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The SC: ISOLDE and Nuclear Structure

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- A force de frapper à coups redoublés sur la même porte, elle finit toujours par s'ouvrir. Ou alors c'est une porte voisine, qu'on n'avait pas vue qui s'entrebâille, et c'est encore plus beau.

Michel Tournier

Le roi des aulnes (1970)

1. Introduction

In 1965, towards the end of the period covered by Ulrike Mersits [mer90a] in Vol. II [hic90a], the 600 MeV synchro-cyclotron (SC) had begun to change clientele. It was to an increasing degree serving nuclear physicists, who turned towards CERN attracted by the possibility of using the secondary beams of muons and pions originally developed for particle physics as tools in nuclear-structure physics. The changeover was also accelerated by the exodus of particle physicists towards the PS and soon also the ISR. By the end of the year 1965 the nuclear research at the machine was already prominent enough to have contributed some of the most important scientific results obtained at CERN during the period 1960-65, as can be seen from a selective listing [wei66a] prepared by the outgoing Director General, Victor F. Weisskopf. This included the use of muonic x-rays for measuring quadrupole moments, studies of excited states in helium from pion interactions with lithium, and measurements of pion double charge exchange. The next development at the SC, the on-line isotope separator ISOLDE, was at that moment being prepared and over the whole period 1957-90 new applications of the SC kept appearing, as can be seen from Table 1.

It was undoubtedly the striking versatility and continual renewal of the activities around the SC that assured it such a long life. It was finally shut down on 17 December 1990 after a decision to transfer the remaining activity, the ISOLDE programme, to a new installation at the 1 GeV PS-Booster. With 33 years of active service in front-line research, CERN's synchro cyclotron must rank among the most productive accelerators in the history of physics, and it seems well motivated that a memorial symposium, called "SC 33" was held at the initiative of the Director General Carlo Rubbia to commemorate this feat. During this one-day symposium [fid92a] it was, however, not possible to cover all the main activities that had existed at the machine, and the same limitation imposes itself on the present attempt to tell the story of the machine in its middle life.

Faced with this plenitude of interesting facts I have chosen to concentrate on the interactions of the users with CERN and the associated technical and scientific metamorphoses of the SC. Four of the six main Sections are dedicated to this aspect, and describe in some detail the birth of a nuclear physics programme at CERN (Sect. 2) and of ISOLDE (Sect. 4), the fight for the cyclotron improvement programme (Sect. 5) and the last successful defence of the cyclotron 1979-81 (Sect. 7). Two Sections (3 and 6) provide a condensed coverage of the physics activities under the medium-energy and ISOLDE programmes. The aim is mainly to capture the flavour of this research, but enough references to review articles have been given to guide the interested reader to more detailed information. The main period of coverage for the present paper is 1964-81, but I have rather freely cited events and scientific results from earlier as well as from more recent times if they helped to put the activities described here in perspective.

Table 1
Physics activities at the SC

Particle Physics Electron decay of the pion g-2 Beta decay of the pion μ channel Nuclear Physics Muonic and pionic x-rays Nuclear muon capture Pion Scattering Nucleon Scattering Reactions of ^3He and heavy ions up to 87 MeV/nucleon Applications Muon-spin resonance (μSR) for solid- state work: Metals, semiconductors, polymers Radioisotopes for medicine	ISOLDE Programme Spins, moments, radii by methods from atomic physics Nuclear spectroscopy and structure Far unstable nuclei and rare decay modes Strength functions and statistical aspects of beta decay Atomic physics: x-rays, optical spectra of francium Implantation for solid-state physics Applications
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2. The Early Interest in Nuclear Physics at CERN

The two Sections (3 and 4) following this one describe some of the activities in nuclear physics around the SC during a period of roughly ten years, from the first initiatives taken by CERN in 1963 and up to the shut-down of the cyclotron for a major upgrading during 1973-74. The present Section, consisting of four, between them quite unrelated, sub-sections, serves to set the stage for the events to follow and to provide some necessary background information.

We begin in Sect. 2.1 by examining some of the mechanisms that brought the nuclear programmes at CERN into being, namely (i) initiatives from CERN, the traditional CERN method, (ii) the interaction of the users with CERN and with each other directly and (iii) through scientific conferences. Once a recognized user community was beginning to exist, it was important to create more formalized links to CERN in the form of scientific committees that could communicate with the users and give proper advice to the CERN Management. Section 2.2 describes how the committees responsible for the research at the SC were organized and functioned.

The last two sub-sections provide scientific background information. In view of the important role that was played by nuclear chemists in the creation and development of ISOLDE, it seemed useful to give (Sect. 2.3) a brief account of some of their early activities at CERN and in their home laboratories. Finally, we give (Sect. 2.4) some indications of the important themes in nuclear-physics research in the sixties and seventies.

2.1 THE CONFERENCES ON HIGH ENERGY PHYSICS AND NUCLEAR STRUCTURE AND NUCLEI FAR FROM STABILITY

A conference held at CERN during the last week of February 1963 was instrumental in launching a programme of nuclear physics at CERN. The initiative came from Victor F. Weisskopf, CERN's Director General 1961-65, who charged Torleif Ericson, responsible for nuclear theory in CERN's Theory Division, with arranging a conference in order "to bring the diverging fields of high energy and nuclear physics together once more". Co-organizer and third member of the Organizing Committee was Amos de-Shalit from the Weizmann Institute in Rehovoth. The meeting was built up around nine invited one-hour lectures, each followed by several hours of discussion. It was decided to make the lectures - or what in some cases seem to be brief summaries of them - available in the form of a CERN report [eri63a], which gives a fascinating and often very accurate pre-view of the research that was to come in this field in the next years. Of special relevance to the topics covered in this chapter are the papers by J.C. Sens on "Muons and Nuclear Structure" and by Ericson on "Interactions of Pions With Complex Nuclei". Torleif Ericson's own recent research at that time had been concerned mainly with statistical aspects of nuclear reactions, and, in particular, the discovery of what today is referred to as the "Ericson fluctuations" [eri66b], an early gift from CERN to nuclear physics. He has later told [eri91a] that not being able to find a suitable speaker on pions and nuclei he decided to take this task himself. During the preparations he became so interested in this field that it became his main occupation during the next decades. It seems, however, that not many others were swayed by the meeting, at least not directly.

The list of participants in the Conference contains relatively few of those people who were to be active in experimental research of this kind at CERN during the

following years. The attendance was largely by senior Europeans and by a surprisingly large number of Americans. It is tempting to conclude that the role of the Conference was to win recognition rather than proselytes.

Another conference series played a role in the development of research in the field of nuclei far from stability, which is the subject of Sections 4 and 6. Around 1963 time seemed ripe for an attack on this problem, which was known to require a heavy experimental effort. As will be discussed in Section 4.2 the first ideas for an experiment at CERN were put forward that year, and the following year Ingmar Bergström (Stockholm) gave lectures on the subject that were widely appreciated and later published [ber66a]. Subsequently, he organized a Conference in 1966 in Lysekil (Sweden), his home town, with most of those present who in the coming years were to contribute to the field. Many significant new results were reported among which the discovery of extreme nuclear systems such as ${}^8\text{He}$ consisting of two protons and six neutrons and of new theoretical ideas due to W.S. Swiatecki and V.M. Strutinskii on the interplay between nuclear masses and shell structure [lys66a], see Sect. 2.4. At that time the preparation of CERN's ISOLDE Programme (to be discussed in Section 4) was already far advanced. Nevertheless the Lysekil Conference and its sequels were essential in creating a strong and enthusiastic European community interested in the CERN experiments. Contrary to what was the case for the muon-pion experiments mentioned above, the ISOLDE type physics tended to be regarded with some reserve by CERN and recognition came only stepwise and over a long period. For the whole of the time period discussed in this chapter it is characteristic that the main initiatives in the fields covered by ISOLDE have come from collaborations in the member countries.

The "International Conference on High Energy Physics and Nuclear Structure" has developed into an institution, and has been held subsequently in Israel and then in many other places, latest the twelfth at M.I.T. in 1990. The "International Conference on Nuclei Far From Stability" has fared similarly; the sixth in the series will be held in Bernkastel (Mosel) in 1992.

