 eV. Furono usati polarizzatori di calcite. Questi polarizzatori sono nettamente migliori di quelli a lastre parallele [...]. Il risultato sperimentale non violò [la diseguaglianza forte] e fu in disaccordo con la predizione quantomeccanica. Questa discrepanza non è mai stata spiegata, anzi c'è stato chi la ha attribuita all'uso dei (buoni) polarizzatori di calcite (Selleri Fisica 900, 1999, pagg. 209-210).

SECTION 2.7.4-P.373: THE ORIGINAL ITALIAN TEXT BY SELLERI DESCRIPTION OF CLAUSER experiments, June 1976

Clauser cercò di ripetere l'esperimento di Holt e Pipkin usando la stessa cascata anche se nell'isotopo ^{202}Hg del mercurio. Inoltre, invece dei polarizzatori di calcite, usò polarizzatori a lastre parallele. Questo esperimento [violò] la diseguaglianza forte e [fu] in ottimo accordo con la predizione quantomeccanica. In un'estensione dello stesso esperimento, Clauser misurò le correlazioni di polarizzazione inserendo lamine a quarto d'onda fra ciascun polarizzatore e la sorgente. Le lamine a quarto d'onda erano ottenute applicando una compressione a lastre di quarzo di tipo commerciale (Selleri Paradossi, 2001, pag. 156).

SECTION 2.7.4-PP.373-374: THE ORIGINAL ITALIAN TEXT BY SELLERI DESCRIPTION OF FRY and THOMPSON experiment, August 1976

Questi autori usarono i fotoni con lunghezza d'onda 435.8 nm e 253.7 nm emesso nella cascata (1.1.0) dell'isotopo ^{200}Hg del mercurio con spin nucleare zero. Lo stato 7^3S_1 di un fascio di mercurio fu popolato con un processo a due passi: (i) eccitazione dello stato metastabile 6^3P_2 mediante bombardamento elettronico; (ii) dopo che tutti gli stati a breve vita media erano decaduti, assorbimento risonante di radiazione da un laser regolabile.

La larghezza di banda del laser era abbastanza stretta da eccitare selettivamente l'isotopo ^{200}Hg del mercurio. I polarizzatori usati erano della varietà a lastre parallele e il campo magnetico nel volume d'interazione era artificialmente ridotto a meno di 5 mG. Poiché lo stato iniziale della cascata aveva momento angolare $J=1$, era necessario tener conto della possibilità di popolazioni diverse e di coerenza relativa fra i sottolivelli Zeeman iniziali, cosa che Fry and Thompson fecero misurando la polarizzazione della radiazione da 435.8 nm. [...] L'esperimento [fu] in ottimo accordo con la teoria e in violazione della disuguaglianza forte (Selleri Fisica 900, 1999, pagg. 210-211).

SECTION 2.7.4-P. 375: THE ORIGINAL ITALIAN TEXT BY LAURORA

Quando nel 1974 Alain Aspect legge l'articolo di Bell sul paradosso EPR, focalizza la sua attenzione sull'indagine sperimentale del teorema in particolare sul 'loophole' relativo alla località. Negli esperimenti di prima generazione gli assi dei polarizzatori erano mantenuti fissi durante le misure, per cui poteva esserci la possibilità di uno scambio di informazioni tra i polarizzatori con una velocità inferiore a quella della luce. Per eliminare tale possibilità, è quindi necessario durante l'esperimento modificare la configurazione dei polarizzatori in un tempo minore di quello di volo dei fotoni dalla sorgente ai polarizzatori (Laurora 2014, pag. 108).

SECTION 2.7.4-PP. 375-376: THE ORIGINAL ITALIAN TEXT BY SELLERI DESCRIPTION OF ASPECT, GRANGIER, ROGER

Questi autori usarono i fotoni da 551.3 nm e 422.7 nm dalla cascata (0.1.0) del calcio. In questo caso gli atomi di calcio furono eccitati al livello $4p^2\ ^1S_0$ da un processo non risonante di assorbimento di due fotoni usando un primo fascio di luce laser di lunghezza d'onda 406 nm ed un secondo fascio da 581 nm, entrambi i fasci laser essendo perpendicolari al fascio atomico di calcio emesso da un forno di tantalio. I fasci laser furono localizzati nella regione di interazione per ottenere una sorgente grosso modo cilindrica di circa 60 micron di diametro e 1 mm di lunghezza [con un'alta efficienza]. Durante l'esperimento la densità atomica in questa "sorgente" variava da $3 \cdot 10^{10} \text{ cm}^{-3}$ a 10^{10} cm^{-3} , cosa che generava un flusso di cascate non minore di $4 \cdot 10^7 \text{ s}^{-1}$. I fotoni della cascata erano analizzati con polarizzatori a una via a lastre parallele e da filtri allo stesso modo che negli esperimenti precedenti. L'alta densità atomica della sorgente generava fino a 100 eventi di coincidenza al secondo, il che permetteva una precisione statistica dell'1% in soli 100 s di raccolta dati. Questi autori ottennero [un] risultato [...] in accordo con la meccanica quantistica e in violazione della disuguaglianza forte per più di 13 deviazioni standard (Selleri Paradossi, 2001, pag. 156).

SECTION 2.7.4-P. 376: THE ORIGINAL ITALIAN TEXT BY SELLERI DESCRIPTION OF ASPECT, GRANGIER, ROGER

Nel 1982 questi autori realizzarono un esperimento, originariamente suggerito e studiato da Garuccio e Rapisarda, basato su polarizzatori a due vie. Ciascuno era un cubo polarizzatore, costruito usando le proprietà di una sottile lamina dielettrica e rivestito contro la riflessione, e che poteva essere ruotato attorno all'asse di osservazione.

L'apparato permetteva di misurare una grandezza proporzionale alla funzione di correlazione usando una tecnica di coincidenza quadrupla per ciascuna delle [...] quattro orientazioni relative dei polarizzatori [...]. Gli autori trovarono sperimentalmente [un risultato] in violazione del limite [...] di Bell ma in accordo con la predizione quantomeccanica (Selleri Paradossi, 2001, pagg. 156-157).

SECTION 2.7.4-P. 377: THE ORIGINAL ITALIAN TEXT BY SELLERI DESCRIPTION OF ASPECT, DALIBARD, ROGER

In tutti gli esperimenti descritti finora gli assi ottici dei polarizzatori erano mantenuti fissi durante ogni gruppo di misure. Quindi in linea di principio c'era la possibilità di uno scambio di informazioni tra i polarizzatori con una velocità inferiore a quella della luce.

Questa possibilità, anche se molto improbabile, dato che non si vede proprio quale meccanismo possa generare un simile scambio, potrebbe comunque essere eliminata se la configurazione dei polarizzatori venisse modificata [durante l'esperimento] in un tempo più breve di quello di volo della luce dalla sorgente ai polarizzatori. Nell'esperimento realizzato da Aspect, Dalibard e Roger un commutatore ottico ridirigeva rapidamente i fotoni provenienti dalla sorgente verso uno o l'altro dei due [polarizzatori]. Diversamente dall'esperimento precedente, erano usati solo i canali di trasmissione dei cubi polarizzatori. La deviazione della luce era ottenuta con una riflessione Bragg da parte di un'onda ultrasonica stazionaria in acqua. La luce era completamente trasmessa quando l'ampiezza dell'onda stazionaria era zero, mentre veniva deviata di 10 mrad quando l'ampiezza era massima. Ogni 10 ns avveniva la commutazione da un canale all'altro, e poiché questo tempo, così come la vita media di 5 ns del livello intermedio della cascata, era inferiore a L/c (40 ns), dove $L=12$ m era la separazione fra i due commutatori e c la velocità della luce, nessun segnale poteva collegare un evento di rivelazione da un lato e il corrispondente cambiamento di orientazione dall'altro. In questo esperimento si rivelavano solo poche coincidenze al secondo, con un fondo accidentale di circa 1 coincidenza al secondo. Gli autori trovarono sperimentalmente una chiara violazione della disuguaglianza forte e un accordo con la predizione quantomeccanica. I commutatori sui due lati avevano un funzionamento periodico, anziché casuale, ma erano guidati da due generatori diversi aventi una diversa frequenza e gli autori, perciò, credevano che funzionassero in modo indipendente. È stato però fatto notare da [Anton] Zeilinger che la situazione può invece nascondere una difficoltà concettuale che rende l'esperimento non conclusivo. Infine, bisogna ricordare che la critica più importante agli esperimenti di Orsay era basata sulla molto probabile presenza di un intrappolamento risonante della radiazione nella sorgente dovuto alla grande densità atomica del fascio di calcio. Una risposta poco convincente a queste critiche è stata data da Aspect e Grangier (Selleri Paradossi, 2001, pag. 157).

SECTION 2.7.4-PP. 381-382: THE ORIGINAL ITALIAN TEXT BY SELLERI ON PERRIE, DUNCAN, BEYER, AND KLEINPOPPEN

Questi autori misurarono le correlazioni di polarizzazione tra due fotoni prodotti *simultaneamente* (cioè non in una cascata atomica) dallo stato metastabile del deuterio atomico $2S_{1/2}$ in un vero decadimento in due fotoni.

L'emissione di un fotone singolo da questo stato del deuterio è proibita e il canale principale per la diseccitazione è l'emissione simultanea di due fotoni, che possono avere lunghezza d'onda diversa da evento a evento, purché consistente con la conservazione dell'energia per la coppia (somme delle frequenze costante). Tuttavia, la finestra di osservazione era in pratica limitata fra 185 e 355 nm a causa dell'assorbimento della radiazione nell'ossigeno atmosferico. In questo esperimento veniva prodotto e usato un fascio di deuterio atomico di densità 10^4 cm^{-3} nello stato metastabile. I due fotoni derivanti dal decadimento dello stato $2S_{1/2}$ erano collimati da due lenti. I polarizzatori erano a lastre parallele. Misurando le probabilità di coincidenza, gli autori ottennero [un risultato] in accordo con la predizione teorica [della meccanica quantistica] ma in violazione della disuguaglianza forte per poco più di due deviazioni standard. La precisione statistica di questo esperimento è dunque inferiore a quella [dei precedenti] esperimenti con le cascate atomiche.

In un'estensione di questo esperimento fu misurata la correlazione fra le polarizzazioni circolari piazzando lamine a quarto d'onda acromatiche sul percorso di ciascun fotone, fra la sorgente e il polarizzatore lineare. I risultati ottenuti non violavano la disuguaglianza e l'accordo con le predizioni quantomeccaniche veniva ristabilito solo postulando un modo molto scadente di funzionamento delle lamine. Resta tuttavia molto interessante che le sole due misure di polarizzazione circolare in esperimenti tipo EPR [...] abbiano indicato che la disuguaglianza di Bell (forte) non è violata ed abbiano richiesto acrobazie intellettuali per cercare, in qualche modo, di interpretarne i risultati in modo consistente con la meccanica quantistica (Selleri Paradossi, 2001, pagg. 157-158. *Italics in the original text. My underline*).

SECTION 2.7.5-PP. 387-388: THE ORIGINAL ITALIAN TEXT BY SELLERI

Oggi [grazie alle ricerche, tra gli altri, di Popper, Lakatos e Kuhn] si è compreso che le teorie della fisica sono spesso assai buone ma mai perfette. E che una teoria veramente moderna dovrebbe essere una struttura aperta, perfezionabile, priva di postulati dal sapore dogmatico e irreversibile. Se tutti i problemi della MQ fossero riducibili ai paradossi della completezza, la strada che porta a una fisica migliore sarebbe spalancata davanti a noi [si avrebbe infatti un'alternativa certa e corretta e la MQ scomparirebbe]. Purtroppo esiste il paradosso di Einstein, Podolsky e Rosen, che è un problema molto più importante, perché si basa sull'incompatibilità fra le predizioni sperimentali della meccanica quantistica e quelle del naturalissimo punto di vista del realismo locale.

[...] la svolta nel paradosso di EPR ebbe luogo nel 1965, quando John Bell mostrò che il realismo locale, pur non essendo una vera teoria scientifica ma solo un punto di vista molto generale sui fatti della natura, conteneva predizioni empiriche. Queste predizioni non erano del tipo $G=g$ (cioè: misurate la grandezza fisica G e troverete necessariamente il valore numerico g) come quelle della meccanica quantistica, ma invece del tipo meno preciso $g_1 \leq G \leq g_2$ (cioè: misurate la grandezza fisica G e troverete sicuramente un valore numerico compreso fra g_1 e g_2). Il risultato di Bell è molto importante perché la ben definita predizione $G=g_{mq}$ casca fuori dall'intervallo permesso dal realismo locale (perché $g_{mq} > g_2$). [Quindi] è impossibile interpretare la meccanica quantistica dal punto di vista del realismo locale, il che costituisce una grande lezione di epistemologia: una teoria scientifica può essere collegata a scelte filosofiche in un modo tanto profondo da essere incompatibile con altre

scelte filosofiche nelle sue stesse predizioni empiriche. Insomma, si può dire che possiamo veramente fare della filosofia a livello sperimentale!

[...] Il fatto nuovo, emerso nel 1965 con la scoperta della diseguaglianza di Bell, è che questo realismo non è metafisico, data la grande ricchezza di predizioni empiriche falsificabili che ha generato. La diseguaglianza di Bell non è che una di queste predizioni, perché sono state scoperte letteralmente infinite altre conseguenze del realismo locale che non sono in alcun modo deducibili dalla diseguaglianza di Bell (Selleri Paradossi, 2001, pagg. 131-132. My underline).

SECTION 2.7.6-P. 390: THE ORIGINAL ITALIAN TEXT BY SELLERI

[...] è utile chiarire bene una importante ambiguità relativa alla diseguaglianza di Bell che rende inaccettabile l'idea, purtroppo assai diffusa, che in certi esperimenti il realismo locale sia già stato falsificato.

[...] Mentre le diseguaglianze deboli sono una conseguenza del realismo locale puro e semplice, quelle forti sono dedotte usando delle ipotesi aggizionali che stabiliscono un confronto tra diversi possibili destini di uno stesso fotone e non sono, quindi, suscettibili di verifica sperimentale. Esse sembrano valide all'interno della teoria quantistica tradizionale, ma limitano fortemente il novero delle possibili teorie basate sul realismo locale. Se si vuole dar fiducia a priori alla teoria esistente, non ha alcun senso lo studio sperimentale del paradosso di EPR, il problema essendo risolto a priori per postulato. Altrimenti, non ha senso imbarcarsi in una ricerca facendo ipotesi che favoriscono fortemente una delle alternative logicamente disponibili. [...] Le predizioni quantomeccaniche relative a tutti gli esperimenti compiuti violano le diseguaglianze forti [che restringono i valori permessi ad un intervallo che, nel caso più favorevole, è il 4% di quello permesso dalle diseguaglianze deboli], ma sono abbondantemente interne all'intervallo [previsto dalle diseguaglianze deboli]. Pertanto, sia le predizioni della [stessa] teoria quantistica sia i risultati degli esperimenti soddisfano le diseguaglianze deboli, e non possono essere dichiarati incompatibili con il realismo locale. È una situazione ben nota, che molti però preferiscono ignorare (Selleri Paradossi, 2001, pagg. 152-153. My underline).

SECTION 2.7.6-P. 392: THE ORIGINAL ITALIAN TEXT BY SELLERI

La consapevolezza del ruolo decisivo delle ipotesi aggizionali nelle ricerche sperimentali degli ultimi trent'anni sul paradosso di EPR non è molto diffusa. Molti autori confondono diseguaglianze forti e deboli. Altri producono argomenti tesi a dimostrare la validità delle ipotesi aggizionali. Ad esempio, Ghirardi critica l'idea che le correlazioni osservate possano in parte essere dovute all'azione dei polarizzatori e dei rivelatori, dicendo: "Mi sembra superfluo sottolineare che tutto ciò che sappiamo sul funzionamento dei rivelatori contraddice questa assunzione". Ci vuole un po' di presunzione a scrivere questo, [perpetrando ancora oggi il fatto che] dal 1927 la fisica ortodossa ha rinunciato a capire le cose del mondo per affidarsi solo al formalismo matematico. Ovviamente anche i realisti sanno poco, ma almeno cercano di avanzare delle congetture! Subito dopo Ghirardi aggiunge: "Ma una incredibile cospirazione di eventi e il verificarsi di processi che non trovano alcuna spiegazione o ragione d'essere in nessuna delle teorie note, farebbe sì che i fotoni e/o i

contatori si accordassero per farci credere di vivere in un mondo quantistico anziché in uno classico”. Queste sono parole forti, che servono a coprire la debolezza delle obiezioni. Ovviamente, se ci si affida alle “teorie note” si fa poi poca fatica a escludere una proposta innovativa! La consapevolezza della vera situazione logica è più diffusa tra i fisici sperimentali. L’ultimo gruppo sperimentale che si è confrontato con il realismo locale ha affermato: “Tutti gli esperimenti finora hanno avuto efficienze di rivelazione abbastanza basse da permettere la possibilità che il sottoinsieme degli eventi rivelati sia in accordo con la meccanica quantistica anche se l’insieme intero soddisfa le disuguaglianze di Bell” (Selleri Paradossi, 2001, pp. 154-155).

SECTION 2.7.6-PP. 392-393: THE ORIGINAL ITALIAN TEXT BY SELLERI

Un buon esempio di come il confronto sperimentale fra l’esistente teoria quantistica e il realismo locale NON dovrebbe essere fatto è fornito da un articolo del gruppo di Rochester [...]. Nel sommario vi si afferma: “Descriviamo un esperimento di coincidenza di due fotoni che dimostra la violazione del realismo locale, come definito da Einstein, Podolsky e Rosen, per circa 45 deviazioni standard...”. Questo esperimento merita di essere discusso perché, se questa affermazione fosse corretta, il dibattito sul realismo locale troverebbe finalmente la sua conclusione. [Però] la ricerca del gruppo di Rochester appare subito in una luce diversa se si fa attenzione all’ipotesi fatta nella breve introduzione: “Dato che i rivelatori fotonici non hanno efficienza del 100% [...] è necessario assumere che gli assorbimenti dei fotoni sono completamente a caso. Con questa ipotesi (del “campione fedele”) [...] mostriamo che il realismo locale è violato”. In realtà l’ipotesi del campione fedele genera una mutilazione molto importante del realismo locale: questo è noto almeno dal 1983, quando quell’ipotesi fu proposta per la prima volta [nella tesi di Aspect]. Assumere che l’insieme delle coppie di fotoni rivelate fornisca una riproduzione fedele delle proprietà fisiche dell’insieme delle coppie emesse dagli atomi è, in sostanza, la stessa cosa che ammettere che quest’ultimo insieme sia omogeneo nel senso di von Neumann, per cui le probabilità osservate non possono avere alcuna struttura. A questo modo non solo risultano proibite le ordinarie variabili nascoste, ma neanche possono essere introdotte le ben più generali “propensioni”, che sono state dimostrate [Afriat, Selleri 1999] costituire la base più naturale per il realismo locale probabilistico. Va però ricordato che le assunzioni su cui si basa il teorema di von Neumann sono note essere inadeguate fin dal tempo in cui fu dimostrato [da Bohm e Bub nel 1966] che, usandole, si favorisce fortemente uno dei due schemi concettuali contendenti. Una prima risposta al gruppo di Rochester fu data da Garuccio, che fu in grado di dimostrare che il parametro cruciale η_0 (rapporto di somme di probabilità quantomeccaniche) deve assumere un valore non inferiore a 0.82 per poter avere un confronto significativo fra la meccanica quantistica e il realismo locale. Il valore reale per l’esperimento di Rochester era 0.10 e, pertanto, Garuccio concluse: “Ciò dimostra che l’esperimento non è in grado di distinguere fra meccanica quantistica e località di Einstein senza fare ipotesi supplementari”. Una seconda risposta fu data da Cabello e Santos, che costruirono un modello a variabili nascoste locali le cui predizioni erano in accordo numerico con le coincidenze fotoniche osservate in quell’esperimento.

Quei [valori] di coincidenze implicavano una violazione del realismo locale secondo i fisici di Rochester, mentre Cabello e Santos giustamente conclusero: “Affermiamo che l’esperimento refuta solo quella famiglia ristretta di teorie a variabili nascoste locali che contengono ipotesi addizionali (Selleri Paradosi, 2001, pagg. 158-159. My CAPS).

SECTION 2.7.7-PP. 406-407: THE ORIGINAL ITALIAN TEXT BY THE LETTER SENT BY SELLERI TO FRANCESCO BARONE

La scoperta di Bell sarà ricordata per secoli futuri, di questo sono certo, ed ho cercato di spiegarne il motivo su NCM. Io lo conoscevo bene, avevamo interagito molto negli anni '70. poi deve aver pensato che il mio entusiasmo per la sua disuguaglianza era eccessivo e una volta mi disse: “Leave me alone, I am an old man”. Non lo disturbai più. Lui viveva questa contraddizione, di aver fatto la più grande scoperta del realismo, pur essendo imbevuto di pragmatismo e di positivismo, e pur essendo costretto a subire queste ideologie al CERN ogni giorno. Un'altra volta mi disse che restava al CERN per sua moglie, che vi lavorava e lavora come sperimentatrice. Se no se ne sarebbe andato in una piccola università irlandese a lavorare esclusivamente sui FQM. Era una persona di intelligenza profondissima, con lui ero sempre dalla parte del torto. Si piantava a gambe larghe davanti alla lavagna, quasi per garantire solidamente un equilibrio che la concentrazione altrove avrebbe potuto mettere in crisi, e tirava fuori in pochi minuti risultati importanti. Cogli anni ho imparato che i britannici sono deboli in filosofia. Lui lo era e pur desiderandolo molto non riusciva a togliersi di dosso i condizionamenti positivistici pesanti che gli venivano dall’ambiente inglese prima, ginevrino poi. Il bisogno di realismo è però gridato in certi suoi articoli, ad esempio là dove ribattezza “beables” le “observables” della meccanica quantistica (Selleri to Francesco Barone, April 13th, 1991-FSPA. My underline).

SECTION 2.7.7-P. 407: THE ORIGINAL ITALIAN TEXT BY THE LETTER SENT BY SELLERI TO SILVIO BERGIA

[...] Io Bell lo conoscevo bene e lo frequentavo negli anni '70. poi mi sembrò che considerasse troppo eccitato il mio interesse per la sua disuguaglianza, e un giorno per me famoso mi disse: “Leave me alone, I am an old man”. Non lo disturbai più. Lui era combattuto tra tendenze positivistiche subite negli studi universitari ed al CERN, ed il bisogno di realismo, gridato quasi in certi lavori, come quando parla di “beables” anziché di “observables” . [...] mi permetto di dirti il mio parere, che è basato sull’ammirazione e l’interesse per i tanti lavori che tu hai dedicato alla disuguaglianza di Bell in anni passati. Una rassegna di quei risultati ed una rivendicazione della loro priorità rispetto a lavori successivi di Mermin sarebbe secondo me estremamente utile per mettere bene a fuoco i contributi italiani alle ricerche sul paradosso di EPR. È possibile, forse probabile, che ci sia stato un tuo cambiamento di atteggiamento rispetto a quei tempi ed a quei lavori, e nulla impedisce che tu descrivi questo cambiamento, ma resta pur sempre un fatto che vi sono risultati nei tuoi lavori di natura più tecnica che filosofica che dovrebbero essere più noti e meglio valorizzati. Io stesso avrei beneficio da una tale rassegna, nonostante ricevessi costantemente i lavori che scrivevi. Ma chi ha più tempo di rileggere le cose passate?

Fra parentesi Mermin non ci sarà a Cesena, perché non l'ho invitato nonostante le pressioni americane. Il fatto è che lo invitammo a Urbino e poi ci trattò come degli scemi (se n'era accorto). Meglio tenere a bada certi grandi geni della fisica contemporanea (Selleri to Silvio Bergia, April 15th, 1991-FSPA. My underline).

SECTION 2.7.8-PP. 408-409: THE ORIGINAL ITALIAN TEXT BY (NUTRICATI 1998)

L'idea che le variabili nascoste non esistano e quindi che l'attuale teoria quantistica sia completa [...] porta a un certo numero di incredibili paradossi, come quelli del gatto di Schrödinger, della scatola di De Broglie, dell'amico di Wigner e della scelta ritardata di Wheeler. Questi paradossi si sciolgono come neve al sole non appena si accetti il ragionevole punto di vista che la teoria presente sia incompleta (il che naturalmente, di per sé, non implica che contenga errori). La loro esistenza è solo conseguenza dell'arroganza implicita nell'idea che una teoria umana possa essere perfetta (Nutricati 1998, pag. 9. My underline).

SECTION 2.7.8-P. 409: THE ORIGINAL ITALIAN TEXT BY (SELLERI 2001-2005)

Tutti i paradossi [...] trovano una ovvia soluzione ammettendo che la meccanica quantistica sia incompleta: non è meglio questa scelta, del doversi occupare di retroazioni del futuro sul passato, di stati della realtà non osservata radicalmente diversi da quelli che vediamo, di prove dell'esistenza di un'anima trascendente e di parapsicologia? Ma c'è di peggio. Il rifiuto di serie ricerche che partano da una franca ammissione dell'incompletezza della teoria attuale impedisce che vengano compiuti quegli esperimenti, suggeriti ormai da diversi autori, che potrebbero far fare un salto di qualità alla nostra comprensione delle più fondamentali proprietà dei sistemi atomici. [...] Solo il paradosso di Einstein, Podolsky e Rosen ha una posizione logica distinta. Non lo si può risolvere ammettendo che la teoria esistente sia incompleta, ma entra in gioco la stessa validità delle predizioni statistiche della meccanica quantistica, che invece non è necessariamente toccata dalla soluzione dei paradossi che abbiamo discusso sopra" (Selleri 2001-2005, pagg. 46-47).

SECTION 2.7.8-PP. 409-410: THE ORIGINAL ITALIAN TEXT BY (NUTRICATI 1998)

Il paradosso di Einstein, Podolsky e Rosen è di un tipo molto speciale: sarà ben presto risolto sperimentalmente. Questo accadrà grazie ad una nuova serie di esperimenti che usano rivelatori ad alta efficienza, forse con coppie di fotoni, o più probabilmente in fisica delle particelle usando le disintegrazioni dei mesoni ϕ in coppie di mesoni K neutri [...]. Il risultato non potrà che essere o il superamento dell'esistente teoria dei quanti, o la morte di quello che appare oggi come la sola descrizione razionale della realtà fisica, il realismo locale. Si può anche dire che una descrizione razionale della realtà atomica diventerà pienamente possibile solo quando si sarà dimostrato che la teoria esistente è errata in qualcuna delle sue predizioni. Da un punto di vista strettamente logico esiste la possibilità che alla fine si debba concludere che quello che è stato considerato razionale nella storia del pensiero (la descrizione causale nello spazio e nel tempo) non sia più razionale a livello atomico.