The ideas for experiments involving on-line experiments on short-lived nuclei had not been sufficiently far advanced to be presented at the 1963 conference but there were informal contacts via CERN's Nuclear Chemistry Group, see Section 2.3. Alexis C. Pappas (Oslo) and Gösta Rudstam (CERN) had reported to Weisskopf and Ericson about the possibilities for studying far-unstable nuclei. The latter summarized the situation in a note [eri64a] which, as far as SC research was concerned, pointed to the main areas (i) scattering of nucleons, (ii) muonic x-rays and nuclear capture, (iii) scattering of pions, (iv) pionic x-rays, and (v) production "by special techniques" of radioisotopes far from the stability line and with very short half-lives. The main ingredients for the CERN nuclear-structure programme were now in place and Weisskopf in a letter dated 13 February (pap90a) invited a number of physicists from inside and outside CERN to a meeting, which took place at CERN already on 24 February. The participants¹ expressed their support for nuclear-structure research at CERN and Weisskopf closed the meeting by encouraging proposals for experiments at the SC.

¹ Present were according to the minutes [pap90a]: G. Backenstoss, Aa. Bohr, G. Charpak, L. Dick, P.M. Endt, T. Ericson, L. Foldy, W. Gentner, K. Gottfried, P. Lapostolle, E.G. Michaelis, A. Pappas, P. Preiswerk, G. Rudstam, J.C. Sens, K. Siegbahn, V. Soergel, P.G. Sona, I. Talmi, J. Teillac, J. Thirion, L. van Hove, J.D. Walecka, V.F. Weisskopf and D. Wilkinson.

2.2 CERN'S NUCLEAR STRUCTURE COMMITTEE AND OTHER SCIENTIFIC COMMITTEES

The activities described above had established the interest at CERN and in the member countries in a nuclear programme at CERN and the next necessary step from CERN's side now was to set up a structure that could give recommendations on the new research. This meant fitting nuclear research into CERN's system of scientific committees, which, as discussed in some detail in the paper by Pestre [pes90a], had come into existence during the years 1959-62. We outline here some of its main features.

The scientific committees at CERN serve as advisors to the Director General, who according to the statutes is the sole responsible for the scientific programme of the laboratory. The committee structure existing in 1964 had been accepted by Council in its meeting on 8-9 December 1960. Under the new organization, the body advising the Director General directly, the Nuclear Physics Research Committee (NPRC), would receive input from the three scientific committees: the Emulsion Experiments Committee (EmC), the Track Chamber Experiments Committee, and the Electronics Experiments Committee (EEC). Since the three committees had jurisdiction at both the PS and the SC, it was necessary to introduce also two scheduling committees, which took care of allocations of machine time, and which reported directly to the NPRC. The EmC in its meeting on December 15, 1960 also decided to create an executive sub-committee, the "Working Party" [emc60a]. At its start the EmC Committee had 20 members in addition to the two machine coordinators and *ex officio* members from the CERN Directorate and machines. The meetings were held as closed meetings, but it was announced that one representative per group should be invited to one of the three annual meetings of the Committee.

Together with the head of CERN's NP Division, Peter Preiswerk, Torleif Ericson now proposed [eri64b] to the Director General to create a Nuclear Structure Committee. (We note for the uninitiated that NP stands for "nuclear physics", and that the Division was responsible for CERN's counter experiments. Thus "nuclear" here as well as in the name CERN itself and in the name NPRC mentioned above could be translated as "subatomic"; it has nothing to do with the sense, in which we talk about nuclear physics in the present chapter.) The Nuclear Structure Committee (NSC) was soon after set up with Ericson as chairman and among the first members were Neil W. Tanner (Oxford) and O.B. Nielsen (Copenhagen), but the complete list of members is not known; we have not found the minutes of the first (and fourth) meetings [vho90a].

The importance of the emulsion technique was, however, decreasing rapidly. Following a proposal by Gregory, Ekspong and van Hove [vho66a] it was already in the spring of 1966 decided to merge the EmC with the NSC. The proposal was presented by Wolfgang Paul, the Physics I Director, in the EmC Meeting [eme66a] on 24 May and in the NSC Meeting² on 12 June. The new body, which was named the Physics III Committee, had as chairman and vice-chairman Gösta Ekspong and Torleif Ericson. The structure was parallel to that of the (re-organized) Track Chamber Committee; it consisted of a "Parliament" (the expression used by Paul) with one official representative of each interested group and a small "Preparatory Committee". It was decided that the

² According to the minutes [nsc66a] of this, the seventh and last, meeting of the NSC, the following were present: Backenstoss, Brianti, Ericson, Herz, Kjelberg, Michaelis, Nielsen, Paul, Soergel.

latter should be composed of Ekspong, Ericson, Paul, and two representatives of each of the two communities thus united: emulsions and nuclear structure. This two-chamber structure relatively quickly developed into the system that has prevailed up to now (1991) for all CERN committees, that of an open meeting with free access followed by the "closed committee meeting". As the name "Preparatory Committee" indicates, the mode of operation was different in the late sixties: The closed meeting, which usually was held in the morning, preceded the open meeting with public presentations and discussions [fid90a] after which the Chairman pronounced the Committee's verdict. This sometimes left the impression that the decisions had been made before the case had been heard, and the preparatory meeting was occasionally jokingly referred to as the "troisième bureau", the most secret of the French secret services.

In 1976 the PH III Committee was re-named the SC Committee (SCC), which existed until 1978. In the years 1966-78 the chairmanship was held successively by G. Ekspong, O. Kofoed-Hansen, D.H. Wilkinson and V. Soergel. A new committee (PSCC) was formed in 1978 to advise on research both at the SC and at the PS, where the nuclear and medium-energy programme at LEAR was beginning to play a prominent role. This committee, which existed until the shut-down of the SC in 1990, was successively chaired by R. Klapisch, P.G. Hansen, H.J. Specht and M.J. Albrow.

2.3 STUDIES OF COMPLEX NUCLEAR REACTIONS BY RADIOCHEMICAL METHODS

Chemical techniques have from the very first beginning played a central role in the development of nuclear physics. The experimental discovery of fission by Otto Hahn and Fritz Strassmann in 1938 and the subsequent discoveries of the transuranic elements and of a host of new radioactivities from fission were all due to chemists working in the borderline field between physics and chemistry, a discipline usually referred to as "nuclear chemistry". The history, achievements and status of this field up to the fifties can be found in an interesting and informative book by J. Haïssinsky [hai57a]. Nuclear Chemistry had been provided with a foothold at CERN from the very beginnings of the laboratory, and has been encountered briefly in the previous volume of *The History of CERN* [mer90b]. Because of the important role that nuclear chemists (together with physicists working in nuclear structure and with isotope separation, see Sect. 4) played in creating ISOLDE, we give here a brief account of the start of nuclear chemistry at CERN.

The science of nuclear chemistry had been born and developed in Europe, notably in France, Germany and Great Britain, but the basic research associated with the nuclear programmes during the war had led to a new blossoming of the field in the United States and Canada. Nevertheless it was a Norwegian nuclear chemist who brought this discipline to CERN. Alexis C. Pappas had been introduced to radiochemical techniques at the University of Oslo by Ellen Glædisch, herself a student of Madame Curie, and had worked as a visitor at the Radium Institute in Paris. During a 2 1/2 year stay (1949-51) at MIT with the group of C.D. Coryell, one of the leading experts on fission, Pappas had studied the "delayed neutrons" emitted from certain fission products. (The word "delayed" here refers to a decay mode proceeding via beta decay to particle-unstable excited levels so that the particles appear with the long lifetime characteristic of the weak interaction. Similar processes will be encountered again in Sections 4 and 6.)

Back again in Norway, Pappas looked for international contacts and experimental facilities that would allow him to continue his research. Prompted by the British chemist Sir Geoffrey Wilkinson he wrote a letter (4 August 1952, in [pap90a]) to Aage Bohr in Copenhagen and essentially offered his services: "In Scandinavia there are very few nuclear chemists and I see it as one of my duties to spread knowledge about this branch of science and to train young scientists". The reply (11 August 1952, in [pap90a]) came immediately from Niels Bohr himself, who invited Pappas to Copenhagen and suggested that he should concentrate his efforts on studies of fission and spallation using the 170 MeV proton beam of the Uppsala synchro-cyclotron, which just had been completed and which "had been placed at the disposal of The European Council for Nuclear Research", i.e. the embryonic CERN. This led to a fruitful collaboration with The Gustaf Werner Institute in Uppsala, later re-named the The Svedberg Laboratory, and involving the Swedish nuclear chemist Gösta Rudstam, the Norwegian Arve Kjelberg, Pappas's first research student, and others. An important collaboration partner and one of Europe's leading laboratories in the field of nuclear chemistry was Günter Herrmann's Institut für Kernchemie at Mainz University.

In a letter with CERN letterhead, dated 21 October 1953, Niels Bohr on behalf of the Theoretical Study Group, placed in Copenhagen and a forerunner for the Theory Division, invited Pappas to join the group as a consultant, an activity that lasted until 1957. It was therefore in all respects a logical choice when Wolfgang Gentner (see mer90b) asked Pappas to help building up CERN's nuclear chemistry laboratory and to extend the research begun at Uppsala to higher energies. The laboratory was ready in 1958 with Rudstam as the first group leader (1959-65) and Kjelberg as the second (1965-70) and with Pappas continuing to serve as a consultant for the Group.

High-energy nuclear reactions, in general, produce many isotopes of each element, and in order to resolve these complex mixtures the nuclear chemists decided to take up a physical method, electromagnetic isotope separation, see Sub-section 4.1. The combination of this technique with chemistry provides samples selected in mass number A and in atomic number Z , thus consisting of only one radioisotope. The isotope separator was designed by Göran Andersson, the specialist in this subject at Uppsala's Gustaf Werner Institute, and was ready in 1961. This technique was to become the basis of the ISOLDE programme, see Sect. 4.

The scientific programme and policy of the Nuclear Chemistry Group as of 1961 was outlined in a memorandum by Gentner and collaborators [gen61a] to Weisskopf. The main activity was cross-section measurements (fission and spallation) at 600 MeV and 24 GeV, mainly carried out in order to understand the mechanism of complex reactions, but some experiments aimed at applications such as the understanding of radioactivities produced by cosmic rays in meteorites and the absolute determination of cross-sections that could serve as beam monitors in other CERN experiments. The Group also carried out a number of experiments that exploited the extreme selectivity and sensitivity of the radiochemical technique to search for rare processes, in particular those involving the production of mesons.

The main contribution during the early years of the Nuclear Chemistry Group was to the systematics of high-energy reaction cross-sections. A very successful attempt by Rudstam [rud66a] to provide an empirical formula for spallation cross-sections is probably one of the most cited results of the group and one that has served as a guide in many later experiments.

Radiochemical and mass-spectrometric research continued to be carried out at the SC and also at the PS, but in later years largely by external teams. As a particularly interesting example we mention the experiments by Raisbeck and Yiou [rai71a] dealing with formation cross-sections and half-lives of presumably cosmic-ray produced light elements (Li, Be, B).

2.4 SOME OPEN PROBLEMS IN NUCLEAR PHYSICS IN THE SIXTIES AND SEVENTIES

In the thirties and forties the nucleus had been viewed as an obscure, densely packed conglomerate of strongly interacting particles. This picture was behind the two most successful theories of the epoch, Niels Bohr's compound nucleus model and von Weizsäcker's liquid-drop model, but although able to explain many features of nuclear reactions and binding they offered little help in understanding the inside of the "black box" that the nucleus continued to be. The 1948 shell model of M.G. Mayer and J.H.D. Jensen appeared as the great liberating miracle that opened the way to the understanding of nuclear structure and spectroscopy at low energies. At the heels of this followed the clarification of the role of nuclear collective motion, the vibrations and rotations, and their relationship with the shell structure. Decisive contributions were made by Aa. Bohr and B.R. Mottelson. The elaboration of the single-particle orbits of non-spherical nuclei in 1955 by S.G. Nilsson owed much to the emerging art of electronic computing for physics, an art that was to become essential to the development of a quantitative nuclear theory over the coming years. The book by Bohr and Mottelson [boh69a] provides an overview of nuclear-structure theory in the beginning of the seventies.

From the mid-sixties and on it was becoming increasingly clear that although the shell model provides a convenient starting point, it is far from representing the final truth, and that it is of great interest to probe its limitations. Many experiments were undertaken to investigate precisely the nuclear charge and momentum distributions, the associated problem of two-nucleon momentum correlations, and the role of sub-structures ("clusters") formed by groups of nucleons. Important tools were the scattering of electrons on nuclei and also the x-rays emitted from electronic and muonic atoms. Owing to the Pauli principle, it is only at higher energy that it is possible to probe the deep-lying states of the nucleus, and reactions such as $(p,2p)$, $(e,e'p)$ and (π,π) were used to this end.

The development was strongly stimulated by some unexpected experimental findings. The heavier nuclei turned out, in spite of their large Coulomb energy, which violates isospin symmetry, to have sharp isobaric analogue states which gave rise to a surge of interest in isospin structure in nuclei. This research was during the sixties and seventies extended to other collective states at high energy (giant quadrupole resonance, Gamow-Teller resonance), which furnish other examples of persistent symmetries.

Of far-reaching importance was the discovery in 1962 by S.M. Polikanov et al. of the "fission isomers". These are excited states of heavy fissionable nuclei. They have surprisingly long lifetimes for electromagnetic decays and equally surprisingly short lifetimes for spontaneous fission. For the latter the enhancement as compared with normal nuclei of the same element and mass amounts to factors of 10^{10} or more. It was soon realized that these isomers represented states with extremely large quadrupole deformations (cigar-shapes) and that this meant a very thin fission barrier and consequently a large probability for tunneling of the fission fragments through the barrier. The question was now why these shapes were so close to being stable.

Techniques for calculating total binding energies and equilibrium shapes were developed, especially by W.J. Swiatecki, S.G. Nilsson and V.M. Strutinsky ("Strutinsky renormalization"), see e.g. [nil72a, sup74a] and Fig. 1, and had great impact also on the understanding of binding and masses in other regions of the nuclear chart, see Fig. 2.

The nuclear physics community paid particular attention to the theoretical predictions of a region of possibly long-lived "super-heavy" elements, isotopes with atomic number near 114 and neutron number near 184 (predicted to be the magic numbers corresponding to the isotope $^{298}\text{Eka-Lead}$ [sup74a]). Many experiments were undertaken to search for these new elements, but the experiments were much harder than had been hoped for, and it is only during the eighties that convincing evidence for the existence of this island of stability has been obtained, largely by P. Armbruster, G. Münzenberg and their collaborators [arm85a, mun88a], who have discovered isotopes of the elements with $Z=107$, 108 and 109.

Nuclear physics is a rich but also complex subject and it is not possible to identify a single frontier line for the research similar to the role that the high-energy frontier has played for particle physics, but we can maybe point to some of the discernible lines: (i) The high Z frontier, i.e. that of the super-heavy elements just discussed. (ii) The isospin frontier, that is the regions in Fig. 2 with very high and low isospin projection $T_z=(N-Z)/2$. This research has been a major subject at ISOLDE and will be discussed in Sect. 6. (iii) The energy frontier, the interactions of nuclei with high-energy probes such as pions, nucleons and fast heavy ions. Examples of such research at the SC will be discussed in Sections 3 and 5.5. (iv) The angular momentum frontier, largely the domain of heavy-ion accelerators at relatively moderate energies. (v) The low-spin frontier, concerned with spatial and momentum distributions of the nuclear ground state and low-lying excited states. This kind of research uses a large number of probes: electrons, muons, photons, pions. Examples of this research at the SC will be given in Sections 3 and 6.

3. Experiments With Muons and Pions

Already by the end of 1964 the scientific programme at the SC had become almost entirely nuclear, as may be seen from an example: In the NPRC Meeting on 17 February 1965 [npr60a] the SC Coordinator, E. Zavattini, gave a summary of experiments that would be on the floor during the coming six months. These included: (i) Studies of nuclear π^- capture at rest (Cernigoi), (ii) Pion production at 600 MeV (Michaelis), (iii) Muonic x-ray studies (Backenstoss-Sens), (iv) Study of the reaction $\pi^+ + {}^6\text{Li} \rightarrow 2p + \alpha$ (Charpak), (v) Inelastic scattering of π^- on light nuclei (Meunier, Spighel and Stroot), and (vi) Background tests for a crystal spectrometer (Nilsson) intended for muonic x-rays. At the following meeting one more experiment had been added dealing with quadrupole interactions of muons with deformed nuclei (Macq). The tools for this research, the beam lines around the SC have been described previously [mer90a], so the following Sections will deal with the physics that can be done with beams of muons and pions.