Naturalmente questa è la soluzione preferita dai difensori del paradigma dominante, ma bisogna aggiungere che la sua completa arbitrarietà scientifica è diventata così evidente a chi ha voglia di vederla che oggi può apparire probabile che l'esito finale di questo confronto sia favorevole al realismo locale (Nutricati 1998, pag. 14. My underline).

SECTION 2.7.8-PP. 410-411: THE ORIGINAL ITALIAN TEXT BY SELLERI'S DOCUMENT

La soluzione più radicale del paradosso è quella che rifiuta l'argomentazione di Einstein, Podolsky e Rosen perché basata sul criterio di realtà. Vi sono a priori tre modi di rifiutare il criterio di realtà di EPR:

1) Uno può dichiararsi insoddisfatto non dell'idea che vi sia una realtà a noi esterna, ma del modo specifico contenuto nel criterio di realtà di riconoscerne l'esistenza. Talvolta questa obiezione è stata fatta, anche se non con grande convinzione. Si può notare che il criterio di EPR è basato sul determinismo, dato che richiede di fare previsioni *con certezza*. [Ma] [...] il paradosso di EPR può essere basato su un criterio di realtà molto più generale, senza che nulla cambi in sostanza. Questa prima speranza di superare il paradosso è dunque infondata.

2) Uno può abbracciare una filosofia esplicitamente idealistica negando semplicemente che esista una realtà oggettiva indipendente dall'uomo e dalle sue osservazioni, almeno per quanto riguarda il mondo degli atomi.

3) Uno può abbracciare una filosofia formalmente agnostica affermando che l'idea di un mondo atomico reale indipendente dalle nostre osservazioni è un'idea *metafisica*.

La posizione assunta da molti fisici importanti è la terza, mentre non esiste praticamente nessuno che difenda esplicitamente la seconda. Occorre però notare che la differenza fra 2 e 3 non è poi così rilevante, dato che in entrambi i casi si proibisce l'introduzione di una realtà atomica (Selleri causalità impossibile 1987, pag. 112).

[...] L'attacco più sistematico al lavoro di Einstein, Podolsky e Rosen, che può ancora oggi essere facilmente adattato al rifiuto delle premesse necessarie per dedurre la disuguaglianza di Bell, fu quello di Bohr. L'arma usata [...] fu la sua idea della complementarità [...] [secondo la quale] è la stessa idea di realtà oggettiva che appare inaccettabile. [Per Bohr] "un criterio di realtà [...] contiene una ambiguità essenziale quando viene applicato ai problemi reali".

[...] Bohr non dice mai che il ragionamento di EPR è sbagliato, anzi sembra proprio pensare che esso sia del tutto corretto se se ne accettano le premesse. È dunque su queste ultime che va cercato il vero disaccordo [...] [e Bohr sembra] trovarlo proprio nell'idea che si possa [...] parlare di una realtà fisica non osservata. In sostanza, Bohr considera reali solo i nostri atti di osservazione e nient'altro. È proprio per questo motivo che la radice del dissidio va considerata di natura filosofica. Si tratta di un'ovvia conclusione, anche se la maggioranza dei fisici contemporanei pensa che Bohr abbia dimostrato errato il ragionamento di EPR (magari senza averne letto il lavoro!).

[...] Bohr [invita] ad accettare "una revisione radicale del nostro atteggiamento nei riguardi della realtà fisica".

Reale, per lui, è solo ciò che è concretamente misurato da un fisico: l'antropomorfismo cacciato dalla porta agli albori della fisica moderna rientra così dalla finestra con la nuova teoria dei quanti.

[...] Bohr non aveva nient'altro da obiettare, e questo mette in piena luce il fatto che la differenza [...] era filosofica nel senso più profondo del termine. La comunità scientifica si trovò davanti alla scelta fra due posizioni filosofiche opposte, una delle quali garantiva il mantenimento del paradigma appena nato, e non ebbe dubbi nella scelta.

È interessante ricordare che Einstein disse in diverse occasioni di non essere riuscito a capire tutto il contenuto della risposta di Bohr e questa non era probabilmente un'affermazione ironica (o lo era solo in parte). In effetti è tuttora possibile fare i conti fino in fondo con le idee di Bohr solo alla condizione di aver fatto alcune letture insolite (per i fisici) sulla dialettica qualitativa di Kierkegaard (Selleri causalità impossibile 1987, pagg. 113-115).

SECTION 2.7.8-P. 415: THE ORIGINAL ITALIAN TEXT BY THE DOCUMENT OF SELLERI

[...] Un terzo autore che introduce questa idea è il noto fisico polacco Rayski che [...] conclude che la funzione d'onda della meccanica quantistica "non si riferisce direttamente alla realtà fisica ma rappresenta piuttosto la nostra conoscenza di questa realtà". L'azione a distanza esibita dal paradosso di EPR può essere considerata come pura apparenza, mentre l'informazione ottenuta con un atto di misura su uno dei due oggetti formanti una coppia di tipo EPR in realtà si propaga all'indietro nel tempo fino all'istante della comune emissione da parte della sorgente e - passando dal primo al secondo oggetto - si propaga con esso in avanti nel tempo.

Un punto di vista simile è sostenuto da Rietdijk secondo il quale la più naturale idea di realtà della teoria della Relatività Speciale implica che eventi che sono futuri per un osservatore possano essere presenti - e perciò già *reali* - per un secondo osservatore in moto rispetto al primo. Siccome la descrizione della realtà del secondo osservatore è tanto valida quanto quella del primo siamo spinti ad accettare la realtà quadridimensionale dell'universo, in cui passato, presente e futuro sono sullo stesso livello - dati, reali ed in tutti i casi "già esistenti". Pertanto vi possono essere delle azioni retroattive del "già esistente" futuro sul presente, ed è possibile immaginare una soluzione del paradosso di EPR secondo questa linea di pensiero. Le precedenti idee [...] sono di certo molto naturali per un fisico teorico necessariamente abituato a trattare con strutture formali invarianti per lo scambio passato-futuro. È difficile per una persona "normale" evitare la sensazione di avere un po' a che fare con della fantascienza, anche perché l'irreversibilità fa parte della vita quotidiana di ciascuno di noi (Selleri causalità impossibile 1987, pag. 118).

SECTION 2.7.8-P. 416: THE ORIGINAL ITALIAN TEXT BY THE DOCUMENT OF SELLERI

Un'altra classe di "soluzioni" del paradosso di EPR postula l'assenza di separabilità dei sistemi quantistici e cerca di salvarne la realtà introducendo meccanismi per la comunicazione istantanea a distanza di due oggetti.

La prima proposta di questo tipo fu probabilmente quella di Bohm secondo cui la non località è proprio l'aspetto più essenziale ed ineliminabile della teoria quantistica: un sistema non può essere analizzato in parti le cui proprietà non dipendano dallo stato dell'intero sistema, non importa quanto ben separate nello spazio queste parti possano apparire (Selleri causalità impossibile 1987, pag. 118).

SECTION 2.7.8-PP. 421-422: THE ORIGINAL ITALIAN TEXT BY SELLERI

Vi sono, fortunatamente, filosofi della scienza che difendono ancora la causalità. [...] Un'importante distinzione che occorre fare quando si considerano i processi naturali di cui si occupa la scienza, è fra causalità e determinismo. Su questo terreno bisogna dire che molto è stato fatto nel Novecento dai filosofi della scienza e poco dai fisici, che hanno superato il determinismo della fisica classica con le leggi probabilistiche quantistiche, ma che poi hanno concluso da ciò che nessuna descrizione causale è possibile. [...] le conclusioni antideterministiche fondate sul teorema di von Neumann erano infondate.

Analoghe conclusioni sono state tratte per le relazioni di Heisenberg e per la complementarità di Bohr [...], tanto che oggi si può dire che una descrizione deterministica dei processi atomici è diventata possibile. Tuttavia il fatto che sia possibile non significa che sia auspicabile, perché processi casuali sono molto comuni nel dominio atomico e sembra una forzatura cercarne una descrizione strettamente deterministica. Inoltre, può essere un errore "tattico" affidarsi a una descrizione un po' estrema come il determinismo, quando una comoda descrizione causale è ampiamente possibile. Questa conclusione è rafforzata pensando alla storia recente della causalità e alla forte repressione generata dal teorema di von Neumann sulle tendenze realistiche.

Se si fosse subito capito che il teorema affrontava il problema del determinismo ma nulla poteva dire contro la causalità, molto spazio sarebbe stato sottratto alla dilagante influenza della scuola di Copenhagen, rinunciando al determinismo ma continuando a difendere una visione causale dei processi atomici (Selleri 2001-2005, pag. 23. My underline).

[Si possono] interpretare moltissimi risultati della meccanica quantistica senza rinunciare alla causalità, anche se non tutti. [...] nel caso del paradosso di Einstein, Podolsky e Rosen il principio di causalità in qualsiasi formulazione ragionevole porta a predizioni empiriche in contrasto con la teoria esistente (Selleri 2001-2005, pag. 25. My underline).

SECTION 2.7.8-PP. 423-424: THE ORIGINAL ITALIAN TEXT BY SELLERI

Per un realista, il criterio di realtà di EPR ha una base concettuale piuttosto solida. Tuttavia, esso può essere criticato in due modi non del tutto indipendenti. In primo luogo, si può osservare che ha un'applicabilità piuttosto ristretta, dato che riconosce come reale nel dominio atomico, in pratica, solo ciò che la meccanica quantistica descrive con autostati dell'osservabile misurata. Perché mai [...] non si possono introdurre elementi di realtà per tutte quelle situazioni che la teoria descrive in modo più generale, mediante sovrapposizioni? Einstein, Podolsky e Rosen erano ben consci di questa limitazione, tanto che scrissero che il loro criterio "pur lontano dall'esaurire tutti i modi possibili di riconoscere una realtà fisica, almeno ci fornisce uno di questi modi" (EPR 1935, pag. 778). Il secondo punto debole [...] sta nella predicibilità con certezza della considerata quantità

fisica: quando mai si può essere assolutamente certi di qualcosa in un situazione fisica concreta? La predicibilità certa del 1935 sembrava essere conseguenza della teoria quantistica per certi tipi di vettore di stato (autofunzioni). La situazione è tuttavia diversa oggi [...] dopo il teorema di Wigner, Araki e Yanase, che ha dimostrato la necessità di misure imperfette, anche in linea di principio, se la teoria dei quanti deve essere consistente con le note leggi di conservazione [...] anche nella sua descrizione dei processi di misura

Queste difficoltà possono essere entrambe superate per mezzo di una generalizzazione probabilistica del criterio di realtà, in cui *la predizione di una probabilità* viene usata per dedurre l'esistenza di proprietà fisiche reali di insiemi statistici opportuni. Il realismo locale probabilistico può essere formulato in termini tanto generali che molti dovrebbero trovare facile accettarlo. Infatti: (a) se non esistesse una qualche forma di realtà dietro a una probabilità stabile e predicibile, allora non resterebbe veramente più alcun tipo di realtà materiale;

(b) anche le stesse probabilità della meccanica quantistica sono locali nel senso [...] del realismo locale probabilistico (Selleri Paradossi, 2001, pagg. 142-143).

SECTION 2.7.8-P. 425: THE ORIGINAL ITALIAN TEXT BY SELLERI

Una nuova descrizione della realtà fisica in termini probabilistici basata sul concetto di propensione fu proposta da Popper, in maniera però slegata dal paradosso EPR. Lo scopo che si proponeva era una riformulazione del calcolo delle probabilità quantistiche in termini oggettivistici e realistici, dunque senza riferimento ai soliti concetti idealistici della scuola di Copenhagen.

Chi scrive ha cercato di sviluppare questo programma e crede di aver dimostrato che ogni formulazione oggettivistica e locale della probabilità porta alla validità della disuguaglianza di Bell e dunque all'impossibilità di un pieno accordo con l'esistente meccanica quantistica (Selleri Paradossi, 2001, pag. 143).

SECTION 2.7.8-PP. 425: THE ORIGINAL ITALIAN TEXT BY SELLERI

[...] il concetto di propensione esprime una tendenza insita nell'oggetto studiato a produrre certi esiti quando viene lasciato evolvere in un certo ambiente. La tendenza va intesa con una sua "intensità" numerica e non semplicemente come mera possibilità qualitativa di produrre una certa evoluzione. La frequenza con cui si manifesterà quella evoluzione tra le tante possibili sarà una misura quantitativa dell'intensità della tendenza, ossia della propensione. Così posto il problema, [...] propensione è un termine abbastanza simile a probabilità. E infatti Popper dice che: "l'idea della propensione è la migliore interpretazione a me nota dell'applicazione del calcolo delle probabilità a un certo tipo di 'esperimenti ripetibili', specialmente in fisica [...]" (Selleri Dogma 1989, pag. 181).

SECTION 2.7.8-PP. 425-426: THE ORIGINAL ITALIAN TEXT BY SELLERI

Da un singolo atto di misura non si può dedurre una probabilità e le propensioni individuali restano non misurabili. Per un insieme di sistemi atomici si possono però introdurre le propensioni a dare diversi risultati con diverse frequenze. Queste ultime propensioni sono invece facilmente misurabili e possono essere considerate una specie di media fisica delle propensioni individuali non osservabili.

Grazie al concetto di propensione si può formulare il criterio di realtà probabilistica. Sia dato un insieme S di oggetti fisici dello stesso tipo (ad esempio, elettroni), un apparato sperimentale A capace di misurare la grandezza fisica F degli elettroni, e siano f_1 ed f_2 i soli due possibili risultati di F [...]. Il criterio di realtà probabilistica può essere formulato come segue:

se è possibile prevedere l'esistenza di un sottoinsieme S' di S senza disturbare in alcun modo gli oggetti componenti S' ed S ; se è anche possibile prevedere correttamente che misure future di F su S' daranno i risultati f_1 e f_2 con frequenze rispettive p_1 e p_2 allora esiste una proprietà fisica reale λ' (propensione) appartenente ad S' che determina i valori delle probabilità e si ha $p_1=p_1(\lambda')$ $p_2=p_2(\lambda')$ (Selleri Paradossi, 2001, pag. 144. My italics).

SECTION 2.7.8-P. 426: THE ORIGINAL ITALIAN TEXT BY SELLERI

[...] Non si vede un ambito di fenomeni a cui questo nuovo criterio di realtà non possa essere applicato. La sua generalità è tale che può forse sembrare del tutto innocuo. In realtà, accoppiato alle idee di località e freccia del tempo, esso costituisce un'arma potente e permette, in particolare, di dimostrare la validità di quelle disuguaglianze di Bell che l'esistente teoria dei quanti invece viola (Selleri Paradossi, 2001, pag. 145).

SECTION 2.7.9-P. 428: THE ORIGINAL ITALIAN TEXT BY SELLERI

È interessante osservare che le violazioni del realismo locale contenute nella meccanica quantistica non dipendono da eventuali difficoltà collegate con la rappresentazione delle grandezze misurabili ("osservabili") mediante operatori.

[...] È invece dalle proprietà algebriche della funzione d'onda descriventi due sistemi correlati che nasce l'incompatibilità con il realismo locale. Ma nemmeno tutte le funzioni d'onda generano violazione della disuguaglianza di Bell: è infatti possibile introdurre *due tipi* di funzione d'onda. Del primo tipo sono quelle fattorizzabili [...], mentre del secondo tipo sono quelle *non* fattorizzabili. Ebbene, solo gli stati del secondo tipo generano il paradosso di EPR, mentre quelli del primo tipo sono pienamente compatibili con la disuguaglianza di Bell! Tutto avviene come se il principio di sovrapposizione, che implica l'esistenza di stati non fattorizzabili, fosse responsabile del paradosso (Selleri causalità impossibile 1987, pag. 107. *Italics in the original text. My underline*).

SECTION 2.7.9-P. 428: THE ORIGINAL ITALIAN TEXT BY SELLERI

Tutte le difficoltà irrisolte della meccanica quantistica (paradosso di EPR, disuguaglianza di Bell, teoria della misura, ecc.) nascono dalla presenza nella teoria di vettori di stato non fattorizzabili. Tale situazione ha portato molti fisici a credere che si potesse riformulare la meccanica quantistica con soli vettori di stato fattorizzabili. In (Fortunato, Garuccio, Selleri 1977) è stata dedotta un'osservabile il cui valore differisce notevolmente a seconda del tipo (fattorizzabile o meno) di vettore di stato associato al sistema fisico studiato. La concreta misurazione di tale osservabile è stata effettuata da Clauser. Successivamente si è però dimostrato in (Garuccio, Scalera, Selleri 1977) che una formulazione della meccanica quantistica senza vettori di stato non fattorizzabili portava a una teoria con nuovi gravissimi aspetti paradossali (FSPA-My underline).

SECTION 2.7.9-P. 444: THE ORIGINAL ITALIAN TEXT BY (LAURORA 2014)

[...] un altro protagonista della storia sulle ricerche del paradosso EPR, che dalla fine degli anni '60 aveva sviluppato un atteggiamento critico nei confronti dell'interpretazione della teoria dei quanti e che ha fornito importanti contributi sia al chiarimento delle idee alla base dei fondamenti della Meccanica Quantistica, sia allo sviluppo dell'indagine sperimentale sulla verifica del teorema di Bell (Laurora 2014, pag. 61).

SECTION 2.7.9-PP. 444-445: THE ORIGINAL ITALIAN TEXT BY (TAROZZI 2013)

La critica di Franco Selleri (d'ora in poi Franco) all'interpretazione ortodossa della meccanica quantistica si è costantemente concentrata sulla sua natura antirealistica, configurandosi come il presupposto filosofico di un programma di ricerca sui fondamenti della fisica che ha evidenziato l'esistenza di ulteriori paradossi e contraddizioni della formulazione *standard* rispetto a quelli già conosciuti, e cercato di individuare nuove vie verso interpretazioni alternative.

Franco ha per primo messo in risalto con estrema chiarezza attraverso argomentazioni rigorose, sia sul piano formale sia su quello fisico, come l'interpretazione ortodossa della scuola di Copenaghen (o meglio di Copenaghen e Göttingen) [...] fosse in aperto conflitto con una prospettiva filosofica di tipo realistico da due diversi punti di vista: in primo luogo per quanto riguardava il concetto teorico fondamentale di funzione d'onda, svuotato di significato fisico nell'interpretazione di Born, in secondo luogo rispetto al problema più generale del cosiddetto realismo locale.

Quest'ultimo corrispondeva secondo Franco alla simultanea assunzione di validità del criterio di realtà fisica di EPR e del principio di località einsteiniano, che appariva incompatibile sia dal punto di vista logico che empirico con la descrizione quantistica attraverso vettori di stato non fattorizzabili, che venivano in quegli anni definiti come stati di EPR o di seconda specie, per distinguerli dagli ordinari stati fattorizzabili di prima specie, mentre ora in letteratura è prevalsa la denominazione di stati *entangled*, o *entanglement* (Tarozzi 2013, pagg. 223-224).

[...] Alle ricerche sul problema dell'incompatibilità sia logica che empirica tra meccanica quantistica e realismo locale, e al duplice problema delle ipotesi in gioco nella dimostrazione del teorema di Bell e del suo dominio di validità, Franco ha dato un contributo che può essere difficilmente sopravvalutato nella fisica del '900. Nella più autorevole ricostruzione del dibattito sui fondamenti e le interpretazioni della teoria quantistica dalle origini fino agli inizi degli anni '70, [ovvero, Jammer 1974], l'autore metteva in risalto l'importanza di questo contributo, mostrando come, oltre ad avere dimostrato la forma più forte [e strutturata, paradossalmente detta debole] della disuguaglianza di Bell, Franco avesse individuato le radici logiche del problema nel conflitto tra stati *entangled* e stati non *entangled*, nonché l'esistenza di altre osservabili diverse da quella di Bell in grado di discriminare rispetto alle descrizioni che utilizzavano l'uno o l'altro di tali stati. [Jammer] si soffermò nuovamente sul ruolo dei contributi allo studio del problema dato da Franco e dai suoi collaboratori, nella sua relazione di apertura al simposio in memoria di John Bell del 1991 (Tarozzi 2013, pag. 226. My underline)

SECTION 2.7.10-PP. 449-450: THE ORIGINAL ITALIAN TEXT BY THE FULL SELLER'S
NOTEBOOK

CARTELLA QUADERNI E APPUNTI-mid 80s

Vari teoremi alla von Neumann

- teorema di von Neumann
- teorema di Jauch-Pironi
- teorema di Gleason
- teorema di Kochen-Specker
- teorema di Salomon
- teorema di Destouches-Fevrier
- teorema di Gudder

non è possibile che questi non capissero i limiti dei loro teoremi

Modelli di Bohm e di Bell fanno l'impossibile

Paradigma ora non protetto

Questo ruolo per la ddB è voluto dai conservatori. Ma gli innovatori vedono cose diverse.

Scoprono cose bellissime

- 1) Formulazione probabilistica
- 2) stesse diseguaglianze di quella deterministica
- 3) propensioni
- 4) le infinite diseguaglianze

Realismo locale banalmente vero in tutte le situazioni esaminate

La ddB scoperta molto dopo la nascita della MQ (1927-1965).

Strano per un fenomeno ultrafondamentale come la non località.

In che senso sarebbero nascoste? Più palesi di così non possono essere!

Le variabili nascoste possono essere locali, nonlocali, miste. Non ha importanza.

**SE FOSSE VERO CHE LA DDB È VIOLATA SAREBBE LA FINE DEL REALISMO
LOCALE.**

**TUTTI I MODELLI REALISTI E LOCALI SODDISFANO IL RL. VIOLATO QUESTO,
DISTUTTI QUELLI, SE LA VIOLAZIONE È SISTEMATICA.**

MEGLIO RIFLETTERE PRIMA DI ACCETTARE LE VIOLAZIONI.

**UNA ISTANZA APERTA A CONTRIBUTI DIVERSI È O DOVREBBE ESSERE
NELL'INTERESSE DI TUTTI.**

**LA CHIUSURA TOMBALE DEL PARADOSSO EPR+DDB SOFFOCHEREBBE TUTTE LE
TENDENZE VERSO IL RINNOVAMENTO**

CLIMA CULTURALE

- Bell era a disagio
- Esperimenti fatti da giovani sconosciuti. Poi, trovato il risultato desiderato, un *tam tam* internazionale li rendeva notissimi
- Risultati spesso dati per scontati a priori dagli autori
- Tensioni e scontri nelle conferenze
- I difensori della località avevano vita dura. Erano pochi perché ci voleva del coraggio ad assumere la difesa della località. Anche questo non ha senso scientifico
- Scambi con Mandel
 - o OCdB circa i ritardi di Aspect (Costa de Beauregard)
 - o Giovani carriere stroncate (es. Lepore, De Caro)
 - o Esperimenti suggeriti, mai fatti (suggeriti da me, per es)

Vi furono esperimenti che violavano la MQ

Holt-Pipkin (mai pubblicato)

Clauser LNC

Catania LNC

Costretti a non pubblicare i loro risultati non perché avessero trovato errori nel loro esperimento, ma puramente con minacce di repressione.

Non è mai partita la dialettica di esperimenti diversi che approfondiscano un aspetto della fisica quantistica.

Tematica inchiodata a una forma minimale.

Selleri: fondamenti della fisica moderna pp. 33-34

“non è un caso che tutti quelli che guardano con simpatia al mondo magico dei fenomeni ‘paranormali’ accettano senza difficoltà la violazione della ddB”

Il valzer delle ipotesi addizionali-diseguaglianze forti e deboli

Teorie probabilistiche locali hanno probabilità di singola particella. Locali nel senso della MQ, eppure violano la ddB.

Se neanche una probabilità sottende qualcosa di reale, tutta la realtà è sparita

quando è nata la MQ non ci fu alcuna discussione/dibattito su quella che è diventata la cosa più profonda dell’intera MQ. Infatti la non località non è nemmeno menzionata dai grandi fisici che poi si divisero in due fronti contrapposti.

SECTION 2.7.10-PP. 450-451: THE ORIGINAL ITALIAN TEXT BY NUTRICATI 1998

Tutti i fisici che si sono occupati della disuguaglianza di Bell (pragmatisti, positivisti, idealisti, spiritualisti, realisti) sono concordi sul fatto che il Teorema di Bell non riguardi un particolare modello di realtà locale ma che si tratti di un teorema della massima generalità, [...] decisivo per gli sviluppi della fisica.

[...] un'eventuale conferma della teoria della meccanica quantistica porterebbe ad annullare la validità del realismo locale e quindi, ad una sconfitta irreversibile del realismo. Il razionalismo, infatti, è una filosofia che allo stesso tempo è estremamente forte ed estremamente debole. È estremamente forte nella sua capacità di penetrare nella realtà, di guidare verso la comprensione dei fenomeni naturali. È estremamente debole nel senso che, abbattendo uno solo dei suoi principi, crolla tutto. Esso è infatti una filosofia che tende alla totalizzazione e quindi anche all'esclusione di ogni tipo di "miracolo". Ecco perché [Selleri] è convinto che la battaglia sulla validità della meccanica quantistica oppure sulla validità del realismo locale, e quindi della disuguaglianza di Bell, è la battaglia decisiva in un senso o nell'altro.

[...] se il razionalismo perde una sola battaglia, perde anche la guerra (Nutricati 1998, pagg. 104-105. My underline).

SECTION 2.8.2-PP. 453-454: THE ORIGINAL ITALIAN TEXT BY WIKIPEDIA, WHOS LINK IS IN FOOTNOTE 626

I primi segnali di ritorno al privato si videro tra la fine del 1978 e l'inizio del 1979, con il rilancio dei consumi durante le feste natalizie, con il rifiuto delle ideologie e dei partiti.

[...] La svolta del riflusso e il ritorno al privato viene spesso identificata nell'autunno del 1980 con la marcia dei quarantamila a Torino, quando tornò alla ribalta l'esistenza di una "maggioranza silenziosa" che si contrapponeva al clamore degli scontri sociali del decennio precedente. Il 14 ottobre numerosi quadri intermedi della FIAT, stanchi delle continue proteste dei sindacati che si opponevano alla cassa integrazione, e che impedivano loro di entrare in fabbrica a lavorare, diedero vita a un corteo "silenzioso" per la città. La loro manifestazione mise a tacere gli scioperi e le occupazioni.

[...] Il periodo si caratterizzò per un diffuso ritorno delle persone dalle piazze alla sfera privata, dall'inizio dell'era della televisione commerciale unito a un decollo della pubblicità e a un incremento dei consumi.

[...] A livello politico iniziò a prevalere la personalizzazione sull'appartenenza ideologica; ci fu così un declino del potere dei sindacati e del Partito Comunista Italiano, parallelamente all'ascesa di Bettino Craxi tra le file del Partito Socialista Italiano, chiamato nel 1976 a risollevarne le sorti del partito che allora si trovava ai minimi storici, stretto nella tenaglia del tentativo di compromesso storico tra la DC e il PCI. [...] Il disimpegno nell'azione politica toccò anche i giovani, sostituito dalla nascita di sottoculture giovanili (Wikipedia, see footnote 626).

SECTION 2.8.2-PP. 454-455: THE ORIGINAL ITALIAN TEXT BY THE WEB ARTICLE
L'ITALIA DEGLI ANNI '80, BY CARLO DE MARIA E TITO MENZANI

Gli anni ottanta sono una categoria storiografica relativamente recente. Rappresentano il decennio che ha incubato il tramonto del comunismo nel mondo occidentale, e di conseguenza anche la fine della guerra fredda e l'ascesa delle istanze neoliberiste. In questa fase si risolse il conflitto fra sogno americano e mito sovietico che si era a lungo sostanziato su due piani distinti. Da un lato vi era il confronto militare, fatto di una corsa agli armamenti e di un duello a distanza per sostenere governi amici nei paesi emergenti. Dall'altro uno scontro culturale, che tendeva a presentare all'opinione pubblica internazionale il proprio modello di sviluppo come quello migliore in assoluto.

Tuttavia, nel 1985 alla guida dell'Unione Sovietica fu nominato Michail Gorbacev, primo segretario del Pcus a non essere stato coinvolto direttamente dallo stalinismo e deciso ad introdurre varie riforme nel paese. L'intenzione di fondo era una correzione della pianificazione centralizzata attraverso elementi di economia di mercato e un allentamento della monocratica sovrapposizione fra strutture dello Stato e del partito comunista, per legittimare anche alcune moderate espressioni di dissenso. In breve tempo, Gorbacev guadagnò un ampio consenso internazionale, e fu visto con favore e fiducia soprattutto dall'opinione pubblica occidentale.

Nel 1985, [Gorbacev e Reagan] si incontrarono a Ginevra, un anno dopo nuovamente a Reykjavik, e un anno dopo ancora una terza volta a Washington.

Pur se nel concreto gli accordi intercorsi tra Usa e Urss si limitavano a pattuire una riduzione degli armamenti e a qualche specifica intesa di non ingerenza a livello internazionale – l'Urss si impegnò a cessare l'occupazione militare dell'Afghanistan – l'impatto mediatico di questi incontri fu enorme e da più parti si convenne che la guerra fredda era approdata ad una nuova stagione.

Nel contempo, l'Europa occidentale proseguiva nel processo di integrazione economica, e in subordine anche politica, che aveva timidamente avviato dopo la seconda guerra mondiale. Gli anni Ottanta segnarono un'accelerazione di questo processo, grazie al lungo e laborioso (ma soprattutto fruttuoso) lavoro diplomatico che portò, nel 1985, agli accordi di Schengen (effettivi solo dal 1990).

Questi stabilivano la libera circolazione delle persone tra i paesi europei firmatari, e furono in un certo senso il preludio del trattato di Maastricht, firmato il 7 febbraio 1992 dai dodici paesi membri della Cee ed entrato in vigore il 1 novembre dell'anno successivo, che sancì la nascita dell'Unione Europea, in sostituzione della precedente Comunità economica.

Questi scenari di progressiva distensione furono accompagnati da una virata liberista nella vita politica dei principali paesi ad economia matura. Gli anni settanta erano stati praticamente ovunque l'acme di una stagione di lotte e di rivendicazioni politico-sindacali iniziate nel secondo dopoguerra e implementate dai movimenti nati dal '68. Nel decennio successivo queste istanze rifluirono, anche sull'onda di un ricambio generazionale che dava spazio a giovani cresciuti nella civiltà del benessere e dei consumi. La *forma mentis* di queste nuove leve e gli interessi di importanti lobby economiche fecero da substrato all'ascesa del neoliberismo, una concezione politico-economica che mira alla deregolamentazione e alla riduzione del peso dello Stato nell'economia pubblica. La premier inglese Margaret Thatcher fu tra i principali leader mondiali a cavalcare questa dottrina, e individuò nell'impresa pubblica il principale obiettivo polemico.

Economisti come Friedrich von Hayek e Milton Friedman – principali teorici del neoliberismo – avevano buon gioco nel denunciare l'inefficienza delle imprese pubbliche, ma la soluzione radicale che proponevano, e cioè la privatizzazione, finì in vari casi per comportare gravi costi sociali, perché applicata in maniera spesso indiscriminata, anche nei confronti di aziende che operavano in monopoli naturali o che gestivano servizi essenziali.

L'altro volto del neoliberismo fu l'attacco alle forze sindacali, accusate di avere un atteggiamento conservativo o troppo ideologico, in ogni caso ostile alla proprietà delle imprese, e dunque colpevoli di frenare lo sviluppo.

SECTION 2.8.2-P. 456: THE ORIGINAL ITALIAN TEXT BY THE WEB ARTICLE L'ITALIA DEGLI ANNI '80, BY CARLO DE MARIA E TITO MENZANI

Proprio in Italia fu ben evidente che il passaggio dagli anni Settanta agli Ottanta avesse comportato una cesura nella storia del sindacato. Dopo le agitazioni del '68 vi era stato un generalizzato miglioramento delle condizioni salariali e le riforme progressiste varate dal Parlamento avevano contribuito ad una generale avanzata delle sinistre in tutto il paese.

Il dibattito interno al Pci e agli altri movimenti e partiti di sinistra finì [invece] per diventare in buona parte autoreferenziale, e trascurare quella parte di cittadinanza che si riconosceva in una proposta politica di segno opposto. Fu una sorta di sbornia ideologica, con un brusco risveglio il 14 ottobre del 1980.

Quel giorno, nel cuore della Torino operaia, si tenne la cosiddetta "marcia dei quarantamila", e cioè una manifestazione dei quadri e degli impiegati Fiat contro il sindacato, che da oltre un mese era in agitazione e impediva a molti di loro di accedere al posto di lavoro attraverso picchetti ai cancelli. [La marcia era] una dura presa di posizione del ceto medio di fabbrica contro la retorica operaista e le prassi rivendicative adottate dal sindacato. Al di là degli effetti immediati – e cioè la rapida chiusura della vertenza che aveva innescato gli scioperi – il corteo dei quarantamila ebbe un impatto emotivo enorme.

SECTION 2.8.2-PP. 456-457: THE ORIGINAL ITALIAN TEXT BY THE WEB ARTICLE L'ITALIA DEGLI ANNI '80, BY CARLO DE MARIA E TITO MENZANI

Tra i protagonisti [...] della politica italiana va sicuramente annoverato Bettino Craxi, divenuto segretario del Psi nel 1976, dopo che il consenso elettorale al partito era sceso sotto la soglia del 10%. Fortemente critico nei confronti del comunismo, Craxi si presentò come un decisionista attento alla società e ai diritti civili, capace di dialogare con i movimenti e con ampie fasce del ceto medio.

[...] Incaricato di formare il nuovo governo, Craxi fu il primo socialista a diventare presidente del consiglio. [...] confermò l'alleanza del cosiddetto «pentapartito», tra democristiani, socialisti, socialdemocratici, repubblicani e liberali, e varò un esecutivo destinato a restare in carica quasi tre anni, un record nella storia della repubblica. Ad esso, tra l'altro, seguì un secondo governo Craxi, più breve, per cui egli rimase di fatto presidente del consiglio dal 1983 al 1987, connotando di conseguenza il decennio degli anni ottanta.

[...] In politica interna, nel 1984, Craxi varò un decreto che ridimensionava la scala mobile, ossia un meccanismo che adeguava i salari all'inflazione. Pur se questo strumento era importante per tutelare il potere d'acquisto, contribuiva ad aggravare il problema del caro-vita. Il Pci e la Cgil fecero un'opposizione durissima alla riforma della scala mobile e raccolsero anche le firme per un referendum abrogativo del provvedimento, che però – a sorpresa – vide i sì attestarsi al 45,7%, contro il 54,3% di favorevoli alla riforma craxiana. Si trattò di una seconda grave sconfitta per il principale sindacato italiano, dopo quella della marcia dei quarantamila.

[...] Inoltre, Craxi riformò il sistema televisivo italiano, all'epoca imperniato sulla dicotomia tra il servizio pubblico nazionale della Rai e le piccole emittenti private, consentendo la creazione di canali televisivi nazionali di carattere privato. Di fatto si sanava la posizione del principale imprenditore del settore, Silvio Berlusconi, titolare di Rete 4, Canale 5 e Italia 1, che già trasmettevano su tutto il territorio nazionale tramite ripetitori interconnessi. Il provvedimento fu molto discusso, perché non solo Craxi e Berlusconi erano molto amici – addirittura il primo fu testimone di nozze del secondo e padrino di battesimo di Barbara Berlusconi – ma perché la nascita del primo grande polo della tv commerciale italiana implicava un debito di riconoscenza di quest'ultima nei confronti della compagine governativa.

Peraltro, da un punto di vista culturale, l'affermazione della nuova televisione commerciale comportò, tra le altre cose, un modello di promozione pubblicitaria molto più aggressivo ed efficace del vecchio e discreto *Carosello*. Alcuni spot degli anni ottanta divennero un cult, entrando a pieno titolo nella "memoria televisiva" degli italiani.

[...] i nuovi governi [successivi a quelli di Craxi] non ebbero la forza di essere duraturi né tanto meno incisivi. Più che altro si registrò un netto distacco tra società civile e impegno politico, che toglieva ogni residuale spazio a proposte eversive o sovversive, ma che finiva anche con l'accentuare una diffidenza nei confronti dei partiti. In particolare, a fronte di crescenti critiche alla compagine governativa, le opposizioni non parevano in grado di guadagnare terreno, ancorate a schemi ideologici che agli albori degli anni novanta non sembravano più attuali.

SECTION 2.8.2-PP. 458-461: THE ORIGINAL ITALIAN TEXT BY THE WEB ARTICLE L'ITALIA DEGLI ANNI '80, BY CARLO DE MARIA E TITO MENZANI

La politica di distensione con gli Stati Uniti inaugurata da Reagan e Gorbacev contribuì ad aumentare la popolarità di entrambi. Tuttavia [...] non sfuggiva ai più che il modello sovietico fosse precipitato in una crisi seria, che si manifestava attraverso un crescente gap tra la qualità della vita russa e quella dei paesi occidentali. Le riforme promesse da Gorbacev avevano proprio l'obiettivo di rivedere i meccanismi economici e istituzionali del socialismo reale, per rilanciare questo modello in termini di competitività e credibilità internazionale. Le parole d'ordine lanciate da Gorbacev furono tre:

perestrojka (riforma), *glasnost* (trasparenza) e *uskorenje* (accelerazione dello sviluppo), anche se fu soprattutto la prima, a volte in abbinamento con la seconda, ad avere maggiore successo mediatico e ad essere ricordata dall'opinione pubblica, anche internazionale.

[...] Sul finire degli anni Ottanta, la portata di queste politiche di rinnovamento fu ben tangibile, ma le conseguenze furono decisamente distanti da quello che Gorbacev aveva immaginato. Dal suo punto di vista, la legalizzazione del dissenso avrebbe dovuto portare a critiche costruttive volte a migliorare il sistema socio-economico sovietico, invece si catalizzarono voci e opinioni discordanti tra loro, che però avevano in Gorbacev il proprio obiettivo polemico. Da un lato, una parte della burocrazia e della classe dirigente criticava le aperture all'occidente, dall'altra una frangia sempre più numerosa di dissidenti non si limitava a chiedere di riformare il sistema ma individuava nell'abbattimento del comunismo il vero fine politico.

La maggiore libertà di informazione diede improvvisamente modo ai cittadini sovietici di percepire una realtà molto diversa da quella che conoscevano attraverso l'esperienza diretta e la propaganda di regime. Gli echi di scandali relativi alla corruzione della classe politica, i confronti fra il tenore di vita a est e a ovest della cortina di ferro, gli sprechi dovuti alle inefficienze della macchina statale vennero via via rimbalzati da un giornale all'altro, approfittando dell'allentamento della censura, e contribuirono a far aumentare notevolmente il malcontento nel paese.

Le conseguenze più immediate [si ebbero però] nei paesi satellite dell'Europa dell'Est. In molti di questi, il comunismo aveva sempre più mostrato un volto tirannico e impopolare, tanto che si faticava ad immaginare una perpetuazione di questi regimi senza l'appoggio di Mosca. In Polonia, ad esempio, operava da un po' di tempo un sindacato indipendente denominato Solidarnosc, di ispirazione cattolica e apertamente ostile al comunismo. Dopo l'ascesa di Gorbacev, Solidarnosc ebbe maggiori possibilità di movimento, e ottenne che ci fossero le prime elezioni libere in una delle due camere del Parlamento. E così, nel giugno del 1989, i polacchi furono chiamati alle urne, scegliendo in massa i candidati di Solidarnosc. Era una prova lampante e definitiva dell'impopolarità del comunismo. Il nuovo papa, il polacco Karol Wojtyła, salito nel 1978 al soglio pontificio con il nome di Giovanni Paolo II, ricoprì un ruolo importante in questa così delicata fase della politica del paese. Egli, infatti, compì nove viaggi apostolici in Polonia, di cui cinque tra il 1979 e il 1991, caratterizzandosi come uno dei principali punti di riferimento politici e spirituali del movimento di Solidarnosc.

Gli eventi polacchi innescarono una reazione a catena. Nel 1989, l'Ungheria decideva di aprire la frontiera con la limitrofa Austria e di consentire ai propri cittadini di espatriare. Veniva così abbattuta la lunghissima barriera di filo spinato che correva lungo il confine, intervallata da torrette di sorveglianza. In realtà, si trattava di un provvedimento di ben più ampia portata, perché sanciva ufficialmente una breccia nella cortina di ferro. Quindi, da praticamente tutti i paesi del blocco sovietico si sarebbe potuta raggiungere l'Ungheria e da lì entrare in Austria, per poi muoversi liberamente nell'Europa occidentale. L'opportunità fu colta soprattutto dai tedeschi della Germania Est, dato che di fatto veniva data loro l'opportunità di aggirare il muro di Berlino e il confine con l'Ovest. In poche settimane 13.000 cittadini tedeschi passarono la frontiera, palesando agli occhi del mondo l'insuccesso del modello comunista. Il governo della Germania Est si dimetteva e il 9 novembre 1989, per ordine delle nuove autorità, venivano abolite le restrizioni alla circolazione fra i settori di Berlino.

Era una data storica, poi ricordata come «la caduta del muro», dato che migliaia e migliaia di berlinesi si prodigarono per abbattere materialmente il simbolo del regime e della separazione fra Est e Ovest. L'evento fu vissuto con grande partecipazione in tutto il mondo, mentre la televisione mostrava le immagini di tedeschi in lacrime che riabbracciavano amici e parenti dopo decenni di forzata lontananza.

Fu una grande scossa emotiva a tutto il sistema delle «democrazie popolari». In Cecoslovacchia, una serie di manifestazioni di piazza vedeva il ritorno sulla scena dei protagonisti della primavera di Praga. Aveva così inizio la «rivoluzione di velluto», ossia il rovesciamento del regime comunista in maniera non violenta e senza che quest'ultimo cercasse di reprimere la rivolta. Viceversa, in Romania, la fine della dittatura fu contraddistinta da gravi violenze, perpetrate soprattutto dall'esercito agli ordini del presidente Nicolae Ceausescu, che non voleva abbandonare il potere dopo oltre vent'anni di leadership. Il 25 dicembre 1989, Ceausescu fu catturato, processato e giustiziato a colpi di kalashnikov da un plotone d'esecuzione.

Si trattava di eventi che aprivano idealmente gli anni novanta, durante i quali ci sarebbe stata la fine del socialismo reale in Europa, la dissoluzione dell'Urss, la riunificazione della Germania, e soprattutto l'introduzione della democrazia rappresentativa e dell'economia di mercato in tutti i paesi dell'Est.

Queste vicende si ripercossero anche sugli assetti politici dei paesi occidentali, dando nuovo fiato alle istanze neoliberiste e accelerando le crisi identitarie dei partiti che si definivano comunisti, tra i quali il Pci. Il 12 novembre del 1989, il segretario Achille Occhetto annunciava la volontà di procedere con lo scioglimento del partito e di costruire una nuova compagine politica impegnata nelle lotte per il lavoro e collocabile a sinistra, ma priva di un cordone ombelicale con l'Urss, peraltro in disfacimento, e non appiattita sull'ideologia marxista.

In quella che passò alla storia come «svolta della Bolognina», perché il discorso fu pronunciato nell'omonimo quartiere popolare del capoluogo emiliano, furono poste le premesse per una riflessione – a tratti molto tormentata – che nel 1991, a Rimini, avrebbe portato il XX congresso del Pci a sancire la fine dell'esperienza comunista e la nascita del Partito Democratico della Sinistra (Pds), con segretario lo stesso Occhetto.

Una non trascurabile fetta di irriducibili non accettò la svolta e operò una scissione a sinistra, andando a costituire il Partito della Rifondazione Comunista (Prc) [...]. Di lì a un anno l'intero mondo politico italiano sarebbe stato sconvolto da uno scandalo giudiziario, e cioè l'inchiesta Mani Pulite, nota soprattutto come Tangentopoli, che chiuse idealmente la prima Repubblica, aprendo una nuova stagione di confronti e dibattiti (L'Italia degli anni '80).

SECTION 2.8.2-P. 463: THE ORIGINAL ITALIAN TEXT IN GARUCCIO, GUARINO, SELLERI 1980

La presa di coscienza di massa dei gravissimi problemi che lo sviluppo capitalistico genera man mano che avanza (sfruttamento del lavoro umano, creazione di enormi aree sottosviluppate accanto alle ricche metropoli, progressivo aggravarsi degli squilibri ecologici, cattiva qualità della vita di

grandi masse pur consumisticamente ricche rispetto ai popoli del terzo mondo...) ha fatto nascere visioni apocalittiche di un'umanità futura costretta a vivere in uno spazio sempre più ristretto ed in un ambiente gravemente inquinato. Corrispondentemente vi è stata una tendenza di certi strati intellettuali a spostarsi verso un idoleggiamento del passato precapitalistico o pretecnologico e ad accettare un atteggiamento di sfiducia verso qualsiasi forma di sviluppo o verso la lotta per aprire prospettive di autentico progresso civile e sociale. È evidente l'interesse dei gruppi dominanti, responsabili dei gravi problemi attuali, a seminare disorientamento e sfiducia nella possibilità di mutare la situazione esistente: non è dunque un caso che vengano proposte soluzioni basate fondamentalmente sull'idea di mantenere inalterata la situazione attuale con tutti i suoi privilegi per esigue minoranze (Garuccio, Guarino, Selleri 1980, pagg. 5-6).

SECTION 2.8.2-P. 464: THE ORIGINAL ITALIAN TEXT IN GARUCCIO, GUARINO, SELLERI 1980

[...] Una caratteristica fondamentale dell'intero rapporto, che deve subito essere segnalata, è il disinteresse per le contraddizioni interne dell'umanità, che viene studiata come qualcosa di fondamentalmente unitario.

[...] Il vizio di fondo dell'intero rapporto emerge poi anche nell'impostazione dei modelli matematici per la proiezione nel futuro dei diversi parametri mondiali.

[...] In parole povere il rapporto dell'M.I.T. ha preso in considerazione un mondo "americano" dal quale sono bandite le rivoluzioni: nessuna meraviglia allora che le proiezioni siano poco attendibili. Detto tutto ciò il rapporto un suo interesse lo presenta egualmente perché in sostanza ci mostra cosa accadrebbe se le forze dominanti riuscissero a imporre il loro modo di vivere al mondo intero ed a reprimere ogni forza di trasformazione.

[...] La risposta è drammatica: rapido esaurimento delle materie prime, enorme aumento dell'inquinamento, insufficiente aumento degli alimenti e quindi fame diffusissima nel mondo, aumento dell'industrializzazione solo fino al 2010 seguito da un rapido calo, ecc.

In breve, con ogni probabilità, la fine del mondo civile attorno all'anno 2000 con probabile sterminio della razza umana per la fame, per lo spaventoso inquinamento, eccetera (Garuccio, Guarino, Selleri 1980, pagg. 6-8).

SECTION 2.8.2-PP. 467-468: THE ORIGINAL ITALIAN TEXT IN FOOTNOTE 643

la recente installazione di missili a raggio intermedio in Europa e la presenza di sottomarini con armamento nucleare al largo delle coste USA, [fattori che hanno] ridotto notevolmente il tempo di volo di missili [strategici]. La possibile adozione del sistema 'lancio al primo allarme' (launch-on-warning), la presenza di circa seimila testate nucleari tattiche in Europa occidentale e la possibilità di una crisi politica acuta tra le due grandi potenze rendono non trascurabile l'eventualità di una guerra nucleare per errore. Si intende investigare questa possibilità effettuando un'analisi dei meccanismi decisionali in caso di crisi ed una analisi al computer, anche con tecniche Montecarlo, del tempo medio di risoluzione dei falsi allarmi e della possibilità che tale tempo superi il tempo di volo dei

missili strategici (AGAB-Progetti di ricerca-Richieste finanziamenti ricerca scientifica dip di fisica armadio D settore 22 palchetto 4 busta 1).

SECTION 2.8.2-PP. 469-470: THE ORIGINAL ITALIAN TEXT BY THE LETTER BY SELLERI TO BLANDINO

[...] Credo che andiamo poco d'accordo su Bush. Lungi da me provare simpatia per Saddam, che ha fatto 8 anni di guerra all'IRAN e poi ne ha rifatta una sanguinosissima contro 30 paesi, fra cui gli USA. Ma qui se si deve parlare di Hitler ce ne sono due. Gli aerei americani hanno massacrato la popolazione irachena, al di là delle chiacchiere propagandistiche sulla precisione chirurgica delle loro armi: su 30.000 tonnellate di bombe una piccola parte era dotata di sensori, calcolatori di bordo, etc., il grosso era fatto di bombe a gravità, come quelle della 2^a guerra mondiale e come quelle del Vietnam. **E hanno colpito i centri abitati volendoli colpire. E hanno fatto una strage di innocenti volendola fare. Questa è la civiltà occidentale? Grazie tanto.**

Da quando ho imparato a conoscerli là da loro gli americani mi hanno sempre spaventato. Qui ci si fa delle pie illusioni sul loro conto, la verità è che sono una nazione violenta a livello di massa e a livello di istituzioni (Selleri to Blandino, 3 April 1991-FSPA. Selleri underline, my bold).

SECTION 2.8.3-PP. 476-477: THE ORIGINAL ITALIAN TEXT BY THE LETTER BY SELLERI TO ALDO COSSU

Caro Cossu, per aiutarti a svolgere il delicato incarico che ti è stato affidato dalla Facoltà del 30 u.s. penso che possano servirti le informazioni seguenti delle quali, sono sicuro, saprai fare l'uso migliore perché sia generato nell'istituto di Fisica un clima nuovo.

1. Quando il prof. Preparata venne a Bari a ricoprire la cattedra di fisica teorica manifestò ben presto l'intenzione di chiudere le attività di ricerca, per lui prive di senso, in Fondamenti della MQ che io ed il mio piccolo gruppo conducevamo già da diversi anni.

L'atmosfera si fece particolarmente pesante sul finire del '77 quando circolava voce, diffusa ad arte, che Preparata fosse d'accordo con i vertici INFN per chiudere queste attività anche bloccando ogni forma di finanziamento. Normali metodi scientifici, come uno o due seminari interni sulle nostre attività, non sembravano dare risultati. Così ricorsi a quella che allora mi sembrava la sola speranza di salvare la nostra attività, scrivendo direttamente a Zichichi. La sua risposta fu molto incoraggiante e servì per qualche tempo a bloccare l'offensiva repressiva.

Indicativo è il modo con cui Preparata accolse questa notizia. L'episodio che segue mi è stato riferito [...]. In una stanza del secondo piano dell'Istituto di Fisica, Preparata stava parlando contro le ricerche del nostro gruppo quando qualcuno dei presenti gli disse che avevamo ricevuto una lettera di appoggio di Zichichi. Al che Preparata si bloccò di colpo ed esclamò: "No, questa Nino [Zichichi] non me la doveva fare!".

2. Nel 1978 un gruppo di laureandi di Pavia guidati da qualche docente va a Ginevra a visitare il CERN. Viene designato ad accoglierli il prof. Preparata, che era allora "staff member" di quel laboratorio.

Come parte dell'intrattenimento venne loro infilata una durissima tirata contro "quel pazzo di Selleri che vuole distruggere la MQ". Vi fu una reazione da parte dei pavesi. La reazione consisté nel fatto che il prof. Piazzoli, direttore della sezione INFN di Pavia, mi invitò a tenere un seminario in quell'Istituto con la esplicita motivazione di voler bilanciare l'opera distruttiva di Preparata nei miei confronti, mostrando ai laureandi che le nostre ricerche avevano una loro validità.

3. Negli ultimi concorsi a cattedra colui che avrebbe dovuto essere il mio principale supporter [...] fu eletto e sorteggiato nella commissione giudicatrice. Con mio grande sconforto ricevetti all'inizio dell'80 una serie di sue telefonate in cui mi diceva che Preparata andava facendo una "terribilmente convincente" opera distruttiva nei miei confronti. In sostanza [...] mi diceva di non contare più sul suo appoggio. Sono seguiti mesi di inferno, anche se la vicenda si è chiusa molto positivamente perché altri commissari del concorso mi hanno appoggiato al di là delle mie stesse attese. Tutti in Istituto sanno che il mio essere fra i vincitori del concorso è stata una sconfitta di Preparata e non mi sembra normale che così dovesse essere.

4. Il clima fra i teorici di Bari, creato da Preparata, era tale che erano possibili episodi di questo tipo: Era in corso una riunione dei teorici baresi, dalla quale io ero assente, quando si seppe l'esito dei concorsi a cattedra. Il più giovane fra essi [...] prese la parola per denunciare le scelte antiscientifiche fatte dalla commissione teorica nazionale, come, ad esempio, "l'aver dato una cattedra a Selleri". L'episodio mi è stato riferito [...].