3.1 MUONIC X-RAYS

The muon may be viewed essentially as a heavy electron. It has electromagnetic and weak but not strong interactions with nuclei, and its large mass, 207 electron masses, means that the muonic atoms formed by the negative muon have smaller dimensions and larger transition energies by a corresponding factor as compared with electronic atoms. It turns out that both sizes and transition energies are essentially on a nuclear scale, and for this reason the negative muon is an extremely valuable probe for nuclear structure. For light nuclei even the most bound states of muonic atoms have the muon well outside the nucleus and hence resemble the hydrogen atom, while for heavy nuclei the muon is mainly inside for the most bound states. At the energies discussed in this chapter the muon-nucleus interaction is believed to be governed completely by the laws of quantum electrodynamics (QED) so that the interpretation of experiments is conveniently free from ambiguities. (Conversely, measurements on muonic atoms furnish some very accurate checks on QED.) The positive muon resembles from the point of view of atomic and molecular physics a light hydrogen nucleus; it has found applications in solid-state and molecular physics.

The powerful new muon channel at the SC [mer90c] had not escaped the attention of nuclear and atomic physicists, but the machine was crowded and "machine time was allocated in hours rather than in shifts" to quote the German (Freiburg) physicist Gerhard Backenstoss [bac91a], who was one of the young physicists around CERN biding his time for a go at the muonic atoms. During a stay in the United States he had had difficulty in being accepted in nuclear physics laboratories, where it was at that time still difficult for foreign nationals to obtain clearance. Inspired by a seminar by Sergio di Benedetti on muonic atoms, he finally had arranged a stay at Pittsburgh, where he learned that the leading lights in the field including also the Stearns and the theorist L. Wolfenstein were all at CERN [bac91a].

Back again at CERN in 1959 as a fellow, Backenstoss had his first chance when Wolfgang Gentner, head of the SC, brought him in touch with Peter Brix from Darmstadt. Brix, together with Hans Kopfermann from Heidelberg, had discovered [bri49a] the first cases of strong nuclear quadrupole deformations (in the rare earths, $Z=60-64$) via the observation of very large isotope shifts in the atomic spectra. As one of Europe's leading experts on nuclear sizes and shapes, his group had the right

background for launching the first experiment [bri62a] in Europe to study charge distributions from the x-rays emitted from muonic atoms. The weak point, however, was the gamma detection, made with thallium-activated sodium iodide crystals. The study concentrated on the $2p \rightarrow 1s$ transition but the resolution was insufficient for resolving the fine structure ($2p_{3/2}, 2p_{1/2}$) splitting, which could be observed only in the $3d \rightarrow 2p$ transition and only for the heaviest elements. In a game where precision was absolutely essential this was clearly an unsatisfactory situation, as could also be seen from parallel and previous American experiments [ste57a, joh62a].

Another physicist, who was keeping an eye on the field, was J.C. (Hans) Sens from Holland, who had obtained his Ph.D. at the University of Chicago working with V.-L. Telegdi on μ^- capture in nuclei. From his review of muonic atoms given at the 1963 Conference, it is clear that Sens had followed the subject closely and that he was thoroughly familiar with its theory. The possibility for a break-through came in 1964, when nuclear physicists began to use lithium-compensated germanium diodes ("Ge(Li) detectors") and later pure ("intrinsic") germanium diodes for gamma-ray spectroscopy. At CERN this new technique had a quick start. A letter [ack65a] submitted already in January 1965 reported some early results on both spherical and deformed nuclei obtained with a small (2 mm thick) commercial Ge(Li) detector with quite mediocre resolving power. It was, however, essential quickly to get better detectors.

Both Sens [sen91a] and Backenstoss [bac91a] emphasize the important role played by Ernst Baldinger from the University of Basel, who at that time was studying radiation damage to transistors. He accepted to attempt to make germanium detectors and within 2-3 months succeeded in fabricating specimens with a thickness of 6 mm. (Thickness is above all essential for stopping the photoelectron inside the sensitive volume.) With these, much better results were obtained, first for spherical nuclei [ack65a] of 18 elements ranging from Cl to Bi. With a resolution of the germanium detector that was still much inferior (20-24 keV full width at half maximum) to what could be achieved just a few years later, there was no longer any problem in resolving the fine structure, and the paper gave a detailed discussion of the intensity rules, governed mainly by statistical weights, but where there are some significant deviations. The main thrust of the paper was, however, the determination of the parameters for the nuclear charge distributions, which were parametrized as the so-called Fermi distribution

$$\rho(r) = N(1 + \exp[4\ln 3(r-c)/t])^{-1}$$

where c is the radius at one half of the central charge density, t the skin thickness, i.e. the distance over which the nuclear charge density drops from 90% to 10% of the central charge density, and N a normalization constant. The correlations between the two parameters were demonstrated in (t, c) plots, similar to the way previous electron-scattering data had been presented. The point is that a single transition energy for a given isotope determines a curve in the (t, c) plane. An intersection between two curves defines a parameter set that is consistent for the two transitions. A general area of best fit, approximately valid for all nuclei studied, was $t=2.2$ fm and $c=1.12 A^{-1/3}$ fm, in excellent agreement with the values previously obtained in electron scattering. Somewhat similar results were found in a subsequent study [dew67a] of deformed nuclei in the lanthanide and actinide regions. For the lightest nuclei (B-Cl) the muon is mainly outside of the nucleus, and the x-ray energies are no longer sensitive to the details of the charge distribution and give [bac67a] the mean value of the square of the charge

radius as the only information, exactly as is the case for the corresponding optical transitions in (electronic) atoms, see Sects.4.6 and 6.2.

In this first phase of muonic-atom experiments the main purpose had been to use muons to learn about the radial distribution of nuclear charge. The results had been important in providing a confirmation by another technique with different sources of systematic errors of the picture already arrived at from scattering of high-energy electrons on nuclei. However, since no great surprises emerged, it is understandable that the programme soon changed character. Sens decided to take up experiments at the PS, and Backenstoss, as we shall see in the following, to an increasing degree initiated experiments involving x-rays from the capture of negatively charged hadrons in nuclei. Around that time Brix's group from Darmstadt, which we have encountered in the beginning of this section, re-appeared on the scene. In a proposal presented by Roland Engfer on 31 January 1967 the group expressed its wish to conduct a second programme on exotic atoms. From the minutes (PH III-67-1) it appears that the Committee was hesitant about permitting a second independent effort at the same machine and in a fast-moving and very competitive field. It recommended tests together with Backenstoss's group to be followed by a review "whether there should be one or several groups in this area". It was the second solution that was to prevail in a spirit of good collaboration between the groups.

3.2 PIONS AND NUCLEI

While the muon provides a clean probe for studying nuclear problems, the pion (or π -meson) is itself part of the problem. It interacts strongly with the nucleons, is spinless and carries isospin 1 and hence forms an isospin triplet π , π^0 , π^+ . Identified early as Yukawa's particle and carrier of the strong force, the charged pion's mass of 273 electron masses implies that the long-range part of its field has a decay length of order of its Compton wave length $\hbar/m_\pi c \approx 1.4$ fm, a quantity corresponding roughly the range of the nucleon-nucleon force and to the internucleon distance in a nucleus. For a pion interacting with a nucleus, the pion field therefore has additional contributions from the virtual pions in the nuclear medium. This is a problem analogous to that of the propagation of a photon through a dielectric medium.

Modern particle physics teaches that the pion is not an elementary particle but, like all mesons, composed of a quark and an anti-quark. Thus the negative pion has the formula ($\bar{u}d$) and it is not point-like but has a root-mean-square charge radius of 0.66 fm. One would therefore tend to believe that this would mean that a description of nuclei in terms of quark structures would be more effective, but this does not seem to be the case for the low excitation energies, say below 1 GeV. The reason is that the essential degrees of freedom at low energies involve only the lowest states of the hadron spectrum. For the mesons these are $\pi(140)$ and the two vector (spin 1) mesons $\rho(770)$ and $\omega(783)$, and for nucleons the ground state $N(938)$ and first excited state, the broad $\Delta(1232)$ which has both spin and isospin equal to 3/2. With these states as the essential ingredients, modern medium-energy theory is able to describe a wide variety of phenomena in pion nucleus interactions as can be seen from the recent book on this subject by Ericson and Weise [eri88a] and from which we cite two examples. The deuteron is the classical example of a loosely bound system in nuclear physics, as the root-mean-square distance between the two nucleons is as large as 4 fm. For this reason, the properties of the deuteron are described very accurately by the simplest pion field, the one-pion exchange potential (OPEP), which is responsible for the nucleon-nucleon

force at large distances. For more dense systems collective spin-isospin modes of low frequency appear; we have already in Sect. 2.4 had occasion to mention the Gamow-Teller giant resonance.

The SC played a central role in the early development of π -nucleus physics. As outlined briefly in Sect. 2.4, the main emphasis in nuclear physics in the early sixties was on nuclear spectroscopy, which served to test nuclear models, and there was no particular reason to believe that a rare and expensive projectile such as the pion could give information that would not be readily obtained with the more abundant conventional projectiles. As mentioned in Sect. 2.1, Torleif Ericson had at the conference in February 1963 [eri63a] given arguments why pions were especially interesting for exploring the nuclear interior together with a first presentation of his and Magda Ericson's theory [eri66a] for the propagation of pions in nuclear matter. He pointed out that the important experiments were pion scattering from nuclei and measurements of x-rays from pionic atoms. We give here and in the following two Sections some examples of how the experimentalists responded to this challenge. More information can be found in a recent article by Ericson [eri92a].

The observation of x-rays from pionic atoms permits the study of the π -nucleus interaction at zero kinetic energy. The formation of pionic atoms with the emission of x-rays had been observed by a number of groups (see the review by Backenstoss [bac70a] and also a recent retrospection [bac92a]) but as for the case of muonic atoms, it was only with the advent of the lithium-drifted germanium counter that more precise investigations became possible. A first round of experiments [bac67b] studied the $2p \rightarrow 1s$ transitions of light elements and demonstrated that the pionic x-rays involving the $1s$ state are shifted in energy and also dramatically broadened by the strong interaction, see Fig. 4. The pace was fierce in this early boom of exotic-atom research: Quite similar results had just before been obtained by the Berkeley group [jen66a]. For the heavier nuclei, the s -states become too broad to permit the observation of x-rays connecting to these states, and a subsequent experiment [poe68a] succeeded in observing shifts and widths for the $3d \rightarrow 2s$ transitions in the elements Al to Zn. The results were in good agreement with the theory of Ericson and Ericson [eri66a]. Subsequently the $4f \rightarrow 3d$ ($Z=49-59$) and $5g \rightarrow 4f$ ($Z=73-83$) transitions were studied [sch68a]. It is maybe worth pointing out that for the cases in which the line broadening is too small to be observed directly, it may still be possible (see [bac70a]) to determine the level widths via the line intensities, which reflect the competition between radiative decay and absorption. The pionic atom data could be analyzed in terms of the parameters of the pion-nucleus interaction potential. The systematics of the real and imaginary parts of this potential has been discussed by Hufner et al. [huf74a].

3.3 TESTS OF QUANTUM ELECTRODYNAMICS AND THE MASSES OF THE PION AND THE MUON

The theory of quantum electrodynamics (QED) is believed to be exact as has been shown in a number of precise experiments, see e.g. the review by Mohr [moh77a]. These experiments, of which "g-2" discussed in [mer90a] is one example, can best be performed on simple atomic systems, for which accurate predictions are possible. The classical example is, of course, the $2p_{1/2}-2s_{1/2}$ energy difference in the hydrogen atom, which led to the discovery of the Lamb shift and to the subsequent development of QED theory. Since the main terms in the Lamb shift involving self energy and vacuum polarization corrections depend strongly on the atomic number Z it is interesting to

perform tests in the strong electric field of heavier nuclei. Although these effects are observable in the x-ray transition energies of neutral atoms the theory of a many-electron system cannot be exact, and the most precise tests require hydrogen-like systems. (Presently such experiments are being performed on highly stripped heavy ions at high energies.)

Muonic atoms offer the advantage that they are always hydrogen-like because the muon is much nearer to the nucleus than the electrons so that electron screening of the field is negligible or a small correction. Since we have seen in Sect. 3.1 that the transition energies can be very sensitive to the nuclear size, it would seem that this effect would be a serious limitation, but it can usually be avoided by studying transitions between states with high angular momentum, and for which the muon wave function at the nucleus is essentially zero.

The effect of the nuclear size was, in fact, the main limitation in an ingenious experiment on the $2p_{3/2}$ - $2s_{1/2}$ fine structure of muonic helium carried out at the SC by Emilio Zavattini, Jean Duclos and their collaborators [zav77a, ber75a]. This experiment represented a long-haul effort begun around 1970. The basic idea is simple. The $2s$ state¹ lies below the $2p$ state and it is long-lived because one-photon decay to the $1s$ ground state is forbidden. (Electric dipole radiation is excluded by the parity selection rule, and magnetic dipole radiation by the orthogonality of the $1s$ and $2s$ wave spatial wave functions.) The level decays mainly via the beta decay of the muon and to about 20% via the emission of two photons. It was shown that this state is formed in the capture of muons in helium. The $2p_{3/2}$ level lies about 1.5 eV higher, an energy difference due predominantly to the contribution from vacuum polarization while those from the fine structure and nuclear size are about an order of magnitude smaller. The essential trick was now to induce the $2s_{1/2} \rightarrow 2p_{3/2}$ transition by means of a powerful pulsed laser operating in the infrared (the transition energy corresponds to a wave length of 8100 Å) and synchronized with the arrival of a muon in the helium target. The formation of the p state was revealed through the emission of the usual K_α x-ray. In a scan of x-ray count rate versus laser wave length a resonance was found with the expected width of about 1.5 meV (milli electron volt), a splendid energy resolution for a medium-energy physics experiment, and leading to a transition energy of 1527.5 ± 0.3 meV. The theoretical value for the transition energy is 1535 ± 9 meV based on a root-mean-square radius for ^4He of 1.650 ± 0.0025 fm determined from electron scattering. If the validity of QED is assumed, the experimental result may also be read as an improved determination of $\langle r^2 \rangle$ for helium.

Other experiments at the SC approached the problem of QED corrections via measurements on heavy atoms with germanium detectors, which offer an energy resolution of the order of 1 keV (kilo electron volt), comparable to the effect of 1-2 keV expected. The effects of nuclear size were avoided by studying transitions between circular orbits with high angular momentum such as $5g \rightarrow 4f$ (for nuclei near lead) or $4f \rightarrow 3d$ (for nuclei near barium). Early results by Backenstoss et al. [bac70b, bac73a], by Walter et al. [wal72a] and also parallel work in North America found indications for a small deviations from theory, of the order of two standard deviations. Subsequent experiments at the SC by Ludwig Tauscher and collaborators [tau75a] and with an

¹ We remind readers not familiar with atomic physics that states with orbital angular momentum $l=0,1,2,3,4,\dots$ are denoted $s,p,d,f,g,\dots$

energy resolution now improved to 450 eV found agreement with theory within error limits of 12-16 eV, so that the QED corrections had been verified to a relative precision of 0.5%. Quite similar results were simultaneously obtained by a Canadian-American group [dix75a].

The comparisons of measured and calculated transition energies in muonic atoms discussed here presupposes that the muon mass is accurately known from other sources. This is, in fact, the case today: The most accurate determination of the muon mass comes from the ratio of the magnetic moments of the muon and the electron, but older measurements are based on transition energies in muonic atoms, as can be seen from the compilation by the Particle Data Group [par88a]. Likewise it is possible to determine the charged pion mass from the energies of such transitions in which the corrections to the Klein-Gordon equation (the appropriate relativistic equation for a particle with spin zero) for QED effects and strong-interaction shifts are manageably small. An early accurate measurement of the pion mass was reported by Backenstoss et al. [bac73a]. The currently most accurate value for the ratio of the pion and electron masses has been measured [jec86a] at the Swiss meson factory SIN to be 273.1268 ± 0.0007 .

3.4 SCATTERING AND PRODUCTION OF PIONS ON NUCLEI

A process that early had captured the imagination of many nuclear physicists was that of pion double charge exchange (DCE) such as ${}^9\text{Be}(\pi^-, \pi^+){}^9\text{He}$. This process, discussed in detail in [eri88a], must take place as a two-step reaction involving two pion-nucleon interactions. Its final states are, in general, highly excited continuum states and the cross-sections for producing individual bound final states are usually small. (Nevertheless DCE has recently served to give information on states in the unbound and extremely neutron-rich nucleus ${}^9\text{He}$, see my comment [han86a].) The DCE process was observed early at the SC by the so-called MSS Group (an abbreviation for "Meunier, Spighele and Stroet"), and they made a valiant attempt to find the tetra-neutron [gil64a]. It was, however with the intensities of that time impossible to observe DCE to individual final states.