5. Le richieste di fondi INFN per il 1981 del nostro gruppo non sono state presentate da Preparata al coordinamento nazionale dei teorici, con il risultato di danneggiare la nostra attività. Viaggi e seminari hanno avuto una sospensione di circa 6 mesi. La situazione è stata da me denunciata in una lettera dell'11 Marzo 1981 al presidente INFN. Solo nel giugno 1981 l'INFN ha potuto esaminare e finanziare le nostre ricerche. Esiste una contro lettera di Preparata che evade la sostanza del problema.

Questi sono i fatti principali. Ne esistono altri che riguardano il dottorato di ricerca e la nostra estromissione, ecc, nelle quali posso entrare se lo ritieni opportuno. Mi pare però che il quadro sia abbastanza netto. Molti cordiali saluti (Selleri to Aldo Cossu, Bari 1 October 1981. AGAB-Istituto di fisica e dipartimento di fisica-Personale Dip Fisica Armadio D settore 21 palchetto 9 busta 2)

SECTION 2.8.3-PP. 479-480: THE ORIGINAL ITALIAN TEXT BY THE LETTER BY SELLERI TO ALDO CINGOLANI

Penso che ti competa essere informato degli sviluppi delle ricerche sui FQM a Bari. Difficilmente infatti tu puoi avere già ora una informazione sufficiente, dati gli scarsi contatti scientifici che abbiamo avuto finora.

Vorrei cominciare col segnalarti due recenti lavori. Del primo ("Can an actual existence be granted to quantum waves?") ti ho fatto avere recentemente una copia.

[...] interessante può essere il fatto che A. Gozzini di Pisa sta montando un esperimento per controllare l'idea. Il secondo ("Generalized EPR-paradox") introduce un modo nuovo di dedurre le disuguaglianze da realtà e separabilità e permette di superare le precedenti difficoltà concettuali di

natura teorica. Penso che [...] sarà molto utile per l'analisi dell'esperimento di Catania che nacque su iniziativa barese e che sta per giungere alla raccolta-dati.

Sono profondamente persuaso del fatto che questi due lavori aprono linee di ricerca per molti versi nuove e che dovremo darci sotto con energia per svilupparne tutte le implicazioni. Ci attendiamo perciò un forte aumento di produttività scientifica nel prossimo anno.

Pure su nostra iniziativa è nata la conferenza di Perugia sul dualismo onda-particella: non abbiamo voluto apparire sul bando per ovvi motivi di natura barese. Coltiviamo d'altronde un'intensa attività internazionale come è mostrato dai seminari fatti nel tardo 1981 in vari centri europei e dal corso per laureati da me tenuto a Vienna. Queste attività di ricerca non sono disgiunte dalla produzione di libri: *The Debate about quantum theory* è un libro che uscirà prima in tedesco, tradotto dal prof. R. Sexl, cattedra di fisica teorica a Vienna, e poi probabilmente in inglese. *La natura dell'energia* sta per uscire in questi mesi in Italia. Un libro di testo con un'impostazione nuova della MQ è in preparazione per l'editore Reidel. Noterai [...] una sollecitazione a inviare lavori a *Foundations of Physics*: sollecitazioni di tal genere non sono per noi affatto insolite.

Queste attività hanno portato diversi riconoscimenti internazionali, i più importanti dei quali sono: un invito ad aderire alla New York Academy of Sciences, l'inclusione nelle 5° (e futura 6°) edizione di *Who Is Who In The World*, l'inclusione nel *Men Of Achievement*. Il nostro tipo di ricerche è peraltro in espansione come provato da fatti diversi dei quali ne ricorderò uno solo: fisici famosi cominciano a lavorare sui fondamenti. Tre esempi sono Marcel Froissart, considerato il miglior teorico francese; Peter Freund dell'Università di Chicago, uno dei fondatori delle simmetrie delle particelle elementari e Oreste Piccioni, scopritore del protone, dell'antiprotone ecc.

Ti manderò tre schede sull'attività degli altri ricercatori del gruppo (Cufaro, Fornari e Garuccio) sicuro che vi troverai informazioni interessanti (Selleri to Aldo Cingolani, 4 January 1982. My *Italics* and underline).

SECTION 2.8.3-P. 480: THE ORIGINAL ITALIAN TEXT BY THE LETTER BY SELLERI TO GIANNI CICUTA

Caro Gianni,

ti ringrazio del tempo che mi hai dedicato ieri e della franca discussione che abbiamo avuto. Te ne sono davvero grato, e non solo in modo formale, perché è raro che qualcuno abbia la voglia di dire a un altro delle "verità" sgradevoli in un'atmosfera non di scontro ma al contrario di dialogo pacato. So per esperienza quanto pesino certi silenzi e trovo simpatico che tu abbia voluto onestamente dirmi la tua opinione. Non ho la pretesa qui di riprodurre esattamente tutto quello che mi hai detto, ma di ricordare solo un punto che emergeva con chiarezza. Tanta, troppa gente della fisica teorica esprime critiche, o perplessità, o riserve sul tipo di ricerche che io ed il "mio" gruppetto portiamo avanti.

[...] La fisica è una scienza, non si può ragionare per sentito dire, qualunque sia l'autorità o la (presunta) competenza degli interlocutori. Dovresti forse capire che non avremmo potuto praticare questa attività per tanti anni senza avere riscontri logici, scientifici e di valore (nei nostri confronti).

Ci sono tutti stati e sono così importanti da bilanciare largamente le avversità che ci hanno accompagnato. Con molta franchezza, vorrei invitarti a documentarti o a tacere. Se ti vuoi documentare siamo qui davvero pronti a fornirti ogni spiegazione scritta e/o orale sulla fisica, ed ogni documentazione sugli elogi, talvolta imbarazzanti [che mettono in soggezione per quanto positivi, ndr], che riceviamo su riviste molto autorevoli e da personaggi molto conosciuti. Naturalmente è tuo pieno diritto di non documentarti, né io ho mai esercitato finora la minima pressione perché tu lo facessi. Ma se non ti documenti lascia perdere le chiacchiere di corridoio.

SECTION 2.8.4-P. 485: THE ORIGINAL ITALIAN TEXT BY (NUTRICATI 1998)

Nel 1978 Franco Selleri e Gino Tarozzi hanno proposto un'interpretazione realista del fenomeno onda-corpuscolo della luce ("Is Nondistributivity for Microsystems Empirically Founded?"). Entrambi [...] negano una concezione unicamente corpuscolare della radiazione elettromagnetica. [...] la presenza o l'assenza dell'interferenza è spiegata dall'associazione dell'onda quantistica, non perturbata, al corpuscolo.

[According to Tarozzi] "Sulla base di questa interpretazione risultava possibile fornire una spiegazione fisica di quelle situazioni che danno origine ad uno dei più gravi paradossi della teoria della misurazione, [...] in cui abbiamo un solo strumento inserito dietro una delle fenditure e consideriamo i casi in cui esso non rivela il passaggio di alcun micro oggetto e viene tuttavia registrato l'arrivo di una particella sul secondo schermo. In questo caso, sulla base della nostra interpretazione, la distribuzione dell'interferenza può essere spiegata non come conseguenza delle nostre conoscenze relative al comportamento del micro oggetto, acquisite non osservando il verificarsi di un certo evento fisico ma come risultato dell'interazione dell'onda quantistica con lo strumento misuratore" (Nutricati 1998, pagg. 67-68. *Italics* in the original text. My underline).

SECTION 2.8.4-P. 494: THE ORIGINAL ITALIAN TEXT BY SELLERI FSPA

una sostanziale modifica dell'esperimento, dove, pur mantenendo l'idea base di verificare la doppia natura corpuscolare-ondulatoria dei fotoni mediante tecniche di coincidenza, i due laser delle precedenti versioni sono sostituiti con una sorgente di luce incoerente e da un amplificatore laser. La teoria di de Broglie (a differenza della meccanica quantistica) ovviamente non contiene alcun postulato sulla riduzione del pacchetto d'onda.

L'ammettere o no tale postulato porta a previsioni conflittuali nello schema sperimentale proposto e sembra quindi possibile una discriminazione empirica fra le due teorie (FSPA).

SECTION 2.8.4-P. 494: THE ORIGINAL ITALIAN TEXT BY SELLERI FSPA

hanno suscitato un ampio dibattito ed un vivo interesse non solo tra gli studiosi teorici dei fondamenti della meccanica quantistica, ma anche tra gli sperimentali dell'ottica quantistica. Questo interesse si è concentrato su tre punti: la descrizione quantomeccanica di un amplificatore di fotoni, il possibile risultato dell'esperimento proposto e la sua interpretazione alla luce della elettrodinamica quantistica, il comportamento dei fotoni su uno specchio semiriflettente (FSPA).

SECTION 2.8.4-P. 515: THE ORIGINAL ITALIAN TEXT BY SELLERI FSPA

Lo [schema di interferometria neutronica per la rivelazione dell'onda di de Broglie] è stato ripreso da Garuccio in un lavoro del 1990 con due fisici portoghesi (Croca e Moreira) e con Lepore ma con una sorgente di coppie di fotoni emessi in un processo di dissociazione parametrica. L'esperimento così proposto è stato eseguito in una prima versione da Wang, Zou e Mandel che hanno creduto di poter trarre conclusioni negative circa l'onda di de Broglie. L'adeguatezza dell'esperimento è stata però criticata, in alcuni suoi aspetti essenziali, in un successivo articolo della stessa collaborazione italo-portoghese, che ha potuto mostrare che i risultati erano in realtà compatibili con l'esistenza delle onde di de Broglie. Queste critiche hanno dato corso ad una nuova esecuzione dell'esperimento da parte di Wang, Zou e Mandel. I risultati di questi esperimenti sono oggetto attualmente di un'attenta analisi sia per quanto riguarda gli aspetti più propriamente sperimentali, che per quanto riguarda le implicazioni teoriche. Allo stato attuale delle ricerche sembra che soltanto ammettendo una probabilità variabile di rivelazione per i singoli fotoni sia possibile riprodurre nell'approccio di de Broglie i risultati dell'esperimento: peraltro la probabilità di rivelazione è proprio l'idea fondamentale che tutti i realisti locali italiani portano avanti in questo settore. Va anche notato che questo esperimento è il primo effettuato, dalla formulazione del dualismo, per verificare la validità delle idee del fisico francese (FSPA).

SECTION 2.8.4-P. 519: THE ORIGINAL ITALIAN TEXT BY THE CORRESPONDENCE BETWEEN SELLERI AND ADRIANO GOZZINI

La esperienza che proponi è eccitante. [...] non pensavo alla possibilità dell' "onda fantasma" di indurre l'emissione stimolata. Sarebbe veramente eccitante un risultato positivo di questo esperimento. Farlo è immediato con il dispositivo già usato da Martinolli.

Quindi lo faremo. Stiamo rifacendo queste esperienze in modo più pulito. La difficoltà sperimentale sta soprattutto nel fatto che l'amplificatore [...] costituisce una sorgente che produce un fondo.

Bisogna evitarlo, cioè sostituire la lampada con un laser e porre i portatubi a una certa distanza molto grande in modo da ricevere solo la luce di interesse ed evitare quella diffusa. [...] Ne parleremo quando verrai a Pisa (Gozzini to Selleri, 1 July 1981-FSPA).

SECTION 2.8.4-P. 519: THE ORIGINAL ITALIAN TEXT BY THE CORRESPONDENCE BETWEEN SELLERI AND ADRIANO GOZZINI

Ora [il laser] funziona e stiamo facendo i preparativi per la prova. Come al solito non mancano tante piccole cose non previste che rallentano il lavoro. Quanto all'amplificatore, penso che se funzionerà (e deve funzionare) sarà più difficile farlo funzionare col semplice raddoppio che con una moltiplicazione più elevata (Gozzini to Selleri, 7 February 1982-FSPA).

SECTION 2.8.4-P. 520: THE ORIGINAL ITALIAN TEXT BY THE CORRESPONDENCE BETWEEN SELLERI AND ADRIANO GOZZINI

Non sai quanto piacere mi fa il tuo amplificatore e la fiducia che mostri nel suo funzionamento. Mi sembra chiaro che se davvero funzionerà si faranno passi avanti essenziali, non solo con le onde

fantasma. Nick Herbert, che ha sottolineato di più fra i teorici l'importantissimo ruolo dell'amplificatore laser, ha bisogno di un'iniezione di fiducia sperimentale, come risulta dalle lettere che mi scrive: fra l'altro non riesce a fare accettare il suo lavoro. Anche per questo ti proponevo di scrivere qualcosa. Ma va bene di aspettare che l'amplificatore funzioni (Selleri to Gozzini, 20 February 1982-FSPA).

SECTION 2.8.4-P. 520: THE ORIGINAL ITALIAN TEXT BY THE CORRESPONDENCE BETWEEN SELLERI AND ADRIANO GOZZINI

[...] l'esperimento appare molto complicato a causa delle differenze della potenza del laser di pompa tra ogni impulso. penso che non otterranno molto con il laser pulsato...ho riesaminato tutta la faccenda e ho visto che l'esperimento è molto più facile usando un laser in continuo e ho calcolato nel dettaglio l'esperimento (Gozzini to Selleri, 17 November 1982-FSPA).

[...] Contributo [...] inviato, con molto ritardo, dovuto al desiderio di metterci qualche risultato sperimentale. Non ci sono riuscito e così ho inviato quanto allegato. Non è grande cosa ma mostro che l'esperimento può farsi, e non appare molto difficile usando un laser in continuo. L'idea di usare il laser pulsato è stata un'idea sbagliata perché la potenza varia troppo da impulso a impulso; la frequenza di ripetizione degli impulsi è troppo bassa ed inoltre il laser dei fiorentini non va molto bene (occorre cambiare il gas molto spesso). Penso che col laser di Catania l'esperimento possa farsi. Ho letto un articolo [...] che afferma impossibile l'amplificazione senza rumore a causa delle emissioni spontanee. Certamente è impossibile, il problema è quello di rendere il rumore piccolo in modo da rendere possibile l'esperimento o gli esperimenti che suggerisci (Gozzini to Selleri, 17 January 1983-FSPA).

SECTION 2.8.4-P. 521: THE ORIGINAL ITALIAN TEXT BY THE CORRESPONDENCE BETWEEN SELLERI AND VITTORIO DEGIORGIO AND RAFFAELE GIOVANNELLI

Non ho trovato né negli articoli di Selleri né nelle pagine lasciatemi da Giovannelli una discussione quantitativa dell'esperimento proposto con il laser impulsato. Prima di discutere molti dettagli tecnici vorrei essere sicuro di aver capito lo scopo dell'esperimento.

Ragionando su di una situazione ideale in cui siano trascurabili gli effetti dell'emissione spontanea dell'amplificatore e il rumore dei rivelatori [...] trovo una probabilità di coincidenza tra i rivelatori pari a $4,5 \times 10^{-3}$.

Le domande che vi pongo sono:

- 1) la vostra previsione è che la probabilità di trovare una coincidenza nell'esperimento ideale sia diversa da quella che ho calcolato sopra?
- 2) se la risposta alla domanda 1 è affermativa, qual è la vostra previsione? O nel caso non siate in grado di formulare una previsione quantitativa, qual è l'accuratezza che richiedete all'esperimento?
- 3) perché occorrono impulsi con durata inferiore al picosecondo per fare l'esperimento?

[...] Sperando di poter presto combinare un incontro [...] (Degiorgio to Giovannelli and Selleri, 17 April 1985-FSPA)

SECTION 2.8.4-P. 522: THE ORIGINAL ITALIAN TEXT BY THE CORRESPONDENCE BETWEEN SELLERI AND VITTORIO DEGIORGIO AND RAFFAELE GIOVANELLI

Caro Degiorgio, nel tuo conticino non mi va bene il fatto che l'amplificatore non gioca alcun ruolo, mentre invece nella nostra idea è essenziale per permettere di rivelare l'onda vuota. Ti accludo un calcoletto fatto alcuni mesi fa che permette di calcolare il numero medio di fotoni $\varphi(x)$ generati da un fotone singolo in un amplificatore di spessore x , $\varphi(x) = \exp(Px)$

dove P è un parametro che si misura appunto misurando $\varphi(x)$.

Similmente si può calcolare il numero medio di fotoni $\varphi(x)$ generati da un'onda vuota: il risultato...contiene il parametro P_0 che potremmo medioevalmente chiamare "virtù stimolatrice" dell'onda vuota. La stima numerica data è forse troppo pessimistica: $P_0 = P/2$ è forse meglio, almeno come stima semiclassica.

Per quanto riguarda il picosecondo credo che Giovannelli l'abbia introdotto per motivi di massima efficienza: anche il nanosecondo può andare bene, secondo me. L'incontro possiamo farlo quando vuoi, con grande mio piacere (Selleri to Degiorgio, 29 April 1985-FSPA)

SECTION 2.8.4-P. 522: THE ORIGINAL ITALIAN TEXT BY THE CORRESPONDENCE BETWEEN SELLERI AND VITTORIO DEGIORGIO AND RAFFAELE GIOVANELLI

Attraverso il tuo calcolo ho cominciato a capire il tuo punto di vista. Che esperimento sta conducendo Gozzini sulle onde di vuoto? Che cos'è la stima semiclassica di P_0 ?

Per completare l'analisi dell'esperimento ideale mi sembra occorra calcolare cosa succede quando l'impulso contiene due fotoni: se il beam splitter ne manda uno a destra e uno a sinistra, sull'amplificatore arrivano contemporaneamente un fotone e un'onda di vuoto.

Quest'onda di vuoto che viaggia assieme al fotone contribuisce, secondo te, all'emissione stimolata? (Degiorgio to Selleri, 5 June 1985-FSPA)

SECTION 2.8.4-P. 523: THE ORIGINAL ITALIAN TEXT BY THE CORRESPONDENCE BETWEEN SELLERI AND VITTORIO DEGIORGIO AND RAFFAELE GIOVANELLI

Vi accludo il calcolo fatto a partire dalla Master Equation che comprende anche l'effetto dell'emissione spontanea. Dovrebbe essere il risultato definitivo, a meno di una riserva legata al fatto che la ME è ricavata in un modello a modo singolo (ci devo riflettere un poco). Non ho fatto in tempo a ricavare i grafici che non dovrebbero riservare grosse sorprese (Degiorgio to Giovannelli and Selleri, 13 February 1986-FSPA).

SECTION 2.8.5.1-P.533: THE ORIGINAL ITALIAN TEXT BY 1974 FRASCATI CONFERENCE BILLBOARD

In Italia un numero crescente di ricercatori s'interessa di FQM. È nata perciò la duplice esigenza di confrontare le iniziative intraprese e di introdurre a questa tematica coloro che se ne avvicinano ora".

Relatori:

Agodi-Baracca-Beltrametti(?)-Brigaglia-Di Toro-De Marzo-Faraci-Fortunato

-Galgani-Garuccio-Ghirardi-Notarrigo-Rapisarda(?)-Recami-Ruggiero

-Salandin-Selleri-Tonietti

Tematiche:

- seminari introduttivi:
- MQ e realtà obiettiva: EPR, teoria della misura, vettori di stato di secondo tipo; dualismo onda-corpuscolo
- Il problema della causalità: problema delle variabili nascoste, teorema di von Neumann e suo superamento; le idee di Bohm, ddB
- Processi stocastici: interpretazione stocastica principio di indeterminazione e equazione di Schrödinger da Fürth a Nelson, energia di punto zero e distribuzione di Planck.
- questioni fondamentali
- causalità e ddB: variabili nascoste locali ed osservabili non dicotomiche, osservabili sensibili ed indifferenti, metodi matriciali in tvn locali, sulla sufficienza delle ddB
- questioni sperimentali: esperimenti sui FQM
- MQ e didattica

SECTION 2.8.5.1-P. 534-FOOTNOTE 725: THE WHOLE NUMBER OF SCHOLARS WHO PARTICIPATED TO CONFERENCES ORGANIZED BY Selleri

-Perugia 1982 (*The wave particle dualism*): G. Lochak, P.W. Milonni, H. Rauch, A. Steyerl, F. Selleri, A. Gozzini, G. Tarozzi, D. Fargue, P. Lochak, P. Caldirola, S. Diner, M.V. Berry, L.S. Schulman, V. Aquilanti, M. Roos,

L. Accardi, P. Suppes, M. Zanotti, F. Fer, M.D. Srinivas, A. Aspect, J. Six, F. Falciglia, L. Fornari, A. Garuccio, G. Iaci, L. Pappalardo, S. Bergia, D. Aerts, C. W. Rietdijk, P. Pearle, O. Costa de Beauregard, S.P. Gudder, W.H. Zurek, M. Boiteux, H.P. Noyes.

-Bari 1983 (*Open questions in quantum physics*): D. Aerts, T.D. Angelidis, J. Andrade e Silva, E. Bitsakis, M. Cini, N. Cufaro Petroni, A.B. Datzeff, A. Garuccio, Ph. Guéret, B. J. Hiley, K. Kraus, L. Mandel, T. W. Marshall, K. R. Popper, O. Piccioni, R. Prosser, H. Rauch, W. Rietdijk, M. C. Robinson, E. Santos, F. Selleri, J. Six, G. Tarozzi, J. P. Vigier.

-Urbino 1985 (*Microphysical reality and quantum formalism*): M. Namiki, D.M. Greenberger, A. Yasin, K.E. Eriksson, E.I. Bitsakis, P.T. Landsberg, D. Home, S. Bergia, F. Cannata, L. Vaidam, J. Hilgevoord, J.B.M. Uffink, E.E. Entralgo, V.V. Kuryshkin, Yu. I. Zaparovanny, Y. Ne'eman, G.C. Ghirardi, C.W. Kilmister, R.K. Varma, A.B. Datzeff, F. Ghaboussi, J.A. Wheeler, O. Costa de Beauregard, J. Rayski, C.W. Rietdijk, A. Sudbery, W. De Baere, M. Conrad, D. Home, B. Josephson, P. Jasselette, A. Avramescio, J. Fennema, T.W. Marshall, A.O. Barut, F. Selleri, S. Welin, O. Piccioni, W. Mehlhop, D. Aerts, D. Bedford, K.R. Popper, W. De Baere, J. Rosen, J.P. Wesley, T.A. Brody, T.D. Angelidis, D. Bohm, C. Dewdney, Ya. P. Terletsky, B. Kanitscheider, V. Buonomano, A. Kyprianidis, P.R. Holland, Y. Ben-Menachem, A. Peres, A. Ron, M. Redei, B. Flanagan, H. Lichte, T. Hellmuth, A.G. Zajonc, H. Walther,

H. Rauch, R. Giovanelli, Y. Koh, T. Sasaki, J.P. Vigiér, G. Grössing, M.C. Robinson, J. Scheer, M. Schmidt, Y. Cantelaube, J.R. Croca, L. Mackinnon, C. Antonopoulos, E. Santos, N.D. Mermin, A.J. Duncan, M.H. Trxier, H.P. Stapp, A. Garuccio, J.L. Sanchez-Gómez, M.A. Horne, A. Zeilinger, S. Pascazio, P. Roussel, D. Gutkowski, G. Scalera, A. Bette, F. Bonsack.

-Cesena 1991 (*Bell's theorem and the foundations of modern physics*): F. Barone, A.O. Barut, S. Bergia, E. Beltrametti, R.A. Bertlmann, J.T. Cushing, B. d'Espagnat, P.K. Feyerabend, G.C. Ghirardi, D.M. Greenberger, D. Home, M. Jammer, H. Kleinpoppen, L. Kostro, L. Mandel, O. Piccioni, E. Santos, A. Zeilinger, E.I. Bitsakis, C. Dewdney, F. Selleri, G. Tarozzi, H.R. Brown, A. Elby, V. De Alfaro, A.I.M. Rae, J.L. Sanchez-Gomez, E. Scheibe, M. Simpson, E.J. Squires, M. Adelman, R. Boudet, J. Bub, M. Czachor, J.V. Corbett, F. Cannata, A.J. Duncan, Z.A. Sheikh, G. Fleming, J. Butterfield, A. Frenkel, F. Károlyházy, P. Frodl, A. Garuccio, P. Ghose, M.A. Horne, H.J. Bernstein, S. Gudder, M.J.W. Hall, A.A. Hnilo, M. Hoffmann, D. Howard, P.N. Kaloyerou, V.L. Lepore.

-Trani 1992 (*Waves and particles in light and matter*): O. Costa de Beauregard, A.O. Barut, P.H. Eberhard, M. Ferrero, A. Garuccio, F. Hasselbach, V.K. Ignatovich, S.J. Prokhovnik, H. Rauch, E.J. Squires, A.M. Awobode, V.I. Balykin, H.P. Boehm, M Božić, L. Chiatti, P. Cornille, J.R. Croca, J.T. Cushing, C. Dewdney, G. Diambrini-Palazzi, M.W. Evans, Y. Hasegawa, D. Home, S. Jeffers, A. Julg, L. Kostro, A.F. Kraklauer, T. Krüger, M.J. Kubiak, V.L. Lepore, R.N. Moreira, G. Nimtz, M. Pardy, J. Pykacz, E.M. Rasel, F. Selleri, T.P. Spiller, T.B. Andrews, D. Bedford, I. Diósi, G. Dorda, O. Hara, A.J. Hatch, W.M. Honig, M. Mugur-Schächter, R. Nakhmanson, J. Reignier, A. Rimini, M. Rivas, J. Salmon.

-Olympia 1993 (*Frontiers of fundamental physics*): H. Arp, W.M. Napier, J.W. Sulentic, M. Zabierowski, R.M. Santilli, E.J. Sternglass, J.B. Miller, S. Bergia, P.F. Browne, T.B. Andrews, A. Paparodopoulos, M. Zabierowski, S.V.M. Clube, H. Yilmaz, C.O. Alley, D.F. Roscoe, A. Tartaglia, A.K. Aringazin, C.I. Mocanu, H.E. Wilhelm, F. Selleri, L. Kostro, T. Morris, A. Martin, J. Czerniawski, H. McCouat, S. Warren Carey, G. Scalera, S.T. Tassos, K. Vogel, C. Strutinski, J.K. Davidson, J. Koziar, L.G. Collins, C. Warren Hunt, S. Ciechanowicz, M. Kokus, A. Jannussis, M. Barone, B. Średniawa, V. Kadyshevsky, M. Rivas, E. Kapuścik, G. Dorda, R.K. Varma, A.K.T. Assis, L.A. Kaliambos, W.M. Honig, G.S. Pogosyan, S. Klein, A. Horzela, C. Wolf, P. Gräff, E.J. Squires, E.I. Bitsakis, J.Barretto Bastos Filho, V.K. Ignatovich, M Božić, Y. Mizobuchi, S.F. Huelga, A. Garuccio.