The MSS collaboration set out in 1963 [str91a] to develop a spectrometer capable of studying scattering of pions. The task was a difficult one, not only because of the low intensity of the pion beam, but also because excellent resolution was needed in order to resolve individual final states in the nucleus. The group over the following years succeeded in building an instrument that had sufficient resolving power to separate individual levels in certain light nuclei. The essential trick, which later became a standard feature for spectrometers in intermediate-energy physics, was to use a "double achromatic spectrometer", that is one in which the beam of pions is analyzed in the first magnet and the scattered particles are re-focused by the second magnet, independently of their initial momentum since the total dispersion in the two arms of the spectrometer is zero. (The analogy with similar arrangements in optics explains why the instrument is called achromatic.) Particles that have suffered inelastic collisions in the target appear elsewhere in the focal plane, so that cross sections for several states are recorded simultaneously. In this way it was possible to measure final states with an effective momentum resolution of 0.5-1% although the incident beam from the first magnet had a momentum bite of 2%.

The spectrometer was first used for a study of π^- scattering on carbon. The results shown in Fig. 3 show a characteristic diffraction pattern in which the interference becomes especially pronounced at 180 MeV kinetic energy [bin70a] demonstrating that

the pion has a very short mean free path in nuclear matter at energies near the Δ resonance. This experiment was the first to demonstrate the role of the Δ in nuclei. A second experiment by the same group [bin71a] studied Coulomb-nuclear interference across the resonance region.

Another group, usually referred to as the Göteborg-Oxford group, studied the production of pions in nuclear collisions. One interesting result was the observation [dom70a] of pionic capture of a proton into bound states, a process that requires a very large momentum transfer and that therefore had been regarded as forbidden. The explanation clearly must involve the collective effect of several nucleons. Similar phenomena have since been observed in a number of other reactions. The same group also investigated [wil73a] effects of charge symmetry and Coulomb distortion in the scattering of positive and negative pions on light nuclei.

3.5 OTHER EXPERIMENTS WITH MUONS

In addition to other work already mentioned, the Engfer-Kankeleit group focused on some of the more subtle features of the muon-nucleus interaction in order to learn about nuclear structure. We give a few examples of the flavour of this work. In the first experiments they studied not x-rays but nuclear gamma rays emitted from states in the target nucleus that have been excited by the x-ray cascade². These gamma rays are emitted while the muon still is present, so the total energy contains a contribution from the muon-nucleus interaction. If the two states have different size, it is found that the transition energy is changed by an amount referred to as the "isomer shift", a finite-size effect of the same kind as that encountered in muonic x-rays (Sect. 3.1) and in optical transitions (Sects. 4.6 and 6.2). This phenomenon is pronounced for the rotational first-excited 2^+ levels of strongly deformed nuclei ($Z=62-76$), which are easily excited and which have [bad68a] different transition energies than those of a free nucleus - or rather a nucleus surrounded by its electrons. (The high resolution for γ -rays of the Mössbauer effect has permitted similar observations on "electronic atoms".) For the heavy element such as thallium ($Z=81$), the coupling of the muon to the nucleus via the magnetic dipole interaction is strong enough for the doublet to be resolved [bad68b]. To give an example of the order of magnitude of this effect, the ground state and first excited state at 203.7 keV in ^{205}Tl are split by 2.3 keV and 1.1 keV, respectively [bac72a]. Like other weak and electromagnetic processes in nuclei, the capture of muons is strongly influenced by giant-resonance phenomena, but is for technical reasons little studied. Petitjean et al. [pet71a] approached this problem for the isotopes of Europium by measuring delayed gamma rays and deduced neutron multiplicities, linked to the excitation energies. A survey of results from a large number of nuclei was given by Backe et al. [bac74a].

During the sixties and seventies a group from Louvain headed by Jules Deutsch and Pierre Macq carried out a number of experiments on weak interactions in nuclei. We take as an example the one that they themselves presented on a recent festive occasion [deu92a], namely the measurement of the parity of the ^{11}Be ground state. The problem was that suspicion had arisen that this state, corresponding to that of the seventh

² These excitations may be thought of as a "tidal wave" in the nucleus that is pulled around by the orbiting muon. This means that the effect is linked to the nuclear quadrupole deformation. Just as in real tidal waves, resonance effects are important.

neutron, was not characterized by the spin-parity combination $1/2^-$, as would be expected from the shell model, but that it was rather $1/2^+$. In order to determine the parity the group measured [deu68a] the direct muon capture rates to the ^{11}Be ground and 320 keV first excited and found them to be forbidden and allowed, respectively. Since the target nucleus, ^{11}B , is known to have negative parity this means that the ground state was indeed $1/2^+$. This was the first example of what today is called an intruder state.

There was also during the sixties a growing interest in the possible application of muonic x-ray spectroscopy to studies of the solid state, for example by detecting shifts in the x-ray intensity ratios for pairs of transitions in the same element imbedded in different chemical compounds. Similar observations were at about the same time being made elsewhere, see e.g. [tau68a,dan68a]. A much more important application that today forms a major research field is the use of muon spin rotation, often abbreviated μSR . This technique exploits the parity-violating asymmetric beta decay of the positive muon for detecting the precession of the muon in a magnetic field. (There are similar, but less important, applications of negative muons.) The primary parameters detected are the precession frequency (there may be more than one) and the attenuation of the correlation due to muon depolarization. The many applications of μSR in solid-state physics and chemistry have been reviewed by Brewer and Crowe [bre78a]. The first proposal to conduct such a programme at the reconstructed SC was made [kar74a] to the Physics III Committee by Erik Karlsson, Ola Hartmann and Lars-Olaf Norlin (Uppsala), just at the moment when the reconstructed SC was getting ready to receive users. During the second half of the seventies and until the mid-eighties a broad collaborative programme in μSR was conducted at the SC by a number of groups including, to name just a few participants, Cesare Bucci (Parma), D. Richter (Jülich) and E. Walker (The Rutherford Laboratory). A good impression of the versatility of this field can be obtained from the proceedings of specialized conferences [muo79a, muo81a] covering the period in question. Applications of μSR in metal physics have been reviewed by Karlsson [kar82a]. Several broad reviews can be found in [exo77a, exo79a].

3.6 LOOKING BACK

This Section has given some examples of the contributions made by the muon-pion programmes at the SC during the golden period from roughly 1965 to 1975. An in-depth assessment of the impact of the work at CERN in this field would, however, have to look in detail at a broader time window and also at simultaneous activities elsewhere, something that clearly falls outside of the scope of the present paper. We note that muon-pion experiments with nuclei had their first beginnings in the U.S.A., and that the Chicago and Columbia cyclotrons and to a lesser degree other machines were very serious competitors throughout the period in question. Especially in the exotic-atom research, the groups were racing neck and neck as can be seen from several cases of similar and almost simultaneous publications. (The reason for this situation should be clear from Sect. 3.1: The advent of one new technique, the germanium diode for gamma-ray spectroscopy developed in nuclear physics, had suddenly opened the floodgates to a host of interesting problems waiting to be answered.) We also believe that some of the most valuable contributions made by the CERN muon-pion experiments are the programmes that they generated at subsequent machines. Already around 1970 some of the physicists at CERN had begun to shift to the PS to do experiments on K^- , antiprotons and Σ^- , and during the first half of the seventies the pion and muon experiments moved to the Swiss meson factory SIN (today the Paul Scherrer Institute

in Villigen, Switzerland), where the intensities were much more favourable. At about the same time similar and also very powerful American and Canadian facilities (LAMPF, TRIUMPH) sprang into operation, and an important medium-energy programme developed at SATURNE (Saclay).

It is clear, however, that during the period discussed here, the SC groups working with muons and pions were very well placed, maybe leading, in the competition on a world scale. The strong interaction between experiment and theory was an important ingredient in this success. Although the present paper does not cover the contributions made by theory, we can at this point at least try to mention some of the names of those, visitors and staff of CERN's Theory Division, who played an important role: J. Bernabeu, J. Blomqvist, M. and T.E.O. Ericson, J. Hüfner, M.P. Locher, F. Myhrer, E. Oset, W. Weise and C. Wilkin. To them and many others goes much of the credit implied in the remark made by Sens [sen91a] to the effect that the experiments at CERN generally were well conceived and were better analyzed than those of the competing groups.