-Athens 1997 (*Relativistic physics and some of its applications*): P.Fleming

F. Goy, A. Horzela, A. G. Kelly, J. Lévy, A.Martin, J.Ramalho Croca, L. Kostro, R. Risco-Delgado, F. Selleri, T. Van Flandern, M. Barone, J.Barretto Bastos Filho, E. Bitsakis, A. Jannussis, N.A. Tambakis, G. Tarozzi, A. A.Tyapkin, A.K.T. Assis, J.I. Cisneros, A.M. Awobode, V. S. Barashenkov, M Božić, H.Broberg, W. A. Hofer, E. Kapuscik, M. Pawlowski, J. P. Wesley, H. Arp, J. Brandes, Z. Jacyna-Onyszkiewicz, B.Lange, D. F. Roscoe, A.P. Bredimas, A.Garuccio, L. Gonzalez-Mestres, S. Baskoutas, J.L. Sánchez-Gómez, C. H Thompson, M.A.B. Whitaker.

SECTIONN 2.8.5.2-PP. 546-547: THE ORIGINAL ITALIAN TEXT BY THE CORRESPONDENCE FROM REGGE TO SELLERI

Ti scrivo informalmente dopo aver letto la proposta di Popper nel tuo rapporto “questioni aperte ecc”. L’idea di P. mi trova completamente dissenziente e non per la sua contrapposizione ad Heisenberg, non sono un fanatico della MQ e seguo le polemiche e gli sviluppi sperando sempre che qualcosa rinnovi o mandi a carte 48 la scuola di Copenhagen aprendo nuovi orizzonti. Ritengo invece che nella discussione si assuma, infondatamente, la natura classica dell’emettitore. Questo è pur sempre soggetto all’incertezza [...]

[...] Non vedo dunque un principio di incertezza violato in misure indirette bensì una dichiarazione diretta sull’emettitore. Non credo dunque in Popper e non accetto le sue proposte o conclusioni. D’altra parte non mi fido dei filosofi (Tullio Regge to Selleri, 14 December 1983. FSPA. Underline in the original text).

SECTION 2.8.5.2-P. 547: THE ORIGINAL ITALIAN TEXT BY THE CORRESPONDENCE FROM SELLERI TO REGGE

[...] l’esperimento di Popper è in linea di principio possibile: non sempre i filosofi dicono bestialità. Restano difficoltà pratiche per trovare la sorgente adatta. [...] permettimi di ricordarti che negli ultimi tempi qui a Bari abbiamo ottenuto risultati sul dualismo e sul teorema di Einstein, Podolsky e Rosen che a me sembrano ancora più netti e importanti di quelli di Popper. In particolare sappiamo perché gli esperimenti tipo Aspect non permettono di concludere contro la realtà separabile (Selleri to Regge, 20 January 1984. FSPA. My underline).

SECTION 2.8.5.3-P. 553-FOOTNOTE 742:

TABLE OF CONTENTS-QUANTUM PARADOXES AND PHYSICAL REALITY

1 / Quantum Theorists and the Physical World.

1.1. Three Central Questions about Physics

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1.3. The Middle Generation

1.4. The Younger Generation

1.5. Conclusions

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2.1. The Problem of Completeness and of Hidden Variables

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2.3. The Spin-1/2 System in Quantum Mechanics

2.4. A Simple Proof of von Neumann's Theorem

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- 3.11. How to do what Complementarity Forbids

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- 4.2. Experiments on the Nature of Duality
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- 4.4. Quantitative Empty Wave Amplification
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- 5.3. Two Types of State Vectors
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- 5.6. The EPR Paradox: Quantum Mechanics Complete
- 5.7. The EPR Paradox: Quantum Mechanics not Complete
- 5.8. From Theory to Practice
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- 6.3. New Proof of Bell's Inequality
- 6.4. Probabilities for Pairs of Correlated Systems
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- 6.6. All the Inequalities of Einstein Locality
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- 6.8. On the Possibility of New Experiments
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7 / Perspectives of Physical Realism

- 7.1. Objectivity of Scientific Knowledge
- 7.2. Mathematics and Reality
- 7.3. The Role of History of Physics
- 7.4. Fragmentation of Modern Physics
- 7.5. Niels Bohr and Philosophy
- 7.6. Quantum Physics and Biological Sciences
- 7.7. Forms of Physical Realism

SECTION 2.8.5.4-P. 559: THE ORIGINAL ITALIAN TEXT OF THE LETTER BY SELLERI TO BARTOCCI

Ora sto scrivendo un libro sulla rivoluzione copernicana per Laterza. Ho studiato molto e sono impressionato dall'intensità della reazione clericale al Rinascimento. Filosofi e scienziati sono stati inquisiti, interrogati, censurati, processati, condannati, imprigionati, torturati, giustiziati.

Non sapevo che il fenomeno fosse così vasto, di solito si parla solo di Galileo e di Bruno! A mio parere il vero martire fu Campanella. Vero è che Bruno fu bruciato vivo e lui no, ma il numero, la durata e il tipo di torture da lui subite supera ogni immaginazione. Tanto che si convertì (in buona fede) alla verità cattolica come conseguenza delle torture! Nessun libro liceale le dice queste cose, ma ci sono 2 Campanella, quello prima e quello dopo le torture: persino La Città del Sole è posteriore. Che il 2° sia un po' stronzo e reazionario non dovrebbe proprio stupire, visto il trattamento. È il 1° quello che conta (Selleri to Umberto Bartocci-FSPA).

SECTION 2.8.5.4-PP. 559-560: THE ORIGINAL ITALIAN TEXT OF THE LETTER BY SELLERI TO TRIGIANTE

Ecco allora un pò di bibliografia:

- E. Garin, *L'umanesimo italiano*, Laterza (1990)
- E. Garin, *Medioevo e Rinascimento*, Laterza (1984)
- E. Garin, *Scienza e vita civile nel Rinascimento italiano*, Laterza (1985)
- E. Garin, *Lo zodiaco della vita*, Laterza (1982)

P. Redondi, *Galileo eretico*, Einaudi (1988)
M. Ciliberto, *Giordano Bruno*, Laterza (1990)
G. La Porta, *Giordano Bruno*, Bompiani (1988)
S.F. Mason, *Storia delle scienze della natura*, Feltrinelli
E. Namer, *La vie et l'oeuvre de J.C. Vanini*

Le informazioni sui “repressi” da Santa Madre Chiesa sono sparse su una vasta letteratura in gran parte ignota anche a me. Anzi, le sarei comunque grato per informazioni addizionali non contenute nel mio quaderno (Selleri to Rocco Trigiante, 29 November 1996-FSPA).

SECTION 2.8.5.4-P. 560: THE ORIGINAL ITALIAN TEXT OF THE LETTER BY SELLERI ON RIMINI MEETING

[...] non va bene. [...] ho partecipato [...] alla tavola rotonda sostanzialmente dedicata ad Halton Arp, nome che i posteri ricorderanno più di quello di ogni altro astrofisico del nostro secolo. Lo spirito di confronto fra posizioni culturali e filosofiche diverse che esiste al Meeting, mi sembra progressivamente oscurato da una strumentalizzazione politica sempre più marcata, e non tanto per quello che si vuole valorizzare in positivo, essendo per fortuna libero ognuno di scegliere gli amici che vuole, quanto per le chiusure e gli anatemi che vengono lanciati su personaggi, partiti, e categorie. Che il più grande movimento giovanile di massa dei nostri giorni si presti a tanto è certo un segno della grave crisi politica che attraversiamo [...] (FSPA).

SECTION 2.8.5.4-P. 562: THE ORIGINAL ITALIAN TEXT OF THE LETTER BY POLIDORO TO SELLERI

Vorrei ringraziarla anche a nome del CICAP per avere citato il comitato nella prefazione all’ottimo libro di Hy Ruchlis. C’è sicuramente molto bisogno nel nostro paese di contributi critici come questo, specie se rivolti specificamente ai ragazzi.

[...] ci farebbe molto piacere, se possibile, ospitare un suo intervento sulle pagine di *Scienza & Paranormale* (Massimo Polidoro to Selleri, 16 February 1998-FSPA. *My Italics*).

SECTION 2.8.5.4-P. 562: THE ORIGINAL ITALIAN TEXT OF THE LETTER BY SELLERI TO POLIDORO

La scoperta di questo autore e la decisione di tradurlo in italiano è merito della Casa Editrice Dedalo che ha insolite inclinazioni razionalistiche.

Sarò bel lieto di contribuire con un articolo. Penserei di preparare qualcosa di nuovo, ad esempio una rassegna/commento della denuncia delle attività paranormali fatta da Gerald Holton, uno dei massimi epistemologi viventi, profondo conoscitore ed estimatore dell’opera di Einstein. È talmente raro che un vero filosofo della scienza si occupi di queste cose che le idee di Holton meritano, secondo me, di essere portate all’attenzione dei lettori (Selleri to Massimo Polidoro, 24 February 1998-FSPA).

SECTION 2.8.5.5-PP. 563-571: THE ORIGINAL ITALIAN TEXT OF THE WHOLE EXCERPTS FROM SELLERI'S TRAVEL NOTEBOOK 1989-1990

12/04 a Milano alla Jaca Book

Jaca Book: confronto su lavori di avanzamento del libro di Arp

Selleri deve dare risposta operativa perché Bagnoli [della Jaca Book] sembrava davvero interessato per ristampe 4 lavori: FQM, relatività speciale, universo senza big bang, terra in espansione. Anche per libro con Guaragnella. Anche per piccoli libri, 4 o 5, su punto della fisica, 60 cartelle di grande respiro, come le prolusioni al College de France. Per far appassionare a un campo. Si vuole farli in più lingue.

13/04 a Vienna da Thirring & C.

Institut für Theoretische Physik.

Seminario "Present status of the epr paradox".

*Circa 30 persone, varie domande anche abbastanza lontane dal tema. **Mi do 6 come voto perché il seminario è stato poco preparato e poco meditato e poteva riuscire meglio***

Thirring

Mi ha dato un tavolo per il poco tempo che sono stato qui.

Non si è parlato di rimborsi per me.

Flamm, Domande provocatorie ma fuori centro (al solito). Lavora sui fondamenti della m.q a modo suo. Dice che è difficile lavorare sui FQM a Vienna. Nei suoi lavori non mi cita.

Bertlmann:

Dice che lui fa tante cose: cromodinamica, le cose di mat di Ciulli ecc e che i FMQ sono solo un suo interesse minore (si vede dal lavoro, penso io). Dice che lui crede che le ipotesi addizionali siano vere. Qui forse ho modificato un po' le sue idee.

Visitata mostra dedicata ad Hans Thirring, nato 101 anni fa. Molto interessante il libro "AntiNietzsche, AntiSpengler" pubblicato nel 1947 e, dice WT [Walter Thirring], scritto a Vienna durante la guerra. È però un libro in tedesco.

14/04 a Vienna da Ranch

All'Atominstitut, seminario con 14 persone dall'aria generalmente poco convinta. Lo stesso Rauch alla fine mostrava di non aver ben afferrato lo spirito della cosa.

Il seminario era stato voluto di venerdì da Rauch? Sì!

Si noti la differenza fra il seminario da Rauch di anni fa (grande aula, tanta gente) e questo: solo dovuta allo sgarbo?

Messaggio di Rauch: i suoi nuovi punti per assorbimento deterministico/assorbimento stocastico sono molto impressionanti perché "8 neutroni sono in grado di modificare osservabilmente il comportamento di 50 neutroni". Se fossero corpuscoli questo non sarebbe possibile!

17/04 a Monaco (Max Planck)

*Seminario: 35-40 persone generalmente attente. Molte domande, durante e dopo. **Ho azzeccato tono giusto: nessuna mia polemica, inferiorità paradigmatica di EdB confessata, attenta e calma risposta ad ogni loro domanda. Molto meglio che a Vienna. Lo spirito giusto è di mettersi nei loro panni e pensare a questo "oggetto strano" che gli viene a parlare di EdB. Perciò bisogna quasi simpatizzare con le loro contestazioni e rispondere con calma e con logica. Evitare l'errore di parlare polemicamente contro Bohr e/o Heisenberg. Solo, c'è la possibilità logica di EdB e va indagata.***

Tutto sommato sono contento di come le cose sono andate, anche se bisognerà vedere quanta strada faranno dentro di loro (nessuna?)

In due hanno detto che il seminario era "very interesting". Paragonando con Dortmund (Vienna?) è chiaro che qui che sei bravo non te lo dicono (pensano che lo sai già da te). Questo rivaluta la cosa. È molto probabile un forte impatto positivo anche a München.

18 aprile a Monaco (Max-Planck)

Ho scritto a Gondesén:

propostagli traduzione tedesca di Che cos'è l'Energia. Ormai 20 mila copie vendute. Riscritto per 3° edizione, nuove figure. Forse Dürr potrebbe fare presentazione di questo libro in RFT (*ripensandoci, meglio Bonse).*

Prof. Hans-Peter Dürr, dirige il Max Planck. Interesse per problemi globali molto vivo, secondo K.E.E. Al seminario corretto nelle domande, con qualche vena protettiva verso gli altri da idee tipo EdB.

Dr. Ivo Derado, mi ha fatto invitare ed ha curato bene tutti gli aspetti concreti (pubblicità al seminario anche individuale, rimborso di 450 DM, foresteria, auto per andare a Garching, ecc). Sottintese o mezze espresse sue riserve circa mia attività.

Pomeriggio/sera da Arp.

Conosciuta la moglie alta, asciutta, francese, simpatica e la figlioletta (5 anni, Delphine, detta Delina).

Vuole che Bertola corregga il suo libro nella traduzione italiana. OK

Vuole presentazione di Bertola, o forse di Maffei. Decida lui.

Mi ha dato copia articolo su Nature

Verrà a Modena. [Dibattito del 23/05. Vedi 12 Aprile]

Verrà a Rimini.

19/04 a Tübingen

Möllenstedt non c'è (è stato operato 8 settimane fa e sta recuperando). Lichte si occupa di tutto, preciso, efficiente, antipatico. Mi dà 250 DM di rimborso. 20-25 persone al seminario, silenziosi, diversamente da München.

Prof. F. Lenz, ha fatto belle lezioni su lunghezza di coerenza. al seminario un po' ostile con in più il bel commento che non bisogna prima avere idee poi cercare esperimenti, ma aspettare che una teoria vada in crisi da sola prima di discuterla!

Dr. Franz Hasselbach ha avuto il coraggio di venirsi a congratulare per il seminario e a chiedere che gli spedisca dei lavori (anche su EPR). Simpatico.

Thomas Lenthner giovane laureato vuole saperne di più circa EPR

Dr. M. Nicklaus, giovane sperimentale in gamba che lavora all'effetto Sagnac per elettroni (non ancora visto, dice Lichte) mi ha fatto vedere il formarsi delle frange d'interferenza, magnifico. Mi ha dato lavori. Suo capo è Hasselbach.

21/4/89 in treno verso Dortmund

Lezioni apprese:

- 1) molta gente resta sbalordita nel vedere teorie alla EdB. Bisogna iniziare il seminario con un cenno al superamento del teorema di von Neumann.
- 2) il lavoro di Aspect, Bell, etc., fa credere che non ci sia più spazio per modelli a variabili nascoste locali (ciò è stato chiaro a München). Riaprire lo spazio parlando in ogni seminario delle diseguaglianze forti.
- 3) non presentarsi mai troppo gentili. Qui efficienza e disponibilità non sono automaticamente simpatia (Derado e soprattutto Lichte). Meglio un po' duro e sicuro di sé.

22/4/89

Il seminario di ieri, (21/4 Dortmund) è andato molto bene. Hanno ascoltato, circa in 30, in religioso silenzio, parte prendendo appunti. Commento riportato da Bonse dei suoi colleghi: "il miglior seminario di fisica che abbiamo avuto". L'aver appreso dagli errori precedenti ha giovato! Crede Bonse che ho allargato spazi per il mio discorso. Bonse è un conservatore in politica, religioso, simpatico. Ha perdonato subito la mia dimenticanza del seminario alle 8 del mattino (!) e lo ha rischedulato alle 15. Vari scambi di idee, non ha l'aria di voler fare mio esperimento. Lui è stato contento del successo che il mio seminario (da lui organizzato) ha avuto presso i suoi colleghi. Bonse era preparato positivamente per la mia venuta, tanto che nella presentazione ha detto che era per loro "un onore" avermi come oratore. Perciò ha superato facilmente il mio errore di orario. Merito di Datta?

Questa è un'altra lezione da meditare. Chi si dà da fare vuole incassare simpatia nel suo ambiente, e non l'opposto.

23/4/89 a Dordrecht (casa editrice Kluwer)

Vendite

Wpd (11/83) 638 successfull

Oqqp (12/84) 660 successfull (best)

Mrqf (2/88) 111

NUOVA RIVISTA

Titolo provvisorio:

FOUNDATIONS OF QUANTUM MECHANICS, RELATIVITY AND COSMOLOGY

Editore:

Io, ma sarebbe buono se ve ne fossero altri 2 per USA, Oriente.

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Amministrazione

Per loro molto meglio averla a Dordrecht. Articoli da autori spediti a loro, da loro a me a Bari, poi referee, poi di nuovo a loro se accettati.

Inizio

Gennaio 1991, con un 1° numero alla fine del 1990. materiale pronto per Maggio-Giugno 1990.

Pubblicazione Novembre 1990

Periodicità

Deve uscire puntuale All'inizio bimensile, poi presto mensile

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(65% di risposte = successo)

DL deve scrivere un questionario per accertare la desiderabilità della rivista. Io debbo fornirgli 100 nomi subito. Debbono essere nomi significativi, gente che ci aiuterà quando la rivista esce. Gli serve "draft scope": 100 parole sul tipo di articoli che pubblicheremo.

Qualità

Debbono essere lavori di grande qualità scientifica e debbono essere selezionati con una alta frequenza di rifiuti da parte dei referee. Dobbiamo e vogliamo ricevere articoli non sollecitati.

Ci deve essere un mio ruolo attivo nell'imporre i primi numeri. Kluwer farà "a lot" negli investimenti e nella pubblicità.

- 1 articolo invitato ogni numero, come rassegna risultati di una certa linea.

- grande ruolo degli sperimentali.

- relatività e astrofisica.

- un po' di storia e filosofia (T.S. Kuhn nell'E.B., ma è rivista di hard physics)

Documento iniziale

Debbo preparargli 100-200 parole con le grandi idee “conferenze fatte”; “perché non bastano le riviste esistenti”; “idee per il prossimo millennio”; “successo di FP”. Deve fare un rapporto ai suoi superiori.

25/4/89 Bruxelles da Reigner

Al seminario circa 10 persone: Reigner, Vigier, Brijtebier, Sjödin, De Baere e amico, altri 3-4 più giovani e 1 anziano italiano che lavora gratis da Prigogine.

Vigier se ne va prima della fine perché “deve partecipare a un meeting politico contro un missile francese da 350 km”. Reigner chiede di fotocopiare ultimi lucidi del seminario. Sembra davvero interessato.

Nella sua università Opus Dei, Ebrei organizzati e massoni la fanno da padroni. Lui e un altro, dice, sono l'alibi della democrazia. È stato invitato a parlare di MQ a un meeting di massoni ma, dice, lui non ha nulla a che vedere con loro.

11 e 12/5/89 Roma II da Carsetti al suo convegno “Scientific Model of Physical Reality”.

È difficile credere che la collezione di copenaghiani non sia voluta da Carsetti

Impressionante omogeneità “a destra”, con d'E che nega addirittura la esistenza dell'elettrone, Primas il più in linea di tutti, Mittelstaedt forse appena un po' sfumato, Götschl filosofo amico di Carsetti tutto in linea con Copenhagen, Cini con una strana teoria (da risentire) in cui crede di poter localizzare la particella anche in presenza di interferenza (!) [naturalmente è “sbagliato” ai suoi livelli, ma perché?]

Quando ho parlato io c'erano 15-20 persone. Nessuna obiezione. Domanda di d'E circa l'esperimento di Stirling. Si è dovuto beccare che gli esperimenti su EPR non sono risolutivi.

Clima non favorevole per me, date le premesse, ma ne sono uscito bene, tutto sommato.

15 e 16/5/89 a Bari. Convegno “Modelli Scientifici della Realtà Fisica”

Garuccio: “forse la natura è fatta in modo tale che il limite 0,82 di Ferrero-Santos non possa mai essere superato per le efficienze dei fotomoltiplicatori”.

Minazzi: si dichiara sorpreso che io ho parlato bene di Kuhn-Popper-Feyerabend. Tolto il primo, gli altri “usano la storia della scienza come un supermarket”. (vero, lo è!). bella citazione materialistica di Leopardi (Lo zibaldone): la materia pensa.

Sue frasi ottimistiche: “lo scienziato militante pensa la realtà degli oggetti che la sua scienza investiga”. “quelli della scienza sono standard di pensiero elaborati all'interno della tradizione storica scientifica che ha portato a tanti successi” (questa la presentava come garanzia della positività del pensiero scientifico contemporaneo).

Ammette che la “nuova filosofia della scienza” sembra avere un ruolo positivo nella scienza (si tratta sempre di K-P-F). L'ho sfottuto in privato: “mi raccomando: K-P-F” e lui “K va bene, ma il resto...”. Evidenti influenze geymonattiane. Simpatico, però.

Bisognerebbe riflettere sui rapporti (intensi, credo) fra idealismo crociano e marxismo italiano.

Bufo ha scelto me per parlare in pubblico.

Questo è coerente con la sua impostazione iniziale del convegno, tutta sparata sul mio libro. Impostazione iniziale poi in gran parte riassorbita dalle partecipazioni francesi (d'E, LL, V).

Conferenza (une physique quantique causale et dans l'espace-temps est elle possible?)

Presenti solo 20 persone fra cui V, LL, d'E, Bufo, Isabelle Stengers, Cini.

Hanno ascoltato, senza commenti. Sostanzialmente è la linea del silenzio, dell'isolamento, del rifiuto del confronto. Viva la fisica che sta andando sempre meglio! Eppure gli ho spiegato gli esperimenti della tripla fenditura.

D'ESPAGNAT Realismo vicino

Réalisme proche (gli elementi di realtà sono tutti concetti familiari). Ma:

spazio-tempo, spazio curvo, creazione-annichilazione dimostrano che il "r.p." non va bene.

Oggettività debole=intersoggettività. Lui ha sempre detto così, altri gli han fatto dire diverso.

Una minoranza crede che vada bene il realismo vicino: Vigier, Selleri, Bohm, Bell.

Accusa di sterilità rivolta a costoro (io poi gli ho risposto: tripla fenditura).

Bell pensa che occorre rinunciare alla relatività sul piano ontologico. "Sono al più balle metafisiche".

Rifiuta anche il realismo matematico secondo cui le formule sono nella realtà. La MQ è chiaramente una teoria fenomenologica, non ontologica, anche matematicamente parlando. La MQ è come una parabola che le religioni usano per parlare in modo "deguisé" di una realtà non descrivibile. La realtà c'è ma non è conoscibile.

Lui e JPV si danno del tu. JPV diventa muto, in sua presenza, su certi problemi fondamentali.

CINI a tavola dice "non voglio confondermi con questi" (→me)

Si considera più realista di d'Espagnat ma meno di me e Vigier. A tavola gli chiedo: se la particella è localizzata su una traiettoria, cosa c'è sull'altra? Risponde con colpi di tosse, giri di parole, valzer di concetti.

STENGERS è per il realismo forte e dice così anche di Prigogine. Sfotte Cini che vuole stare "sulla corda" a metà tra d'E e me+V. Si è messa in filosofia perché la MQ la scioccava (causa scatenante). Trova "laides" teorie come quella degli universi paralleli di Everett-W e quella della coscienza dell'osservatore di Wigner (io ho aggiunto le scelte ritardate in un intervento). Nettamente simpatica dal punto di vista filosofico. Si confessa materialista. Si confessa storicista su mia richiesta però ci sono limitazioni che vengono dalla realtà.

LEVY-LEBLOND Sempre eguale ma ancora più a destra. Si dice pronto, ormai, a seguire d'E che gli criticava l'ontologia dei suoi "quantoni". 6 critiche di d'E, lui in sostanza ed esplicitamente gli dà ragione su tutto, rispondendo. Niente di nuovo nel suo dire rispetto ad un articolo che mi mandò vari anni fa. Dice sempre le stesse cose, ormai è un ferrovecchio della MQ apologetica.

12-14 ottobre-Roma-Accademia dei Lincei

Per il livello degli oratori, la dignità del luogo e la mano ferma degli organizzatori, questa è stata la migliore conferenza cui ho partecipato quest'anno.

3-9 settembre 1990 London-Imperial College-PIRT.

Circa 40 persone. Molto ben accolto mio paper su teorie di etere e relatività speciale. Coro di elogi con unica eccezione finale di Prokhovnik.

DUFFY demonio organizzativo di queste conferenze dal quale si scoprono tendenze mistiche. Wegener mi ha detto che appartiene alla "salvation army". Poi ha fatto parlare Craig, decisamente mistico, poi ha dato spazio a Cavalleri, emerita testa dell'Opus Dei. Con me ha tenuto un atteggiamento scostante, non ho capito bene perché (opera di Cavalleri? Questione della rivista?)

ROSCOE Simpatico. Ha detto che per lui sono il 1° italiano che non crede al big bang.

PROKHOVNIK Dice: "I am a mathematician". È un tipo in gamba e ha avuto successo. Ha detto però che la mia roba l'avrebbe dimostrata sbagliata (contro il coro generale) però poi non ne ha parlato. Ho comprato per £ 2 il suo vecchio libro ristampato in Australia (circa 65 anni).

SECTION 2.8.5.5-PP. 572-574: THE ORIGINAL ITALIAN TEXT BY SELLERI DOCUMENT

Le ricerche che sto portando avanti da diversi anni sui fondamenti della MQ hanno come motivo ispiratore fondamentale il tentativo di dedurre teoricamente da certe idee generalissime (gli elementi di realtà di Einstein, le onde vuote) delle conseguenze che possano essere verificate (meglio, falsificate) sperimentalmente. Nel periodo che finisce con il 1980 mi sono interessato alla diseguaglianza di Bell come fatto teorico, cercando di chiarire nel modo più lucido e chiaro la ormai stabile incompatibilità fra predizioni empiriche della MQ e il principio di realtà separabile. Il limite di queste ricerche era solo quello di non uscire da schemi teorici in cui l'alternativa alla mancanza di separabilità era in buona sostanza una immagine deterministica del mondo. Nel triennio 1981-1983 ho lavorato invece in uno schema probabilistico in cui la realtà è fatta di insiemi statistici più che di singoli sistemi e in cui le proprietà reali sono le probabilità predicibili dei vari auto valori. Due lavori hanno mostrato che questa idea generalizzata di realtà porta a conseguenze empiricamente significative nella forma di una diseguaglianza più generale di quella ormai classica di Bell che è violata dalla MQ almeno per efficienze dei contatori non inferiori al 90%. Dunque l'incompatibilità con l'idea di una realtà separabile permane pure in condizioni probabilistiche, un risultato che nessuno aveva anticipato. L'esperimento avviato a Catania sulla diseguaglianza di Bell potrà senza difficoltà verificare la validità o la violazione anche di questa diseguaglianza.