In the next Section we shall discuss the ISOLDE Programme during roughly the same time period of time, and we shall see that its characteristics were in almost all respects the opposite of those mentioned. ISOLDE was very much an isolated effort with only modest competition on the world scale; no floodgates had opened and the effort in the early years was concentrated on difficult and only in the long run rewarding experimental problems, which had little glamour and very limited theoretical support. My own view, at the time under discussion as well as today, was and is that the muon-pion programme clearly was more topical, more timely and more interesting than ISOLDE.

4. The Early ISOLDE

The new frontier represented by the boundaries of the nuclear chart (Fig.2) was one that attracted many nuclear physicists and chemists in the mid sixties. Theoretical ideas had emerged as to what new physics one might learn there (Sect. 2.1) and new techniques were being developed [lys66a]. Of these electromagnetic mass separation appeared especially promising because it offered the cleanest conditions and consequently high sensitivity to rare events. From the experimental point of view, however, the problem was difficult, but it was encouraging that one group already in 1950-51 had succeeded in carrying out a successful pilot experiment, which is described in Sect. 4.1. The rest of the Section gives an account of the creation of the ISOLDE Collaboration and of its early research programme.

4.1 ON-LINE MASS SEPARATION, COPENHAGEN 1950-51

A wide variety of techniques are used for electromagnetic isotope separation; well known are the mass spectrographs used for measuring relative abundances of ions or for measuring atomic masses. Another variety is the large-scale isotope separators called calutrons that were used in the nuclear-weapons programmes. European laboratories were active during the fifties and sixties in developing an intermediate type instrument, usually referred to as "isotope separators", of laboratory size and to be used as a tool for nuclear physics research to purify mixtures of stable or radioactive isotopes. There was much international collaboration and many informal contacts between the European physicists¹ working in this field. The development of this specialty can be traced through a conference series [ele65a], and a number of those involved played a role in developing the field of on-line mass separation.

The first experiment in which an isotope separator was connected directly to an accelerator was carried out in Copenhagen in 1951 by Otto Kofoed-Hansen and Karl Ove Nielsen [kof51a], both working at Universitetets Institut for Teoretisk Fysik (UITF, later re-named the Niels Bohr Institute). The experiment ran for a short time only but nevertheless played an important role as a demonstration of the feasibility of the on-line technique and of its great power for sorting out the components of a complex reaction mixture. The experiment, although simple, incorporated all the same elements as a modern on-line experiment: The Copenhagen cyclotron produced fast neutrons from an internal beryllium target and these bombarded an external target consisting of 10 kg of a mixture of uranium oxide and baking powder (essentially $(\text{NH}_4)_2\text{CO}_3$). The decomposition products (NH_3 , CO_2 and H_2O) of the latter served to sweep volatile fission products, mainly krypton and xenon, the two noble gases in fission, along through a 5 cm diameter metal tube towards the electromagnetic isotope separator. Just before the ion source the carrier gases were removed in a cold trap. The experiment was further complicated by the necessity of keeping the long metal tube, connecting the two machines, on the acceleration voltage of the separator, 50 kilovolt. The measurements of radioactivity were carried out with very simple detection systems, essentially Geiger-Müller counters, and permitted the discovery of several new isotopes of krypton and

¹ Some of those involved were Ingmar Bergström and Göran Andersson (Uppsala, later Stockholm and Gothenburg, respectively), René Bernas (Paris, later Orsay), J. Kistemaker (Amsterdam), K.O. Nielsen (in Aarhus from 1961), W. Paul (Bonn), and many others.

their descendants. The experiment came to an end somewhat prematurely. During the first part of the fifties the UITF was constructing new buildings and the cyclotron was moved to a new area, where, unfortunately, it was too far away from the isotope separator to permit a continuation of this research, something that was frequently suggested by visitors to the Institute in the coming years.

The 25th anniversary of this experiment was marked in 1976 in Cargèse (Corsica, France) during the 3rd International Conference on Nuclei Far From Stability, a sequel to the Lysekil Meeting (Sub-section 2.1). In a lively talk, the spirit of which is well preserved in the written version [kof76a], Kofoed-Hansen contrasted the primitive experimental techniques of 1951 with those of 25 years later. In fact, said he, not even the term "on-line" had been known in 1951. (It entered physics in the early 1960s with the use of digital computers). He also pointed out that the experiments had been pursued with a single well-defined aim, namely to discover new noble-gas radioactivities that could be used in experiments to detect the neutrino via its (missing) recoil momentum. Around 1950, Kofoed-Hansen and other workers at the UITF had carried out several experiments of this kind, which served to provide an underpinning for Pauli's "neutrino hypothesis". The point is here that the neutrino had been invoked to account for missing energy and angular momentum; consequently it was important to check that the hypothesis predicted the correct amount of missing linear momentum. One of the isotopes discovered (^{89}Kr) was, in fact, used in such an experiment [kof51b], but we know today that its decay scheme is far too complicated to permit any simple interpretation of the results.

The purpose of the Copenhagen experiment thus was not an all-out attack on the problem of the far-unstable radioactive isotopes; the problem as such was hardly recognized at the time, and the primitive detection methods of the period did not allow much information to be extracted from a complex decay with many beta- and gamma-rays. In fact, one technical reason behind the emergence of this problem in the mid-sixties was that solid-state detectors based on silicon and germanium had just been developed and permitted studies of particles and gamma rays with high resolution and good efficiency.

4.2 THE ISOLDE COLLABORATION IS FORMED

Although many physicists were interested in on-line mass separation, it was the nuclear chemists at CERN who had their hands on the best accelerator in Europe for such a purpose, the 600 MeV synchro-cyclotron. In 1963 the first outline of a plan for placing an electromagnetic isotope separator in the external beam of the SC was circulated as an internal NP Division report by Rudstam and Andersson (rud63a), the latter now at Chalmers Technical University in Gothenburg. Following Weisskopf's call for proposals (see Sub-section 2.2) the Nuclear Chemistry Group organized a meeting at CERN on 10 April 1964 in which scientific aims and organization of the project were discussed. There was general enthusiasm for the idea and it was among other things decided to have the separator planned by a working party². Later other working parties

² The members of the working party were: G. Andersson (chairman), R. Bernas (Orsay), B. Hedin (CERN-SC, see ref. [pes90a] pp 98-100), K.O. Nielsen (Aarhus), and O. Skilbreid (Copenhagen). Among the other participants in the meeting were R. Foucher (Orsay), T. Ericson (CERN), E. Hagebø (CERN), A. Kjelberg (CERN), O.B. Nielsen (Copenhagen), A.C. Pappas (Oslo), G. Rudstam (CERN), S. Sundell (CERN) and R.J. Walen (Orsay), as can be seen from the minutes in [iso91a].

appeared, one on target and chemical separation methods and one on data handling and electronics. More will be said about the former in Section 4.4. The minutes and progress reports from these various committees and sub-committees can be found in the ISOLDE files [iso91a] and give a month-to-month picture of the development over the next 3 1/2 years, up to the first beam on target.

In a series of meetings from May to September 1964 the Working Party arrived at what was essentially the final design, see Fig. 5, and in which one should note that the target-ion-source unit is separated from the analyzing magnet by a drift tube, which traverses a shielding wall, and which is displaced from the target plane in order to avoid fast neutrons streaming from the target into the experimental area. A second collaboration meeting was held on 9 November 1964 [iso91a] and it was decided to submit a final proposal to CERN. Other important decisions were to set up a "Finance Committee" to look after common funds and to let the construction work on the isotope separator "commence in Denmark, probably at Aarhus". The meeting also considered a name for the programme; among those suggested were EBIS (external beam isotope separator), COLIS (CERN on-line isotope separator) and ISOL (isotope separator on line). None of them were adopted.

In a memorandum of 26 October 1964 the chairman of the Nuclear Structure Committee, Torleif Ericson, recommended the on-line isotope separator to the NRPC, saying that the technical support (from the external groups) was fully adequate, that Western Europe was in a leading position in this field, and that the NSC considered "the project to be of high scientific value". The proposal was presented by Ericson in the NRPC on 2 December 1964 and was approved [npr64a], and on 17 December 1964 the Director General invited the groups to carry out the experiment.