In un altro lavoro il principio di realtà separabile predice per il sistema di due kaoni neutri un rate $< 12\%$ di quello previsto dallo schema solito. Qui la critica del determinismo è meno importante dato che si ha a che fare con interazioni forti per la rivelazione dei mesoni K e l'ottenimento di efficienze prossime a 1 non dovrebbe presentare difficoltà insormontabili. So che il fisico francese J. Six di Orsay voleva fare un esperimento su queste predizioni ma credo che non abbia avuto il tempo-macchina al CERN. Il convegno di Bari "Open question in quantum physics" da me organizzato, ha portato a una svolta nell'analisi degli esperimenti di Aspect et al. di Orsay. È nata una collaborazione

Italia-Gran Bretagna-Spagna che ha portato a tre lavori in cui si è evidenziato in primo luogo che l'alta intensità dei fasci atomici usati da Aspect comporta un notevole rescattering risonante di fotoni nel fascio (35% circa), cosa confermata da un mio calcolo includente l'effetto Doppler.

Risultati utili a Catania a cui si consiglia una densità atomica $\leq 10^9$ atomi/cm³ per non avere più dell'1% di rescattering risonante nel fascio. Altro risultato della collaborazione Italia-Gran Bretagna-Spagna è la discussione di modelli che ammettono che la probabilità di rivelare un fotone possa essere diversa da fotone a fotone sempre dando in media l'efficienza quantica dei fotomoltiplicatori. Tutta la fisica dei fotoni singoli resta inalterata, solo per coppie di fotoni correlati vi possono essere differenze. Questi modelli possono essere formulati in modo da riprodurre la funzione di correlazione della MQ. Si avrebbe solo una piccola oscillazione proporzionale al coseno di quattro volte l'angolo fra gli assi dei polarizzatori. Non così piccola da non essere misurabile anche a CT. Sviluppo in un articolo con Garuccio del 1984. si è mostrato che esperimenti con tre polarizzatori mostrano effetti facili da dimostrare sperimentalmente se le probabilità di rivelazione sono variabili da fotone a fotone. In quattro lavori si è discussa la possibilità che l'onda vuota di Einstein, idea simile a quella di onda virtuale che Bohr propose nel 1924, possa agire sui sistemi eccitati stimolandone una più rapida diseccitazione.

Vi sono esperimenti che danno già indicazione positiva in tal senso, si pensi a emissione di luce da parte di atomi in prossimità di specchi. L'esperimento è stato discusso in tre conferenze internazionali (Perugia 1982, Atene 1982, Bari 1983). Ha avuto una buona accoglienza sia da parte di esperti di fondamenti che da fisici di altro genere e ha generato una serie di lavori di fisici italiani, francesi, austriaci, inglesi e statunitensi che stanno uscendo sulle principali riviste. La realizzazione più ovvia potrebbe ottenersi con un laser gain tube (duplicatore di fotoni) ma anche con la superfluorescenza. Due opere di rassegna sintetizzano le idee di lavori che affrontano da punti diversi i problemi posti dalla natura del dualismo onda/corpuscolo e dall'incompatibilità tra l'idea di una realtà separabile e certe predizioni empiriche valide per la situazione insolita di sistemi atomici e subatomici separati da una distanza macroscopica (entanglement, decoherence). Questi problemi avranno certo una soluzione accettabile per tutti se continuerà l'attività sperimentale, quella teorica avendo ormai raggiunto un livello soddisfacente sia dal punto di vista quantitativo che qualitativo (FSPA).

SECTION 2.9-P. 581-FOOTNOTE 774: THE REFERENCES IN SELLERI'S "EINSTEIN-PODOLSKY-ROSEN PARADOX FOR KAON PAIRS AT THE DAFNE ACCELERATOR"

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SECTION 2.9-PP. 589: THE ORIGINAL ITALIAN TEXT BY SELLERI LETTER TO MAGGI

Caro Maggi, rispondo alla tua circolare del 10 settembre u.s. Effettivamente qualcosa di nuovo che può interessare l'INFN ce l'ho. Ho finito un lungo lavoro che riguarda il paradosso di Einstein-Podolsky-Rosen per coppie di kaoni neutri prodotti nel decadimento di mesoni vettoriali “ ϕ ”. Il risultato è che esiste una discrepanza minima del 25% fra ogni possibile teoria di realismo locale e la meccanica quantistica. La *phi-factory* di Frascati è la macchina ideale per fare questo esperimento. Il lavoro è completo in tutte le sue parti, tranne la scrittura: sono rallentato nello scrivere da altri impegni, per esempio dalle 4 conferenze internazionali di settembre cui partecipo. Comunque presto il lavoro vedrà la luce. La ricerca è stata oggetto della tesi di laurea di Sebastiano Cobianco di Bologna: al momento della tesi (fine giugno u.s.) c'è stato molto interesse da parte soprattutto di Zichichi (anche di Giacomelli). I risultati sono stati presentati in una conferenza (Oviedo) e in un seminario (Belfast). I miei precedenti lavori sullo stesso argomento del 1983, del 1990 (con D.

Home) e del 1992 (con P. Privitera) sono superati dagli ultimi risultati che sono finalmente assolutamente generali, cioè veri per ogni teoria di realismo locale (Selleri to G. Maggi, 16 September 1996-FSPA- My underline).

SECTION 2.9-P. 590: THE ORIGINAL ITALIAN TEXT BY SELLERI LETTER TO BELTRAMETTI

È dall'83 che lavoro sul paradosso EPR per le coppie di kaoni e finalmente sono arrivato in porto con una dimostrazione generale dell'incompatibilità fra predizioni della QM e realismo locale. Il terzultimo atto della vicenda è stata la tesi di Cobianco a Bologna, che ha suscitato l'interesse di Giacomelli e Zichichi che ne hanno voluto una copia. Il penultimo è stato un seminario in UK. L'ultimo è il lavoro che sto completando e che manderò al Physical Review, cosa che non faccio da vent'anni.

C'è una discrepanza del 25-30% fra RL e MQ per la rivelazione congiunta di due K^0 bar a tempi propri diversi. La generalità del nuovo lavoro è tale che le critiche di Ghirardi, comunque sempre un po' capziose, sono superate in blocco.

Sto cercando di propagandare l'idea in Italia, perché l'esperimento è fattibile solo a Frascati colla phi-factory (KEK è molto dopo, ok, ma forse anche Stanford non c'è). Ne parlerò anche qui all'INFN di Bari il 7 Novembre p.v. il grosso dello sforzo lo farò appena il preprint è tecnicamente pronto: ogni calcolo analitico è terminato, manca solo la parte minore dei calcoli numerici.

SECTION 2.9-P. 592: THE ORIGINAL ITALIAN TEXT BY SELLERI LETTER TO UGO AMALDI JR

Caro Ugo,

ti mando copia del mio lavoro conclusivo sul paradosso di EPR per coppie di kaoni neutri. Questo lavoro supera alcune difficoltà ancora presenti nella lettera che pubblicammo nel 92 con Privitera, la principale delle quali è il fatto che alla fine usammo la teoria di Home e Selleri, insufficientemente generale. Ora però è tutto a posto.

Sarebbe bello se ti interessasse fare questo esperimento alla macchina di Frascati: è l'unica possibilità per risolvere davvero il paradosso di EPR. Una cosa così non la dico con leggerezza. Alla gente di Frascati sembra che non freggi più niente di queste cose, dopo la devastazione concettuale dovuta al lavoro di Srivastava e Widom, parlo almeno di Franzini (Selleri to U. Amaldi Jr., 5 May 1997-FSPA-My underline and bold)

SECTION 2.9-PP. 593-594: THE ORIGINAL ITALIAN TEXT BY SELLERI LETTER TO DIAMBRINI PALAZZI

Caro Diambrini, lo spazio per parlare alla vostra conferenza ora si sarebbe creato: parto in aereo da Roma il giorno 24 mattina, dunque, se fosse possibile, parlerei volentieri il 23. E' facile sostenere che il mio eventuale talk non c'entra per nulla con la "UNIFICATION" però ho sempre lavorato pensando a DAΦNE e i miei risultati potrebbero interessare a qualcuno. Vedi tu naturalmente. Allego titolo e abstract dell'eventuale seminario" (Selleri to Diambrini-Palazzi, 24 March 1997-FSPA. My underline).

SECTION 2.9-P. 598: THE ORIGINAL ITALIAN TEXT BY SELLERI LETTER TO LANCERI

Caro Lanceri, dopo essermi confrontato con Alexis il quale mi ha suggerito di rivolgermi a te in qualità di futuro coordinatore per la fisica di BaBar, vorrei proporre alla tua attenzione l'interessantissima possibilità di compiere a BaBar un fondamentale esperimento. Si tratta del controllo delle predizioni della meccanica quantistica e di quelle (incompatibili) del realismo locale. L'esperimento fu proposto, come forse ricordi, nell'articolo di Alexis e mio poi pubblicato sull'*European Physical Journal C*. In seguito ho presentato il test anche al Kaon2001 di Pisa e alla 10th Lomonosov Conference di Mosca riscuotendo un certo interesse. Adesso che BaBar ha raccolto dati in quantità sufficiente e che ha pubblicato la misura del Bd-mixing con eventi dileptonici, pensiamo sia giunto il momento di riproporre con maggior enfasi il nostro esperimento alla comunità di BaBar.

In via preliminare vorrei discutere con te per individuare la strategia migliore che possa portare BaBar nel giro di 12-18 mesi a pubblicare il risultato dell'esperimento proposto.

Per iniziare Alexis suggeriva un seminario teorico da fare durante un prossimo vostro incontro di collaborazione in cui fare emergere l'importanza e l'attualità del test che BaBar potrebbe effettuare. Nel frattempo si potrebbero contattare i responsabili del gruppo di analisi del B-mixing e/o i ricercatori che si sono già impegnati nella misura del B-mixing. Si può successivamente pensare di coagulare forze di alcune persone o gruppi su questa analisi.

Ti invito dunque a pensare a un percorso che porti, rispettando i tempi di un esperimento complesso ed in presa dati, a coagulare all'interno di BaBar un gruppo di fisici che portino a compimento questo test. Sono ovviamente ben felice di offrire la mia completa disponibilità a questo gruppo (Selleri to Lanceri, 2001-FSPA. My underline).

SECTION 2.10-P. 619: THE ORIGINAL ITALIAN TEXT BY SELLERI LETTER TO FERRERO

Caro Miguel, [...] questa volta arriverò a Oviedo con il contributo scritto pronto per gli atti. Ci sono due possibili discorsi che io posso fare:

1. EPR paradox for $K^0\bar{K}^0$ pairs. Questo è la continuazione del mio discorso alla prima conferenza di Oviedo. E' di fisica delle particelle, un pò tecnico.
2. Noninvariant one-way velocity of light. Questo è un discorso più immediatamente carico di storia e di filosofia. Ho trovato un'alternativa seria alla relatività speciale che secondo me è meglio dell'originale.

La scelta fra le due possibilità dipende da te. Io preferirei il secondo argomento perchè è comprensibile a tutti, e so per esperienza che può interessare l'uditorio. Tuttavia quella di Oviedo é una conferenza sulla meccanica quantistica e si potrebbe anche dire che la relatività non c'entra. Per favore decidi tu quale dei due discorsi farò e dimmelo al più presto (Selleri to Miguel Ferrero, 12 January 1996-FSPA).

SECTION 2.11-PP. 631-632: THE ORIGINAL ITALIAN TEXT BY (MINAZZI 2013)

Una [...] complessa, ricca ed articolata visione della conoscenza scientifica costituisce, del resto, una consapevolezza critico-epistemologica e scientifica che ha sempre accompagnato le molteplici disamine che Selleri, nel corso di vari decenni, ha costantemente consacrato all'indagine critica di diversi problemi aperti, direttamente connessi soprattutto con l'analisi dei fondamenti della fisica moderna.

Per questa ragione di fondo Selleri denuncia le unilateralità dogmatiche speculari di due approcci come quelli propri dello scientismo e dello storicismo: "Bisogna sfuggire al falso dilemma scientismo/storicismo ed accettare il fatto che una teoria scientifica può funzionare benissimo anche se pesantemente condizionata da scelte filosofiche dei più svariati tipi. Occorre considerare le diverse teorie per quello che sono veramente, fuori da ogni schematismo. Per fare questo, strumento indispensabile è una conoscenza abbastanza approfondita della storia della disciplina che si vuole studiare criticamente e delle sue connessioni reali con i più generali processi culturali e sociali".

[...] Selleri parla anche, esplicitamente e programmaticamente, del "ruolo ineliminabile della storia della scienza, non più soltanto come curiosità culturale, ma anche come strumento di comprensione critica per ogni ricercatore della propria disciplina". Ma, si badi, per Selleri questa apertura alla dimensione storica della scienza deve sempre e necessariamente coinvolgere anche la riflessione filosofica perché proprio "legando la scienza alla filosofia si può tentare di recuperare pienamente la dimensione storica del pensiero scientifico e di comprendere la natura dei travagli che hanno dato origine alle varie concezioni scientifiche" (Minazzi 2013, pagg. 182-184).

SECTION 2.11-PP. 632-633: THE ORIGINAL ITALIAN TEXT BY (NUTRICATI 1998)

Per gran parte della sua vita da studioso, Selleri ha concentrato i suoi studi sulla chiarificazione dei contenuti scientifici e filosofici della meccanica quantistica (Nutricati 1998, pag 123).

[Per Selleri] lo scienziato [...] non deve avere la presunzione d'essere sganciato dalle sfere filosofica, storico-sociale e culturale, nel momento in cui si affaccia alla scienza.

La filosofia con cui Selleri affronta le problematiche scientifiche svolge un duplice ruolo: da una parte cerca di recuperare quella prospettiva storica del pensiero scientifico che, distanziandosi dallo storicismo (inteso come analisi di quei contenuti culturali che, condizionando storicamente l'attività scientifica, la trasformano in "moda culturale") permette una maggiore comprensione delle conquiste conoscitive oggettive e delle differenti modalità di approccio alla realtà fisica; dall'altra l'atteggiamento filosofico è recuperato nel suo porsi come attività critico-costruttiva del prodotto scientifico-razionale della fisica contemporanea. Nell'approccio epistemologico di Selleri i due aspetti si intrecciano coerentemente con la sua adesione al realismo scientifico, imprescindibilmente condizionato dai principi di causalità e comprensibilità. (Nutricati 1998, pag 124)

[...] se la comprensione dei fenomeni fisici implica la necessità di oggettività, non è possibile non prendere in considerazione gli elementi arbitrari e soggettivi insiti nell'atto creativo.

La teoria scientifica risulta così essere una costruzione molto complessa che sintetizza e organizza evidenze e dati empirici, assieme a tendenze culturali e filosofiche.

[...] Quello che secondo Selleri occorre è un'ecologia della scienza, cioè un'ecologia teorico-concettuale che permetta di eliminare quella struttura dogmatica e autoritaria, e che allo stesso tempo evidenzi il dato di fatto secondo cui nella scienza non tutte le idee sono uguali ma ci sono idee positive e idee negative per il progresso.

La posizione che egli incarna è, allora, quella del neorealista che si batte contro le posizioni irrazionalistiche di coloro i quali sostengono la completezza della teoria quantistica.

Il cavallo di battaglia scientifico e filosofico a favore del realismo è il paradosso di EPR che, a parere di Selleri, col tempo diverrà elemento decisivo per la vittoria o sconfitta della concezione realista. (Nutricati 1998, pag. 125. My underline).

[Battaglia di Selleri contro il dogmatismo cieco].

Quest'ultimo preclude non solamente ogni tipo di dibattito filosofico, che dovrebbe essere sempre vivo nell'ambito della ricerca, ma tende a soffocare proprio quella ricerca che va "controcorrente" e la quale, come è accaduto molto spesso nella storia della nostra civiltà ha dato vigore allo sviluppo della conoscenza della realtà e del rapporto che intercorre tra essa e l'uomo (Nutricati 1998, pag 173).

CHAPTER 3

SECTION 3.2-PP. 643-645: THE ORIGINAL ITALIAN TEXTS BY (GIACOMINI 1971)

[In 1905, within the paper *Zur Elektrodynamik bewegter Körper*] Einstein, partendo da considerazioni elettrodinamiche, critica il concetto di quiete assoluta non solo nell'ambito della meccanica, ma anche dell'elettrodinamica. La nuova congettura in cui non si parla più di quiete assoluta è da lui chiamata "principio di relatività" e viene presentata unitamente a un'altra ipotesi, quella che la luce si propaghi nel vuoto con una velocità indipendente dalla velocità della fonte di emissione. In questo scritto viene poi esaminato il concetto di simultaneità, di cui si propone una definizione operazionistica, e vengono chiariti i risultati dell'applicazione del principio di relatività (Giacomini 1971, pag. 852).

[...] La memoria [di Einstein del 1905] è distinta in due sezioni: cinematica ed elettrodinamica. La sezione cinematica a sua volta consta di due parti ben divise fra loro: nella prima si può far rientrare la definizione di contemporaneità; nella seconda lo sviluppo formale di una teoria della trasformazione delle coordinate (Giacomini 1971, pag. 862).

[...] Vi è dunque una relatività nella definizione di simultaneità, dovuta alla velocità finita con cui qualunque segnale si trasmette, relatività che si trasmette anche nella relatività delle lunghezze.

[...] la seconda sezione della prima memoria introduce già un concetto che Einstein svilupperà a fondo e generalizzerà nella relatività generale: vi sono delle leggi fisiche [...] che sono invarianti relativisticamente, ossia hanno la stessa forma in tutti i sistemi in moto relativo uniforme [Sistemi di riferimento inerziali-SRI] (Giacomini 1971, pagg. 863-864).

[...] al di là del valore sperimentale [...] si può scorgere un valore di metodo assai grande. Infatti, ancora nella prima sezione della prima memoria, Einstein ha mostrato come, a partire dalle definizioni di contrazione dello spazio e di dilatazione del tempo, sia costruibile ex novo una cinematica relativistica.

Ecco allora apparire le definizioni di velocità relativistica, la spiegazione dell'effetto Doppler e, soprattutto, la definizione relativistica del momento e della dinamica delle singole particelle; ecco cioè, come si è detto, nascere tutta una nuova meccanica (Giacomini 1971, pagg. 866-867).

Secondo [...] Edmund Whittaker, storico della scienza moderna, la scoperta della relatività speciale va attribuita più a Lorentz e Poincaré che ad Einstein, benché questi abbia espresso la teoria in modo conciso e l'ha presentata chiaramente nei suoi risultati sperimentali. Anche Percy W. Bridgman ha sottolineato l'apporto di Lorentz alla nascita della teoria della relatività speciale, poiché questi aveva mostrato che la nozione di quiete assoluta nello spazio, ossia l'esistenza di un sistema di riferimento privilegiato, era priva di fondamento.

La tesi di Whittaker è storicamente e criticamente esatta? È vero che Poincaré e Lorentz hanno scoperto i risultati fondamentali della teoria speciale prima di Einstein? Dobbiamo chiederci, prima di tutto, se Einstein [ne] conosceva i lavori [...]. Oltre a ciò dobbiamo chiederci se egli ha davvero espresso gli stessi punti di vista dei due scienziati che lo avrebbero preceduto.

È vero che fin dal 1902 Poincaré aveva messo in evidenza l'impossibilità di far risultare un movimento relativamente all'etere. In [*Science et hypothèse*] aveva assunto la relatività dei moti di due sistemi che siano in moto relativo uniforme l'uno rispetto all'altro, negando la contemporaneità di due eventi che avvengono in luoghi diversi. Egli però restava sempre nel campo delle ipotesi, supponendo che una nuova meccanica potesse nascere solo in seguito ad esperienze certe. Pare che Einstein abbia effettivamente letto *Scienza e ipotesi* prima di pubblicare la sua memoria del 1905 e che da quel libro abbia tratto il nome "relatività" per indicare la sua teoria.

Lorentz aveva proposto le sue equazioni per spiegare l'esito dell'esperimento di Michelson-Morley, ed era riuscito a prevedere in base ad esse che l'aumento della velocità di un elettrone provoca un aumento di massa di questo. Einstein conosceva la memoria di Lorentz ed estese il risultato da questi ottenuto a tutti i corpi, togliendo alle equazioni il carattere preminente di ipotesi di circostanza, per dar loro un significato operativo.

Mentre Lorentz riconobbe che il significato fisico delle sue formule era stato chiarito da Einstein e Minkowsky, Whittaker vuole attribuire non ad Einstein ma a Lorentz la scoperta della relatività.

D'altro lato, benché Einstein affermasse di non avere studiato a fondo i lavori di Poincaré e di aver conosciuto solo l'opera di Lorentz sulla teoria elettromagnetica, bisogna tuttavia tener presente che già nel 1904 Lorentz aveva trattato del principio di invarianza, traendolo da Poincaré.

Tutti questi fatti rendono lecito sostenere che Einstein fu in un certo senso l'ordinatore di temi e suggerimenti già esistenti nella fisica del suo tempo, purché non si dimentichi che egli resta comunque il creatore di una nuova meccanica. In pochi momenti della storia della fisica è più evidente che le grandi idee sono frutto di una certa atmosfera scientifica più che di intuizioni geniali ed isolate. Si può concludere che senza le anticipazioni di Poincaré la teoria della relatività avrebbe trovato maggiori difficoltà ad imporsi sul piano filosofico, senza l'opera di Lorentz sarebbe forse mancata la possibilità di dare forma matematica a dei fatti operativi. Nulla però è tolto alla genialità di Einstein dalla constatazione del fatto che la sua opera ha coronato un insieme di critiche alla meccanica classica, più che aprirle (Giacomini 1971, pagg. 874-876).

SECTION 3.3-P. 656: THE ORIGINAL ITALIAN TEXTS BY (DE MARIA-MALTESE 1997)

Compito di questo articolo è ricostruire dettagliatamente le dinamiche specifiche in cui si articola questa ostilità e mettere in luce le ragioni di questa lunga opposizione analizzando i preconcetti scientifici ed epistemologici che, di volta in volta, assunsero la forma di un tenace sostegno del riduzionismo elettromagnetico con una conseguente difesa dell'etere e dei riferimenti privilegiati, o della teoria newtoniana della gravitazione e di una vera e propria allergia alle teorie fisiche altamente matematizzate che affondava le radici in una diffusa ignoranza dei formalismi che occorreva controllare per penetrarne il significato fisico (De Maria, Maltese 1997, pag. 2).

SECTION 3.3-PP. 656-657: THE ORIGINAL ITALIAN TEXTS BY (DE MARIA-MALTESE 1997)

La SRT ebbe in Europa e Stati Uniti un'accettazione lenta, tortuosa, difficile, con l'unica eccezione della Germania dove, già nel 1907-9 un piccolo ma prestigioso gruppo di fisici teorici (Planck e altri) comincia a comprenderne la novità e la portata e si schiera apertamente a suo favore.

Altrove (Francia, Inghilterra, Stati Uniti), la resistenza è molto più forte ed è guidata dai fisici sperimentali. In questo contesto l'Italia assume il ruolo di "estremo inferiore" nella scala dell'accettazione della RR dapprima e della RG poi. Nel nostro paese, infatti, la resistenza alla RR è all'inizio forte sia fra i matematici, sia fra i fisici e gli astronomi; in queste ultime due categorie, l'opposizione alla relatività è particolarmente tenace e perdurante.

È pur vero che alcuni matematici di rilievo come Guido Castelnuovo e Tullio Levi-Civita a cominciare dal 1911 iniziano a mostrare un atteggiamento di disponibilità e che Levi-Civita [...] a seguito della corrispondenza con Einstein del 1915 sviluppa grande interesse per la RG. Comincia, a partire dal 1916, a produrre i suoi celebri contributi nel campo e dal 1919, con uno sparuto gruppo di colleghi e collaboratori [...] dà vita a una campagna propagandistica a favore della relatività per rompere l'accerchiamento, superare la diffidenza e aprire crepe nel muro di ostilità sia fra i suoi colleghi matematici sia, soprattutto, fra i fisici. Sta però di fatto che Levi-Civita e collaboratori sono perfettamente consapevoli, ancora nel 1919 (l'anno dell'eclisse da cui prendono le mosse la conferma della RG e la "canonizzazione" di Einstein) di muoversi in un ambiente intellettualmente ostile e di costituire, in tale ambiente, una minoranza.

I fisici (a cominciare dal decano della comunità, Augusto Righi) e gli astronomi osservativi restano ostinatamente contrari alla relatività e, a partire dal 1912 e fino ad oltre la metà degli anni '20, i primi trasformano la loro iniziale diffidenza passiva in un'attiva discesa in campo per attaccare sul piano teorico alcuni pilastri della SRT e per proporre esperimenti volti a confermare la fondatezza dei loro attacchi. Primi attori di tale offensiva sono Michele La Rosa, Quirino Majorana, Augusto Righi (De Maria, Maltese 1997, pagg. 1-2. My underline).

SECTION 3.3-P. 657: THE ORIGINAL ITALIAN TEXTS BY (DE MARIA-MALTESE 1997)

Fedeltà all'etere e opposizione all'introduzione di ipotesi "metafisiche" in fisica caratterizzano l'atteggiamento di diffidenza e non di ostilità mostrato dai fisici sperimentali in quegli anni (De Maria, Maltese 1997, pag. 5).

SECTION 3.3-P. 658: THE ORIGINAL ITALIAN TEXTS BY (DE MARIA-MALTESE 1997)

[...] La polemica scatenata da Abraham [contro Einstein a seguito della sua teoria scalare sulla gravitazione, primo step verso la GRT] fu seguita con molto interesse dai fisici italiani, i quali videro in lui un sorta di paladino dell'etere, nonché un difensore della meccanica e dell'elettromagnetismo classici.

[...] Il grande credito che le asserzioni di Abraham ebbero in Italia poggiano sul fatto che questi era, per ammissione dello stesso Einstein, "colui che mostrava la migliore comprensione (della relatività)". Abraham fu anche il tramite attraverso il quale Levi-Civita entrò in corrispondenza con

Einstein. Il carteggio tra i due si sviluppò, intenso, nella primavera del 1915, e Levi-Civita contribuì a convincere Einstein ad abbandonare la sua precedente teoria a covarianza limitata e a giungere nel novembre 1915 alla corretta formulazione delle equazioni del campo gravitazionale, generalmente covarianti. A seguito della [corrispondenza] con Einstein, Levi-Civita, smessa la cautela che aveva contrassegnato i suoi primi interventi, divenne un entusiasta sostenitore delle teorie einsteiniane e si impegnò in un intenso e creativo lavoro sulla RG, pubblicando tra il 1917 e il 1920, ben quindici lavori. Dagli anni della guerra in poi Levi-Civita rappresentò il punto di riferimento per chi, in Italia, voleva interessarsi di relatività (De Maria, Maltese 1997, pagg. 6-7).

SECTION 3.3-P. 658: THE ORIGINAL ITALIAN TEXTS BY (DE MARIA-MALTESE 1997)

[Egli mostrava] diffidenza contro questa teoria “metafisica” e matematicamente troppo complessa. Righi credeva che la relatività fosse al massimo un utile strumento matematico di indagine, ma sperava che presto lo stato di avanzamento delle conoscenze divenisse tale da poter fare a meno delle sue implicazioni “metafisiche” (De Maria, Maltese 1997, pag. 19).

SECTION 3.3-P. 659: THE ORIGINAL ITALIAN TEXTS BY (DE MARIA-MALTESE 1997)

L’atteggiamento degli astronomi italiani sulla relatività merita una breve riflessione. Il loro antirelativismo è veemente e trova la sua ragion d’essere in una decisa difesa del paradigma newtoniano (spazio euclideo e legge di gravitazione universale). Essi preferiscono giustificare le “anomalie” di questo paradigma con modifiche alla legge di Newton o ipotizzando l’esistenza di masse ad hoc piuttosto che ammettere la RG. Negli anni ’10 ipotesi del genere erano peraltro state avanzate nella comunità internazionale degli astronomi ma erano ben presto state emarginate di fronte al successo della RG. In Italia esse erano ancora in auge negli anni ’20, dimostrando così il carattere della battaglia di retroguardia combattuta dai loro sostenitori.

Il quinto congresso internazionale di filosofia, tenutosi a Napoli nel 1924, ospitò un’intera sezione sulla relatività, presieduta da Marcolongo che pronunciò un discorso di apertura dal titolo significativo: “fra relativisti e antirelativisti”, dal che è facile dedurre che, ben 5 anni dopo l’eclisse da cui era scaturita la prima grande conferma sperimentale della RG e 19 anni dopo la RR, in Italia lo scontro tra nemici e fautori della relatività era lungi dall’essere concluso (De Maria, Maltese 1997, pagg. 31-32. My underline).

SECTION 3.3-P. 659: THE ORIGINAL ITALIAN TEXTS BY (DE MARIA-MALTESE 1997)

[...] l’ostilità nei confronti delle teorie di Einstein mise radici [in Italia] e crebbe su tre capisaldi.

Il primo è la difesa dell’etere come riferimento privilegiato e veicolo per la trasmissione dei fenomeni ottici ed elettromagnetici; il secondo, strettamente connesso al primo, fu la riluttanza ad abbandonare la geometria euclidea come griglia per lo spazio assoluto e la visualizzazione dei fenomeni; [il terzo] fu la fedeltà nei confronti della gravitazione universale newtoniana (De Maria, Maltese 1997, pag. 33).

SECTION 3.3-P. 660: THE ORIGINAL ITALIAN TEXTS BY (DE MARIA-MALTESE 1997)

Come detto all'inizio, l'Italia non fu la sola nazione a mostrare questa opposizione, la cui veemenza e durata collocano però il nostro paese in fondo a un'ipotetica scala di accettazione della relatività (De Maria, Maltese 1997, pag. 34).

SECTION 3.3-P. 660: THE ORIGINAL ITALIAN TEXT BY (BERGIA 2005)

Una svolta metodologica, la sostituzione di una metodologia induttiva con una ipotetico-deduttiva, si sarebbe dimostrata feconda. Ma risultava totalmente estranea alla mentalità, al credo epistemologico di buona parte dei fisici europei e alla quasi totalità degli italiani. È doverosa, a questo proposito, una precisazione: non stiamo qui includendo nella categoria i fisici-matematici. E questo su base sociologica: della fisica matematica si occupavano essenzialmente, se non proprio esclusivamente, i matematici. Ebbene, la formazione dei fisici, nel senso stretto che si è cercato di precisare, era di stampo positivistico. [...] Buona parte della comunità dei fisici italiani fu quindi pregiudizionalmente ostile alla teoria einsteiniana. Ma mentre i più si limitarono a un giudizio negativo aprioristico, vi fu chi, proprio dall'atteggiamento di base [...] fu portato a formulare critiche che risultarono costruttive e che avrebbero messo in moto qualche sviluppo; e, in qualche caso, a condurre esperimenti che, concepiti per confutare ipotesi einsteiniane, ne fornirono in effetti una corroborazione (Bergia 2005, pagg. 263-264).

SECTION 3.3-PP. 660-661: THE ORIGINAL ITALIAN TEXT BY (BERGIA 2005)

Einstein fu in più occasioni esplicito nel riconoscere il ruolo fondamentale avuto dall'opera dei fisici matematici italiani, da Levi Civita in particolare, oltre che da Ricci, per la costruzione della sua teoria. Nell'articolo del 1916 nel quale presentò in modo organico la sua costruzione, egli scriveva: "I mezzi matematici necessari per la teoria della relatività generale erano già pronti nel 'calcolo differenziale assoluto', il quale si basa sulle ricerche di Gauss, Riemann e Christoffel sulle varietà non euclidee, ed è stato eretto a sistema da Ricci e Levi Civita e da essi applicato a problemi della fisica teorica" (Einstein statement in Einstein A. (1916): "Die Grundlagen der Allgemeine Relativitätstheorie (The Foundation of the Generalised Theory of Relativity)", *Annalen der Physik*, 49, p. 769, here cited by Bergia, footnote n. 20, pag. 275).

SECTION 3.3-P. 661: THE ORIGINAL ITALIAN TEXT BY (BERGIA 2005)

Una menzione particolare merita Federico Enriques. [...] Membro dell'Accademia delle Scienze dell'Istituto di Bologna dal 1902, promosse la nomina di Einstein a membro corrispondente dell'Accademia. Einstein vi avrebbe tenuto tre conferenze il 22, 24 e 26 ottobre del 1921. Lo stesso Enriques presentò l'oratore nell'occasione della prima di esse. L'evento destò un'eco notevole, e contribuì alla diffusione delle tematiche relativistiche nell'ambiente culturale italiano (Bergia 2005, pagg. 278).

SECTION 3.3-PP. 661-662: THE ORIGINAL ITALIAN TEXT BY (GIACOMINI 1972)

[...] il primo, che va dal 1920 al 1930 circa, è quello in cui compaiono le critiche fondamentali neo-kantiane, machiane ed empiriocriticiste, oltre a quelle di Whitehead, Schlick e Bergson. Il secondo momento, che si può far giungere fino al 1950, è quello in cui si sviluppano le critiche dell'operazionismo di Bridgman e delle tendenze neopositivistiche. Anche se si svolgono in questo periodo, cioè dopo il 1930, un posto a parte meritano le critiche dei marxisti russi, perché quello che avviene in Russia tra il 1930 ed il 1940 non ha relazione di sorta con gli studi di critica scientifica e filosofica nell'Europa occidentale di quel periodo. L'interesse che muove i filosofi sovietici è di affrontare, partendo dai presupposti del materialismo dialettico, le teorie di Einstein, e ciò provoca fra le varie tendenze del marxismo sovietico un acceso dibattito che non ha nessuna controparte nell'epistemologia occidentale (Giacomini 1972, pag. 439).

SECTION 3.3-P. 662: THE ORIGINAL ITALIAN TEXT BY (GIACOMINI 1972)

[Le critiche del primo gruppo] sono tutte dei tentativi di porre in relazione il pensiero di Einstein con i problemi e le prospettive di una particolare filosofia, e cercano di sottolineare come la teoria della relatività abbia confermato vuoi il kantismo, vuoi la filosofia dell'evoluzione, e così via.

[...] dopo il 1930, quando [...] si sviluppano le critiche del materialismo dialettico e dei fondatori della meccanica quantistica, vediamo che non si tratta più di imporre certe categorie filosofiche per spiegare il significato della relatività, ma piuttosto si tratta di mostrarne i limiti e i difetti da vari punti di vista, in particolare da quello del valore conoscitivo delle teorie. Sono cioè critiche a prevalente carattere metodologico. In fondo, mentre nel 1920-30 si cerca di spiegare filosoficamente la teoria della relatività, dopo il 1930 le critiche sono volte alla valutazione della portata conoscitiva di questa teoria. Nel 1949 poi [...] le critiche si spostano a punti particolari, [...] approfondimento di temi specifici, che non sono più spiegazioni del significato globale della teoria della relatività, ma critiche al suo valore gnoseologico. Infine, gli studi apparsi attorno al 1960 svolgono quasi tutti un solo tema, che d'altronde è il più importante nel pensiero di Einstein, ossia quello dei rapporti tra geometria e fisica.

Percorrendo brevemente le critiche alla teoria della relatività, troveremo come fatti rilevanti l'opposizione permanente fra interpretazione operativa ed ontologica della teoria, il ricorrente tentativo di far passare lo spazio-tempo o come una mera costruzione del soggetto conoscente o come una sostanza entro la quale si svolgono gli eventi (Giacomini 1972, pagg. 439-440).

SECTION 3.3-PP. 662-663: THE ORIGINAL ITALIAN TEXT BY (GIACOMINI 1972)

[...] Davanti alla teoria della relatività risultarono insufficienti i vari convenzionalismi usciti dalla crisi del positivismo, i quali non potevano ammettere che la teoria di Einstein avesse una portata reale di trasformazione dei concetti. Le due tesi del totale ontologismo e del totale convenzionalismo escono battute nel corso del dibattito, forse perché non possiedono gli strumenti a capire l'origine storica delle idee di Einstein e la loro possibilità di realizzazione. Infatti nel pensiero di Einstein è presente tutta una tradizione, sorta nella Germania del XIX secolo, di geometrizzazione della fisica, e che risale a Riemann ed Helmholtz.

[...] Essa non si pone il problema della differenza tra ontologismo e convenzionalismo poiché ritiene di identificare la massima obiettività possibile della fisica con la più chiara ed euristicamente perfetta forma geometrica.

[Le critiche spiritualistiche a loro volta falliscono] dal punto di vista metodologico perché insistono troppo sulla libera creatività della mente umana. Le critiche più penetranti sono invece quelle che distinguono tra teoria della relatività generale e speciale salvando la seconda e non impegnandosi sul valore conoscitivo della prima. [...] Ugualmente interessanti sono le critiche che prendono le mosse dal centro della relatività, ossia dal rapporto tra fisica e geometria [...] per far capire che l'importante della teoria della relatività è la direzione di ricerca, il tipo di indagine indicata, ossia la tradizione scientifica in essa presente (Giacomini 1972, pagg. 440-441).

SECTION 3.4-P.666: THE ORIGINAL ITALIAN TEXT BY SELLERI LETTER TO MONTI

[...] Ci sono cose che uno scrive a una persona, ma che poi non vorrebbe vedere divulgate all'intero popolo italiano. Esempio? Ti ho detto di non credere nel principio di relatività speciale, ma la stessa cosa (che è vera) non la direi a una persona che mi giudicherebbe subito male (e.g. Amaldi), se non dopo averla articolata bene in un articolo scientifico. Questo articolo non so quando (e neppure di sicuro se) lo farò, avendo per il momento altre priorità.

[...] Complessivamente è impressionante l'esercito di relativisti che si sta formando in dissenso con Einstein: Aspiden, Silvertooth, Monti, Essen, Marinov, Wesley ecc. Può darsi che stia montando una marea che porterà a grandi cose. Ivi tu sei il rappresentante della fisica italiana. È una buona fisica, all'altezza delle migliori del mondo e forse, culturalmente, la migliore in assoluto (Selleri to Monti, 20 April 1988-FSPA. My underline and bold).

SECTION 3.4-P.669: THE ORIGINAL ITALIAN TEXT BY SELLERI LETTER TO CAVALLERI

Il tuo articolo con Bernasconi è molto bello e lucido, si vede che sei una testa pensante. [...] c'è una sola cosa che non capisco e ti pregherei di spiegarmela. [...] .si derivano le TL a partire dalla contrazione delle lunghezze e dalla dilatazione degli intervalli temporali.

Altrimenti le basi sono classiche, ad esempio si combinano le velocità così: c-v. Non ho difficoltà a sintonizzarmi con voi, la vostra filosofia mi va benissimo. Il punto è però che dopo aver ottenuto le TL, andando avanti al solito modo che tu ben sai, si trova che le velocità si combinano invece [diversamente] (Selleri to G. Cavalleri, 16 October 1990. Selleri underline).

SECTION 3.4-PP.669-670: THE ORIGINAL ITALIAN TEXT BY SELLERI LETTER TO BLANDINO

[...] queste eccezioni rendono non del tutto convincente la conclusione di una "normale" validità della località. Tuttavia queste eccezioni vengono bloccate dalla RT: ogni segnale richiede un tempo finito per propagarsi fra due punti A e B, e chi sta in B non può sapere cosa è accaduto in A prima che quel tempo sia passato.

La MQ invece richiede una propagazione istantanea di influenze fra A e B, tramite la riduzione del pacchetto d'onde. E bada bene, ciò è richiesto anche dalla teoria dei campi quantizzati nella sua versione relativistica!

Il che dimostra che tale teoria è in realtà poco relativistica e che costituisce un pasticcio logico. Comunque il punto resta questo: la diseuguaglianza di Bell, deducibile da realismo + relatività speciale, è in contrasto con la QM. Tale contrasto è a livello sperimentale e costituisce una gravissima crisi della fisica contemporanea. In sostanza occorre scegliere tra RT e QM. Una via d'uscita invocata dallo stesso Bell è quella di un ritorno alla "relatività" di Lorentz, con il suo bravo tempo assoluto, con le lunghezze che si contraggono realmente, con l'etere e "forse" (lui ci credeva, io ci credo poco) con la possibilità di superare la velocità della luce rispetto ad un sistema in moto. Intendiamoci, a me piace molto di più Lorentz [che] Einstein, filosoficamente, ma credo poco a questa soluzione del paradosso di EPR. La mia soluzione preferita è lo smantellamento della teoria quantistica (Selleri to Blandino, 3 April 1991-FSPA-My underline).

SECTION 3.4-P. 678: THE ORIGINAL ITALIAN TEXT BY BARONE ON TRR

A tutt'oggi esiste un filone della ricerca in fisica il cui scopo è cercare di recuperare l'etere, la cui presenza potrebbe essere osservabile con una nuova generazione di esperimenti collegati anche alla cosmologia.

G. Builder e S. Prokhovnik, fisici australiani e J.S. Bell, irlandese hanno lavorato in questa direzione facendo da battistrada a molti altri. Builder, in due articoli, *Ether and relativity* e *The constancy of the velocity of light*, entrambi del 1958, pubblicati nell'*Australian Journal of Physics* fa una critica molto acuta della TRR. In particolare, egli asserisce che la velocità della luce deve essere interpretata come "velocità della luce misurata in un SRI e che la misura deve considerarsi come la media sul tragitto di andata e ritorno".

[...] Assumendo questa procedura di sincronizzazione degli orologi e l'ipotesi di un etere associato ad un sistema di riferimento universale collegabile alla massa delle stelle fisse, Builder arriva alle stesse conclusioni della TRR.

H.E. Ives in un articolo del 1945, pubblicato in *Phil Mag.* riuscì a giustificare la contrazione delle lunghezze, la dilatazione dei tempi e l'aumento della massa studiando l'interazione di un'onda elettromagnetica con una particella libera, completamente riflettente ed imponendo la conservazione dell'energia e della qdm sia nel SRA sia in un SRI. Ruderfer e Prokhovnik richiamandosi ai lavori di Ives e Boulder hanno formulato una teoria relativistica dell'etere, che spieghi tutti i risultati di Einstein. L'insieme di questi tentativi di formulare una teoria fondata sull'etere, compatibile con la TRR ha dato luogo alle cosiddette "Teorie neo-lorentziane dell'etere" [...].

D'altro canto, osservazioni cosmologiche sull'isotropia della materia nell'Universo, sulla radiazione di fondo a 2.7 K e sull'origine della materia oscura, necessaria a giustificare l'elevata velocità di rotazione intorno al proprio asse di alcune stelle poste ai limiti delle galassie a spirale, Via Lattea inclusa, e che al 90% sarebbe diffusa in un alone sferico intorno ad esse, depongono in favore di un sistema di riferimento fondamentale per la propagazione della luce.

SECTION 3.5-PP. 684-685: THE ORIGINAL ITALIAN TEXT BY THE LETTER SENT BY SELLERI TO CARBONARO

La velocità della luce in una sola direzione è non misurabile secondo molti “esperti” che ho sentito parlare in conferenze internazionali o con cui ho parlato personalmente. Lo stesso Prokhovnik ha fatto questa affermazione a Trani [nel 1992].

Siccome dalla fisica classica si ottiene tutta la relatività speciale tranne che viene fuori una velocità diversa da c in una sola direzione, la gente fa questo postulato a mio parere solo per salvare capra e cavoli. Non c'è davvero base empirica, tranne il fatto che misurarla in una sola direzione è molto più difficile e impreciso. Scusa, l'esperimento di Roemer non è forse una misura di questo tipo? La luce infatti va da Giove [qui alla Terra]. Io so immaginare modi in laboratorio di misurare la velocità della luce in una sola direzione grazie a ruote dentate ma mi limito a raccontarti un esperimento che mi ha detto il Bartocci per telefono. Prendi un laser, attaccalo solidamente ad una roccia e registra il punto di arrivo della sua luce a 1 km di distanza. Poi vedi cosa succede durante un anno. Se la velocità della luce si compone con quella della Terra attorno al sole, 10^4 volte più piccola (30 km/s contro 300.000 km/s), ci debbono essere deviazioni di 10^{-4} radianti, cioè di 10 cm a quella distanza! Il puntolino luminoso dovrebbe descrivere un'ellisse per Lorentz, restare fermo per Einstein! Facile e bella, no? Ma siccome mala tempora curunt è ovvio che nessuno la farà! **Io sono convinto che Lorentz avesse ragione e so già qual è la giusta teoria: è quella basata sulle trasformazioni di Tangherlini che abolì il termine contenente x nella trasformazione del tempo. Così però aveva la contrazione di Lorentz e la dilatazione degli intervalli temporali. La sua però era una teoria di etere, come si vede subito ad esempio dal fatto che le sue trasformazioni non formano un gruppo. Oppure ricorda che nel mio lavoretto ho dimostrato che ogni trasformazione diversa da quella di Lorentz descrive una teoria di etere.**

Ci sarebbe un grosso lavoro da rifare per vedere quali sono le conseguenze elettromagnetiche della trasformazione di Tangherlini (Selleri to Carbonaro, 3 February 1993-FSPA. Selleri underline, my bold).

SECTION 3.5-P. 689: THE ORIGINAL ITALIAN TEXT BY THE LETTER SENT BY SELLERI TO BARTOCCI

Ma torniamo alla relatività. Io sto avendo un'esperienza bellissima dopo la lettura dei lavori di Builder e di Prokhovnik: la teoria della relatività speciale è la teoria dell'etere! Cioè essa è la teoria delle velocità assolute chiarissimamente indicate dalla sola soluzione nota del paradosso dei gemelli. Per me è un capovolgimento di posizione, e non dispero più di arrivare fra poco a indicare dove si verificherà il “breakdown” della RS.

Questa della relatività è una cosa stupenda che tutti dovrebbero accettare, se il mondo della fisica non fosse travagliato da quella mostruosa crisi che gli fa chiudere le porte in faccia ad ogni idea nuova, per difendere spasmodicamente idee vecchie e superate.

[...] sto correggendo le bozze di un'enciclopedia della fisica [...] che contiene molte voci storiche, filosofiche e critiche. L'abbiamo fatta in due fisici di Bari. Ci sono poi 4 prolusioni introduttive, tutte di fisica alternativa:

1. Narlikar sull'astrofisica.
2. Owen sulla geofisica (Terra in espansione).
3. Tyapkin sulla RS con vasti accordi con le idee di Builder e Prokhovnik.
4. Io sulla MQ.

Speriamo che la stampa noti la novità di questo lavoro, coi libri si rischia sempre di lavorare per niente (Selleri to Bartocci, undated, possibly before 1993, year of publication of Dictionary-FSPA. My underline).

SECTION 3.5-PP. 704-705: THE ORIGINAL ITALIAN TEXT IN (NUTRICATI 98)

- quelli che riconoscono nella velocità del gemello in viaggio la causa del rallentamento dei suoi processi biologici;
- quelli che invece cercano di attribuire lo stesso rallentamento alle accelerazioni subite alla partenza, al momento dell'inversione della direzione di volo e all'arrivo.

Ovviamente gli autori del secondo filone cercano di salvare la perfetta simmetria dei movimenti rettilinei e uniformi richiesta dal principio di relatività speciale. La loro posizione è però indifendibile, come fu dimostrato da Builder [nel 1958] con un semplicissimo argomento: in fisica si può riconoscere la causa di un fenomeno variandola e verificando l'esistenza di corrispondenti variazioni nell'effetto. Nel caso dei nostri gemelli, se quello in viaggio raddoppia la lunghezza dei percorsi di moto rettilineo uniforme lasciando inalterati i processi di accelerazione e frenamento, trova raddoppiata la sua differenza di età da quello rimasto a terra. Nulla cambia, invece, se si modifica l'accelerazione lasciando invariati gli integrali della velocità. Perciò la velocità e non l'accelerazione è responsabile dell'invecchiamento asimmetrico (Nutricati 1998, pagg. 23-24).

SECTION 3.5-PP. 707-709: THE WHOLE ORIGINAL ITALIAN TEXT BY THE LETTER SENT BY SELLERI TO SEVERAL SCHOLARS ON HIS BOOK "LEZIONI DI RELATIVITÀ"

LEZIONI DI RELATIVITÀ-da Einstein all'etere di Lorentz

È un libro eretico, se "eresia" è opposizione a dogmi stabiliti. È invece ortodosso, se "ortodossia" è ricerca della verità come per G. Bruno e G. Galilei.

La teoria della relatività ha avuto successo nello spiegare molti fenomeni noti e nel predirne di nuovi. Costituisce dunque un'importante avanzata nella nostra conoscenza del mondo. Tuttavia è difficile credere che sia una forma di conoscenza irreversibile. Al contrario, se c'è una lezione dell'epistemologia (Popper, Lakatos, Kuhn) questa riguarda proprio la natura congetturale delle teorie fisiche del Novecento. Nel 1949, rispondendo a Solovine che gli aveva inviato una lettera di auguri per il settantesimo compleanno Einstein scrisse: "Tu immagini che io guardi all'indietro sul lavoro della mia vita con calma soddisfazione. Ma da vicino la cosa appare ben diversa. Non c'è un solo concetto di cui io sia convinto che resisterà stabilmente". Einstein non nascose la transitorietà delle sue creazioni. Il suo ultimo articolo, una prefazione di poche pagine a una raccolta di saggi di autori italiani scritti per i 50 anni della relatività, termina affermando che la fisica era lontana dall'avere una base concettuale affidabile.

I successi delle teorie relativistiche sono notissimi. E tuttavia sarebbe sbagliato concludere che ogni confronto con gli esperimenti si sia concluso invariabilmente con un perfetto accordo.

La fisica è un'attività umana e da noi prende il difetto di sbandierare i successi e di nascondere le difficoltà e i fallimenti. Così un grande silenzio ha circondato l'effetto Sagnac (del 1913) per il quale esiste una vera e propria incapacità esplicativa delle due teorie relativistiche, nonostante i tentativi di alcuni ricercatori (Langevin, Post, Landau e Lifshitz). Altro silenzio ha circondato il significato della "correzione Sagnac" della fisica satellitare, caso clamoroso di una teoria che viene usata così: se ne applica un'altra (la meccanica newtoniana) appiccicandole certe "correzioni relativistiche", una delle quali totalmente impropria. Inoltre ci sono le mezze spiegazioni dell'aberrazione della luce stellare e del paradosso dei gemelli, fenomeni per i quali il formalismo matematico della teoria riproduce le osservazioni, al prezzo però di distorcere i significati al di là del lecito. Dietro alle equazioni di una teoria c'è una enorme struttura qualitativa fatta di risultati empirici, generalizzazioni, ipotesi, scelte filosofiche, gusti personali, convenienze. Quando si prende atto di questa realtà si comprende facilmente che la relatività, accanto ai suoi innegabili successi, può presentare punti deboli e può anche sopravvivere a qualche fallimento. La correttezza del formalismo matematico non basta a omologare una struttura come non contraddittoria e neanche mille fisici incondizionatamente favorevoli alla teoria sono una garanzia sufficiente, perché quasi sempre i loro pensieri sono stati orientati in senso acritico fin dagli studi universitari, verso un'accettazione acritica della teoria dominante

In realtà le due teorie relativistiche sono piene di paradossi, la soluzione dei quali, in un nuovo contesto, è un obiettivo del libro. Il mio disaccordo con chi nega l'esistenza dei paradossi ruota attorno all'idea di "buon senso". Questa espressione, traducibile in ogni lingua neolatina, è assente in altre. Gli inglesi usano "common sense" che però è cosa diversa perché il senso comune è quello della maggioranza degli uomini e nelle cose della scienza la storia insegna che la maggioranza ha raramente ragione. Da noi il "buon senso" si collega alle "sensate esperienze" di galileiana memoria. Bene, se il buon senso ci dice che una certa predizione della teoria è irragionevole vi sono due possibilità: (1) che il buon senso ci inganni; (2) che nella teoria si annidino ipotesi contro natura che danno un significato distorto alle sue predizioni. E' noto che il Novecento ha seguito la moda di dichiarare il buon senso obsoleto, ma il mio libro mostra che la seconda strada è percorribile ed elimina i paradossi della relatività. Si parte dalle "trasformazioni equivalenti" contenenti un parametro libero, e_1 , il coefficiente di x nella trasf. del tempo ("parametro di sincronizzazione") che esprime gli aspetti convenzionali della teoria in modo sintetico. Le trasformazioni di Lorentz si ottengono come caso particolare per un certo e_1 . Calcoli espliciti mostrano che le osservazioni empiriche sono spesso insensibili a e_1 (Römer, Bradley, Fizeau, Michelson-Morley, Doppler, ...). Pertanto in linea di principio esistono infinite teorie che possono spiegarle egualmente bene.

Le trasformazioni fra sistemi inerziali, applicate al mondo reale, non possono evitare la questione delle accelerazioni perché i laboratori sperimentali hanno un'accelerazione piccola ma non nulla. Le accelerazioni, sia lineari (argomento delle due astronavi) che centripete (argomento delle piattaforme ruotanti), rompono la simmetria fra le trasformazioni equivalenti.

Le sole compatibili con tutti i fatti noti sono le “trasformazioni inerziali”, basate su $e_1 = 0$ e quindi sulla simultaneità assoluta. Solo con $e_1 = 0$ si può evitare una discontinuità fra sistemi inerziali e sistemi accelerati.

L'effetto Sagnac trova finalmente la sua completa spiegazione e la fisica satellitare si svuota dei suoi misteri. Emergono limpide soluzioni dei paradossi relativistici. Esempio la soluzione del paradosso dei gemelli: tolto di mezzo il relativismo resta che l'invecchiamento minore è sempre di chi ha subito di più gli effetti della velocità assoluta. La nuova teoria mostra che il movimento genera effetti concreti sui corpi in moto. Bisogna quindi ammettere che lo spazio sia fisicamente attivo, il che chiaramente costituisce un ritorno all'etere di Lorentz. La spiegazione dei fenomeni relativistici da parte di una teoria basata sull'esistenza di un sistema privilegiato riesce clamorosamente bene.

Se avrai la pazienza di leggere il mio libro vi troverai altre cose interessanti, sui segnali superluminali e sulla necessità di abbandonare il *big bang*, per esempio.

Infatti nello spazio ordinario il cosmo del big bang sarebbe una struttura irregolare composta da una regione vuota centrale, il cratere dell'esplosione, una regione intermedia contenente i corpi celesti e una parte esterna contenente solo radiazione. Qualunque fosse la nostra posizione vedremmo una volta celeste molto diversa da quella che rivelano i grandi telescopi. Nessuna struttura nello spazio a tre dimensioni, nata da un'esplosione avvenuta 10-20 miliardi di anni fa, può contenere un universo con le caratteristiche osservate. È per questo che tutti i modelli teorici del big bang trovano assolutamente indispensabile introdurre la quarta dimensione. Bisogna allora osservare che dal punto di vista concettuale questi modelli hanno equilibrio instabile, perché derivano dallo spazio a quattro dimensioni della relatività generale, a sua volta figlio dello spazio di Minkowski della relatività speciale. Pertanto il grande botto dipende essenzialmente dal mescolamento dello spazio col tempo della relatività speciale. Insomma, tutto entra in crisi se si modifica la quarta trasformazione di Lorentz. Ma questo è proprio quello che io faccio sostituendo le trasformazioni inerziali a quelle di Lorentz! Insomma, se sono giuste le trasformazioni inerziali il big bang non può essere vero.

SECTION 3.7.2-PP. 736-737: THE ORIGINAL ITALIAN TEXT BY THE LETTER SENT BY SELLERI TO PAOLO MAFFEI

[...] Siccome le cose si capiscono meglio da lontano, forse il rapporto Arp/astrofisica può essere per me più illuminante di quello Selleri/fisica quantistica, dati gli inevitabili aspetti soggettivi che complicano il secondo. La domanda è questa: è possibile che Arp abbia ragione, che le quasar siano vicine, ma che tutti gli astrofisici rifiutino di vedere la luce? **Spero tanto che la risposta sia negativa, davvero preferirei. Ci sarebbe l'errore caparbio di un singolo che non vuole adeguarsi a qualunque prezzo, ma la sanità media dell'astrofisica sarebbe salva. Se invece la risposta fosse positiva, dato che Arp sta avanzando la sua tesi da vari decenni, bisognerebbe concludere che siamo caduti in un periodo di assoluto oscurantismo e di codardia in cui nessuno per quanto potente e per quanto inserito nei massimi laboratori astrofisici osa ergersi a difesa della verità. Perché qui la verità dipende direttamente dalle osservazioni ed è comunque più accessibile, mi sembra, di quella quantistica in cui considerazioni teoriche e filosofiche oscurano tante menti.**

Il fatto è che posso dire di conoscere gli argomenti di Arp (ho anche fatto un seminario su di essi) e non vedo cosa impedisca una loro sistematica distruzione, se sono falsi, agli occhi di quanti, come me, non riescono a dimenticarli.

Invece trovo un libro di Sagan che ne parla un pochino appena, per dire che potrebbe anche avere ragione! And Dallaporta a Padova è stato possibilista. Mi interessa moltissimo il tuo parere sincero al 100%. Ti prometto di non parlarne con nessuno. [...] Ma io vorrei anche capire se vale la pena o no che io continui a dedicare il mio tempo in parte a Arp. Dov'è la verità? (Selleri to P. Maffei, 4 February 1989-FSPA. Selleri's underline, my **bold**).

SECTION 3.7.2-P. 737: THE ORIGINAL ITALIAN TEXT BY THE LETTER SENT BY SELLERI TO TULLIO REGGE

Ma Arp l'hai letto? Io l'ho studiato. E per un anno intero. Che la gente non accetti le mie idee è normale, visto che sono proposte teoriche. Ma Arp presenta un numero grande (circa 30) di evidenze sperimentali indipendenti. Sono convincentissime, con la sola riserva che gli esperimenti non li ho fatti io e non posso sapere se se li è inventati. Ho allora parlato con diversi astrofisici: Maffei, Bertola, Braccesi. Pur con sfumature diverse non ce n'è uno che accetti le conclusioni di Arp. Però non ce n'è nemmeno nessuno che contesti la validità dei dati sperimentali. Respingono Arp con argomenti speciosi ("è isolato...") ma non sul terreno scientifico. Da questi contatti è uscita in me rafforzata la conclusione che Arp abbia ragione, e che la comunità scientifica degli astrofisici sia più chiusa e dogmatica di quella delle alte energie, e di molto.

Rifiutano Arp perché i motivi dello spostamento verso il rosso delle quasar non sono chiari teoricamente se lui ha ragione. Bel modo di ragionare! (Selleri to Tullio Regge-FSPA. My underline).

SECTION 3.7.2-P. 740: THE ORIGINAL ITALIAN TEXT BY THE INTRODUCTION MADE BY SELLERI TO VAN FLANDERN

Tom ha ricevuto il Ph.D. in Astronomia all'Università di Yale (Stati Uniti) nel 1969. Ha passato 20 anni al U.S. Naval Observatory, dove è stato il capo della divisione di Meccanica Celeste. Ora ha fondato un'organizzazione con base a Washington, la Meta Research, che promuove ricerche su idee altrimenti non finanziate solo perché in conflitto con le teorie dominanti dell'astronomia. È editore della rivista trimestrale Meta Research Bulletin. Ed è associato alla ricerca presso il Dipartimento di Fisica dell'Università del Maryland, dove studia la precisione del sistema di posizionamento globale ("Global Positioning System").

La casa editrice North Atlantic Books ha pubblicato il suo libro, "Dark Matter, Missing Planets and New Comets" (2a edizione nel 1999). Come i suoi articoli scientifici, il libro è critico su molte idee standard dell'Astronomia, come la nube di Oort, la palla di neve sporca, e la teoria del Big Bang. Tom guida anche delle spedizioni per osservare le eclissi per i ricercatori e per il pubblico.

I suoi interessi addizionali includono la salute, la biochimica ed i calcolatori, con enfasi sulle grafiche di presentazione. Uno dei suoi articoli recenti “The speed of gravity - What the experiments say” [Phys. Letters A 250, 1 (1998)] è di interesse eccezionale e se ne consiglia vivamente la lettura.

SECTION 3.8-P. 745: THE ORIGINAL ITALIAN TEXT BY THE LETTER SENT BY SILVIO MORA TO SELLERI

Il mio interesse [per la possibile sopravvivenza dell'etere, per la compatibilità dell'etere con la TRS] nacque in parte dallo stupore per le contestazioni che alcuni studiosi di valore muovevano ogni tanto alla soluzione data dalla relatività al paradosso dei gemelli e in parte dallo strano rifiuto di Ives di accettare l'interpretazione relativistica dell'effetto doppler trasversale, da lui messo in evidenza per la prima volta con Stilwell, nel suo famoso esperimento che provava [anche allo stesso tempo] l'effetto di dilatazione del tempo. Tu hai posto con molta chiarezza il problema della ricerca di tutte le possibili trasformazioni spaziotemporali equivalenti a quelle di Lorentz ma non contenenti il principio di relatività. [...] come tu giustamente sottolinei il principio di relatività poggia su una convenzione adottata sulla sincronizzazione degli orologi, che prescinde dalla conoscenza di c su un percorso di sola andata, misura mai fatta e forse non eseguibile (S. Mora to Selleri, 12 May 1996-FSPA).

SECTION 3.8-P. 746: THE ORIGINAL ITALIAN TEXT BY THE LETTER SENT BY EMILIO SANTOS TO SELLERI

Mi è piaciuto sapere il tuo motivo per cambiare la relatività. Lo considero necessario per reintrodurre l'etere. Sono d'accordo che l'etere sia necessario in quanto il vuoto della fisica non si può immaginare come uno spazio realmente vuoto. Per ME non andrebbe cambiata la TR ma se la necessaria esistenza di un etere implica il cambiamento della relatività, va cambiata (E. Santos to Selleri, 6 May 1998-FSPA).

SECTION 3.8-P.755: THE ORIGINAL ITALIAN TEXT BY THE LETTER SENT BY SELLERI TO MICHELE RUSSO

le mie idee suscitano reazioni furiose da tutti i lati, per esempio c'è un dibattito in corso su Internet, con dozzine e dozzine di interventi, su quello che chiamano il “paradosso di Selleri”, che sarebbe poi la faccenda della discontinuità fra piattaforme ruotanti e sistemi inerziali della TRS (25 Agosto 1998-FSPA).

Tengo a precisare che io non sono partito con un fine preciso (come potrebbe essere quello di demolire il big bang). Mi sono limitato a comprendere che la sincronizzazione degli orologi di Einstein era una convenzione, non un fatto della natura. Lo dice lo stesso Albert nel suo articolo del 1905 sulla relatività. Poi ho costruito l'insieme delle teorie diverse solo per la convenzione sulla velocità della luce. Poi mi sono accorto che non erano perfettamente equivalenti e che quella che conteneva la scritta trasformazione del tempo era meglio delle altre, per tre ottime ragioni indipendenti. Tutto il resto ne è derivato di conseguenza (Selleri to Russo, 12 June 2002).

SUMMARY AND CONCLUSIONS

PP.770-771: THE ORIGINAL ITALIAN TEXT BY SELLERI FSPA

[...] c'è gente che ha saputo realizzare i propri sogni, lei mi dice a proposito di suo fratello e suo marito. Come li invidio! Le speranze che io possa realizzare i miei sono ben scarse, anche se fido di appartenere ad un movimento di revisione delle basi della fisica, che prima o poi vincerà. La crisi della fisica, ben nascosta ai non addetti ai lavori, è devastante, sono decenni che perdono tempo dietro a costosissimi ed inutili progetti. Ma il controllo delle carriere e delle pubblicazioni lascia spazio ad uno sparuto gruppo di coraggiosi che possono liberamente scrivere e parlare, tanto nessuno ascolta. Nel 1927 c'è stata una mutazione genetica della fisica, perché gli irrazionalisti hanno fondato una nuova teoria vincente che regolava i conti filosofici col secolo dei lumi, col Rinascimento e con quanto di buono la cultura umana aveva prodotto. Da allora si brancola nel buio e l'attività "scientifica" consiste di una serie di conati matematici e di ricerche sperimentali che cercano di corroborarli, ma che quasi sempre li falsificano. Ci si può consolare dicendo che almeno gli sperimentali non barano. È così, almeno fino al prossimo salto di qualità. Pensi solo alle "grandi idee" della fisica contemporanea: i quark ed il big bang. Bene, c'è ampia evidenza che entrambe siano idee sbagliate: i quark non esistono ed il big bang non c'è mai stato. Ma che importa, le idee si sono affermate, ed un'idea vale l'altra quando non si capisce nulla! Quanta tristezza. Per quello che valgo io sono il figlio italiano di questa tristezza e di questa crisi. Tollerato, più che ascoltato. Rispettato un pochino, anche se noioso. Dopo tutto non posso nemmeno lamentarmi troppo perché il nostro paese da questo punto di vista è meglio degli altri. Quante carriere spezzate, quante persone isolate in giro per il mondo perché rifiutano di adeguarsi all'andazzo!

[...] Ma forse anche lei sa quanto mediocre possa essere il mondo della scienza, anche se l'avrà visto da un punto di vista favorevole perché vincente! (Selleri to Loris de Finetti, 3 February 1993-FSPA. My underline).

P.773: THE ORIGINAL ITALIAN TEXT BY SELLERI FSPA

Mi fa piacere che lei mi consideri un riferimento "globale e liberatorio". Liberare la mente dei fisici (o almeno la mia e quella di qualche amico) dalle storture del Novecento è un pò il compito che mi sono dato. Mi considerano arrogante per questo, ma io continuo a non sentirmi tale: per me è stata una scelta esistenziale, non avrei proprio potuto fare diversamente senza sentirmi infelice. La fisica è stata una scelta di vita gioiosa e non ho voluto che mi fosse strappata soltanto perché non era più di moda abbracciare il razionalismo. Non mi interessa la moda, voglio solo la verità, naturalmente nella misura del possibile. Ho cercato di capire i veri motivi di certe scelte negative e quando ci sono arrivato mi sono sembrati extrascientifici, importati dalle zone basse della filosofia (Selleri to Luciana Negro, 1 October 1999-FSPA).

PP.774-775: THE ORIGINAL ITALIAN TEXT BY SELLERI FSPA

Fin dai tempi del liceo Franco Selleri aveva sentito una forte curiosità per le questioni fondamentali della fisica. Fantasticava sugli atomi e sulle componenti subatomiche della materia (elettroni, protoni, neutroni, ...) ed era colpito dalla struttura dell'universo: I libri parlavano di centinaia di miliardi di galassie, ciascuna composta di circa cento miliardi di stelle. Il suo destino era segnato, doveva studiare fisica all'università. Così fu, ma andò incontro a delusioni proprio sulla questione atomica. Infatti la dottrina dominante delle scuole di Copenaghen e Gottinga insegnava che non si deve credere che la materia sia davvero composta di atomi. L'“ingenua concezione atomistica” al massimo poteva essere adottata per fare ordine nei fenomeni fisici, ma solo un colossale semplicismo avrebbe potuto far credere nella sua “verità”.

Era la fine delle speranze di capire il mondo in cui viviamo, ma il nostro non si rassegnò. Se le scuole dominanti avevano ragione, allora erano destinati al fallimento tutti i tentativi di dare descrizioni causali nello spazio e nel tempo dei fenomeni atomici. Quaranta anni, duecento articoli e dieci libri dopo Selleri afferma invece che c'è uno spazio logico disponibile per violare le relazioni di indeterminazione di Heisenberg e il principio di complementarità di Bohr, così come è dimostrabilmente possibile formulare le leggi del movimento in modo tale da riaffermare l'esistenza della simultaneità assoluta, negata dal principio di relatività. La conclusione è la sconfitta del pessimismo dominante la fisica del XX° secolo, mentre si apre la possibilità di rispondere positivamente alle richieste giovanili del nostro.

Si era appena diffusa la voce della sua scandalosa attività di ricerca che negli ambienti scientifici vi furono commenti, spesso ferocemente negativi per le teorie non “ortodosse”, fra le quali quelle degli atomi reali. Per difenderle Selleri non esitò a sacrificare i rapporti con altri fisici, scelta dolorosa ma necessaria per riaffermare la validità del “buon senso”.

Per fare qualche esempio, alle conferenze internazionali organizzate da Selleri ha partecipato il Gotha della fisica fondamentale mondiale: David Bohm (Londra), Olivier Costa de Beauregard (Parigi), Terletski (Mosca), Virendra Singh (Bombay), Mikio Namiki (Tokyo), John A. Wheeler (Texas), Leonard Mandel (Rochester), Henry Stapp (California), David Greenberger (New York) e numerosi altri di tutti i paesi scientificamente sviluppati,

Si deve considerare con attenzione l'interesse per le proposte scientifiche di Selleri dimostrato dai filosofi della scienza. In una lettera datata 12-2-1987 Karl Popper scrive: “Mio caro Franco, la tua lettera del 2 Dicembre con il tuo bellissimo articolo ‘Proprietà di coerenza degli amplificatori fotonici’ mi è arrivata solo oggi. Sono convinto che farà decidere a favore delle onde vuote”.

Anche gli storici della fisica hanno accolto le nuove proposte come ammissibili. Il più noto – Max Jammer – dice: “Selleri ha scritto con rigore e chiarezza un libro superbo, comprensibile e informativo per ogni persona interessata nella meccanica quantistica, che risulta stimolante provocando il pensiero anche per quelli che non condividono la convinzione filosofica dell'autore.”

Anche se i contatti con altri fisici teorici sono stati sporadici, e di breve durata la sua attività si è ben presto conquistata un generale rispetto in un ambiente scientifico in cui erano largamente dominanti idee opposte alle sue.

Per esempio, un fisico americano molto noto - D. Greenberger – scrive a proposito di un suo libro: “Ecco un libro che stavo aspettando da molto tempo, un resoconto da simpatizzante delle ragioni per cui una piccola banda di fisici crede in diverse eresie come l’incompletezza della teoria quantistica e l’ esistenza di un’interpretazione causale dei fenomeni quantistici (....). Il professor Selleri è quasi unico nell’essere qualificato per compiere questa impresa, un vero scettico che tuttavia ha una profonda comprensione dell’interpretazione di Copenhagen. Se aggiungete a questo un’onestà intellettuale sempre presente e un senso fondamentale di imparzialità che è molto evidente in questo libro, potete apprezzare di che documento speciale si tratti (....) Sono profondamente impressionato dalla profondità che Selleri ha mostrato nel presentare le sue opinioni alternative.”

Ancora più importante è il giudizio di Louis de Broglie, uno dei padri fondatori della fisica moderna. In una lettera datata 17 Maggio 1980 scrive : “Le ricerche che Selleri compie, del quale io conosco bene i lavori, sono centrate su uno dei piu’importanti problemi della meccanica ondulatoria, quello dell’interpretazione fisica della funzione d’onda (....) Posso dare un parere molto favorevole delle sue ricerche e penso che Selleri meriti pienamente d’essere aiutato e incoraggiato sia per i suoi lavori passati che per quelli che sta facendo (Notes for an autobiography?-FSPA).

As stated in the footnote n. 128 at page 82 of my thesis, I am reporting here a picture of the original letter by John S. Bell to Selleri written on 28 March 1967, as well as the Selleri’s erratum and handwritten remarks. All are concerned with the Amaldi, Selleri unpublished paper “Weak interactions and unitarity”:

SECTION 1.6-PP. 82-FOOTNOTE 128: THE WHOLE LETTER SENT BY BELL TO SELLERI;
THE SELLERI’S ERRATUM AND HANDWRITTEN REMARK

ORGANISATION EUROPÉENNE POUR LA RECHERCHE NUCLÉAIRE
CERN EUROPEAN ORGANIZATION FOR NUCLEAR RESEARCH

Téléphone: (022) 41 98 11

1211 GENÈVE 23

Telex: GENÈVE-2 25 48

Télégramme: CERNLAB-GENÈVE

Genève, 28 March 1967

Doctor Selleri
 Università degli Studi
 Istituto di Fisica "Guglielmo Marconi"
 Piazzale delle Scienze, 5
R O M A

Dear Dr Selleri,

I was very intrigued by your recent work with Amaldi on unitarity. After some effort I come to the following conception of the situation. I would be interested in your reactions.

Suppose that in some representation the S matrix is symmetric (time reversal)

$$S = \tilde{S} \quad \text{or} \quad S^+ = S^*$$

The unitarity condition is then

$$S^* S = 1$$

where $*$ denotes complex (not hermitian) conjugation. With $S = 1 - iz$ we have

$$z - z^* = iz^* z$$

With $z = x + iy$, where x and y are real, we have finally

$$(2) \quad 2y = x^2 + y^2 \quad 0 = xy - yx \quad (1)$$

This looks like two equations; however solving the second by iteration

$$y = \frac{1}{2}x^2 + \frac{1}{2}\left(\frac{1}{2}x^2\right)^2 + \dots$$

we find that the first is satisfied automatically. Thus, at least within the limits of the iteration solution, there is only one independent equation and not two. This is related to the fact that

$$S^+ S = 1 \quad \text{and} \quad S S^+ = 1$$

are not independent, but follow from one another, when S is not singular.

It would appear from the above that x can be prescribed arbitrarily. To see how one might arrive at a different conclusion consider the following example:

$$z = \begin{pmatrix} A & B & C \\ D & E & F \\ G & H & I \end{pmatrix}$$

whence by unitarity we have for C

$$C - C^* = i(A^*C + C^*I + B^*F)$$

Suppose now we substitute for A, B, F, I , first order approximations which are real. Then the imaginary and real parts of the equation give in turn

$$\text{Im } C = 0 \quad (\text{if } A \neq I)$$

$$0 = AC + CI + BF$$

whence

$$C = \frac{-BF}{A+I}$$

It would seem that C must be of the same order of the others and that its real part cannot be arbitrarily prescribed in first approximation - contradicting what was said above. However if we recall that B and F can develop imaginary parts in second order, we have at the first step, not $\text{Im } C = 0$ but

$$\text{Im } C = \text{2nd order or higher}$$

At the second step we then calculate $\text{Im } C$ explicitly in second order from an equation in which the first order real C can be given any value, including zero.

Here is a case, it seems, where the exact solution of approximate equations can be misleading.

With best regards,

John Bell

John Bell

ERRATUM:

"Weak Interactions and Unitarity"

by U. Amaldi jr. (Istituto Superiore di Sanità, Roma)
and F. Selleri (Istituto di Fisica dell'Università, Bologna)

In going from equation (13) to equation (15) some terms have been unduly neglected. These terms, when retained, show that the discriminant of the system is zero. As a consequence the result about the non-consistency of weak interactions theory with unitarity is wrong. Sorry.

Dear Bell,

The work on weak interactions and unitarity was wrong, as you suspected, perhaps for reasons different from the ones of your letter. Sorry for the time you spent with it. Now there is consistency with unitarity and only now I understand it had to be this way because of the very construction of the S-operator.

Best regards

Franco Selleri

ACKNOWLEDGMENTS

First and foremost, I am deeply indebted with Selleri's second wife, Angela Pizzi and his youngest daughter, Diana Selleri. The present dissertation would not have been possible at all without their remarkable and deep help and involvement. They have always been really available, kind and considerate in sharing with me their personal and invaluable recollections as well as in giving me the chance to have access to the whole private collection of Franco Selleri, giving me a unique, favoured insight in Selleri's life inside and outside the physical research. I will never have enough words to thank them!

I am particularly appreciative to all scholars I have interviewed: Vincenzo Paticchio, Ludvik Kostro, Augusto Garuccio, Michele Barone, Francesco Guerra, Giancarlo Scalera, Nicola Cufaro Petroni, Francesco Forti, and Adalberto Piazzoli for shedding light on some dark point in my research, and for having shared with me their personal experience, their views on physics as well as their relationship with Franco Selleri and with the common landscape he experienced in those years.

I am especially and deeply grateful to Michele Barone, who has spent a whole week with me in Bari and in Athens (where he has invited me and he has done everything he could to make me feel and warmly welcomed), recalling his past life and his deep and long lasting involvement and friendship with Franco Selleri, covering physics and private life. Michele Barone allowed me access to an impressive amount of documents of his private collection, giving me an unsurpassed view on critical correspondence never found elsewhere. Moreover, he has shown a constant interest along the path of this present work, sending me repeatedly other impressive documents. Long and friendly discussions with him and his kind wife Crigno have been really inspiring.

I would like to warmly thank Dr. Giuseppe Ventrella (and his whole staff) at AGAB-Archivio Generale Ateneo Barese of the University of Bari, for his enormous kindness and help in addition to his great passion for work. He allowed me five times to visit the Archive and to have the

opportunity to research in the personal folder of Franco Selleri and in the whole archive of Institute of Physics (later Department of Physics). I am particularly keen on appreciating his competent assistance and his great availability during my archival work, along with his warm and friendly support during my stay. I felt really welcomed over there.

I would like to express my deep gratitude to Dr. Barbara Ancarani and Barbara Alzani at the SIF Archives (ASIF) as well as to Prof. Luisa Cifarelli, President of SIF in 2018, for giving me the intriguing and rare opportunity to investigate on their Archives. I could find out a huge amount of appealing documents that shed light on Franco Selleri's critical activities within SIF during his period as a member of the Executive Board. They have been always really kind and considerate in helping me in every situation, even during the 2020 Pandemia.

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I am deeply indebted to Chip Calhoun (Digital Archivist, Niels Bohr Library & Archives), Gregory Good (The Spencer Weart Director-Center for History of Physics-American Institute of Physics), Corinne Mona (Assistant Librarian, Niels Bohr Library & Archives), and to Allison Rein (Associate Director of Library Collections & Services, Niels Bohr Library & Archives), for giving me the permission to quote several excerpts concerning the impressive interview Olival Freire Jr. made to Selleri and deposited there. All of them have been really kind and considerate and they have done everything they could to help me, beyond all imagination.

My great appreciation goes also to Prof. Olival Freire Jr, whose work is always a source of inspiration for me.

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I would like to warmly acknowledge Dr. Thomas Hainscho, at Karl Popper Copyright Office-University Library Klagenfurt (Karl Popper Sammlung of the AAU), for giving me the permission to quote several excerpts concerning the impressive correspondence between Selleri and Karl Popper, which is deposited there. And for being thoroughly available, and considerate, as well as full of deep advices.

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I wish to express a very special gratefulness to Dr. Flavio del Santo, with whom we exchanged material for the respective works. He has been always very close with his warm friendship and hospitality in Wien, following my work with great attention and sensibility. On top of everything else, he gave me concrete support, helpful discussions and exhilarating suggestions in different phases of my path, also thanks to his comprehensive and inspiring papers as historians and physicist.

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