The "Finance Committee" was subsequently set up with members Bernas, Gentner, K.O. Nielsen, Pappas and Rudstam, but already at its first meeting on 24 April 1965 [iso91a] it decided that the committee was too small. In order to assure a reasonable degree of attendance at all meetings it was felt necessary to have two members from each of the six "countries" (including CERN) leading to a total of 12 members. The term country here stands as an abbreviation for "group of laboratories from a given country participating in the Collaboration". There were five such groups: France, Germany and the three Scandinavian countries. Rudstam was elected chairman of the Committee. It was also realized that the name "Finance Committee" had other connotations at CERN, and it was decided "until a better name was found" to call the project ISOLDE (from isotope separator on line) and hence the committee the ISOLDE Committee. The financial basis for the collaboration was decided by fixing the contribution to common funds to one and the same amount for all: 60 000 S.Fr. per country and year. This money, a total of 360,000 SFr/year, was used by the ISOLDE Committee to cover common expenditures, in particular for the isotope separator and its associated equipment. It should be remembered, of course, that the individual laboratories in addition had to bear the full cost of their experimentation at ISOLDE. Thus, in one meeting the Collaboration had arrived essentially at an organization and mode of operation that was to last for the whole of the period under discussion in the present paper.

Still during the same meeting the Committee was informed by K.O. Nielsen that the construction of the separator would start in Aarhus in July 1965 and that an engineer and a technician had been found for this job. Another important problem was brought up by the Deputy Division Leader of MSC, Ernst G. Michaelis, who together with

Preiswerk attended part-time. He pointed out that the radiation levels outside the CERN site would exceed the permissible levels unless the proton beam were passed through a beam tunnel about six meters below ground level to an underground irradiation cave. Any other solution would limit the permissible running time to an unacceptable degree, especially in view of the possibility of an increase in the intensity of the extracted beam, maybe by an order of magnitude. The very far-sighted solution with the underground hall was studied further by the MSC and in due course adopted by CERN; it was an essential ingredient in making it possible to continue the programme (as ISOLDE-2) after the completion of the SC Improvement Programme (Section 5). Another meeting of the ISOLDE Committee took place on 17 September the same year; it was concerned mainly with organizational and administrative matters.

The ISOLDE Committee met again¹ at CERN on 22 April 1966 and examined the progress in the construction of buildings and the separator, in target development and in electronics and data handling. A sign that the first experiments were coming near was that the Committee decided to appoint a coordinator for the experiments at ISOLDE. The choice fell on the Nuclear Chemistry group leader, the Norwegian Arve Kjelberg, a forceful personality with considerable diplomatic talents.

On the same day the NSC Chairman T. Ericson joined the Committee for a discussion of scientific priorities and the groups were asked to outline the research topics that they wished to tackle first. The most interesting result of this part of the meeting, however, was that a procedure was adopted for interfacing the Collaboration with the CERN scientific committees. The problems that made ISOLDE differ from other CERN experiments were clear enough: ISOLDE was not an experiment with a single purpose but a general tool, a "facility", that could be called upon to provide a wide variety of radioactive beams. Some of these beams would be easy, some hard and some impossible to make. Furthermore it was already clear that the programme would be composed of many different experiments and that the final blend would have first to take both feasibility and scientific merit into account and also that some concessions would have to be given to the taste - or lack of the same - of the individual groups. The solution found by Ericson and the ISOLDE Committee was to demand the individual experimental teams to prepare internal proposals on the basis of which ISOLDE would set its priorities and prepare a menu, a joint proposal to be submitted annually or perhaps bi-annually to the NSC and from there on through the normal CERN channels. Thus, formally ISOLDE was one experiment requesting machine time, i.e. the proton beam, from CERN, just like any other experiment. In reality the situation was more delicate. The ISOLDE Committee would often during the years to come have to face external criticism that it was receiving "block time" and that it was using the strong parts of the programme to let less significant parts tail-gate. Conversely there was occasionally muted internal criticism that ISOLDE was using CERN as a bogeyman to suppress certain parts of the programme, but there never was a case in which a group was sufficiently convinced of its case to take it to the CERN committees, which would have been its right. The fact that this system was to function unchanged for 15 years is probably the best sign that it worked well and served the interests of all involved, and

¹ Present at this Meeting were: G. Andersson (Gothenburg), R. Bernas (Orsay), R. Brandt (Heidelberg), G. Brianti (CERN), A. Kjelberg (CERN), K.O. Nielsen (Aarhus), O.B. Nielsen (Copenhagen), A.C. Pappas (Oslo), B. Povh (representing W. Gentner, Heidelberg) and G. Rudstam (Studsвик).

elements of it were retained when the re-organization came in 1981, after the end of the period covered in this Chapter.

Proposals for the initial experiments at ISOLDE were received at the next Committee Meeting (21 October 1966) and formed the basis for a proposal [iso91b] accepted by the Physics III Committee in its meeting on 31 January 1967. The time zero was now approaching and on 29 May 1967 the Committee held its last meeting before the start of the experiments. As could be expected it was concentrated around monitoring the progress in the preparations. At Aarhus University, there were now two teams working in parallel on the isotope separator, one doing mechanical tests and preparing parts for shipment, the other doing ion-optical tests. Beam lines for the transport of a mass-separated beam from the exit of the dispersion chamber to the experiment were discussed by René Bernas and Ole Bent Nielsen, who had a first version under construction in Copenhagen. Two ion sources had already been delivered from Aarhus. The Division Leader of the MSC Division, Giorgio Brianti, reported that the underground building now was ready, and that the proton beam had been tested and was better than had been promised. A technical detail illustrates the care with which CERN worked. The health physics group had monitored the radiation background with proton beam on and found that it was acceptable except in one case, that of an accidental spill of the proton beam upstream in the long beam line. This would lead to dose rates up to 10 times the tolerance level. Brianti proposed to make the area fail-safe (and therefore accessible to personnel during runs) by extending the shielding wall by 40 cm to a total of 290 cm. To understand this point it should be remembered that the very conservative CERN safety practice did not permit the use of active safety systems, for example a monitor that would interrupt a beam under certain conditions. (No nuclear reactor would work without such techniques). Personnel safety was ensured by passive devices that excluded a faulty condition, for example by a beam blocker that was moved mechanically into the beam before personnel could enter a certain area. For the future work at the separator the free access to the experimental hall turned out to be vital; some of the more delicate experiments at ISOLDE would require frequent or even constant supervision.

Finally the nuclear chemists were almost ready with the targets, which will be discussed in more detail in Sect. 4.4. With beam-on less than five months away the programme was under control and the minutes of the next ISOLDE Committee Meeting show that the first 3 shifts of tests with proton beam on the target went well. The ISOLDE documents do not mention the date of the event, which was 16 October 1967 according to the CERN Courier (7, 206). ISOLDE had successfully made the transition from a project to a running programme. We now turn to the techniques and the physics behind this.

4.3 THE ISOLDE FACILITY

The installation that had come into being through 3 1/2 years of effort is shown schematically in Fig. 5, which is taken from the detailed report published by the Collaboration following the first two years of operation of ISOLDE [kje70a]. The long external beam line from the cyclotron served the ISOLDE target-ion-source unit placed behind a 3 m shielding wall separating the target area from the experimental area. The beam could be deflected onto a second target position and beam dump, foreseen for particle physics experiments. This was at the time a valuable addition to the capabilities

of the SC since the other experimental areas around the machine did not have sufficient shielding to permit the use of the full proton beam.

The radiation safety problems were severe in this inner sanctum of ISOLDE, and increasingly so over the years as progress was made in proton beam intensity and target techniques. Unlike other "hot" spots at CERN the problem was not only external radiation but also the risk of severe radioactive contamination; after all, the targets served to release volatile radioactivity including alpha emitters in large quantities. From a safety point of view it was therefore somewhat unfortunate that access to the zones of high radiation levels necessarily passed through the main experimental area where many and, usually, rather undisciplined external users worked. As a first of many steps to improve safety, the original design had foreseen two cavities in the floor to serve for temporary storage of irradiated target units; since these were highly radioactive it was convenient to let them cool there and then at a later time remove them to the chemistry laboratories for post mortems and possibly repairs. It is actually remarkable that in 23 years of operation there never were any serious cases of contamination, which must be credited jointly to the house group and to CERN's health physics services for many years represented by Jan Tuyn, efficient but also level-headed and unbureaucratic.

On the other side of the shielding wall was the main experimental area, see Fig. 5. The separator, its control console and high-voltage supplies together with the access paths from the lift (to the left) take up the larger part of the room and leave little place for the experiments. Fortunately some electronics including the on-line computer could be placed in the room above, that is at first-basement level.

The isotope separator was described in a paper by G. Andersson, H.E. Jørgensen and K.O. Nielsen [and70a]. For the benefit of the reader more used to high energy experiments, it is maybe useful to inject at this point that the beam steering of slow heavy ions is best done with electrostatic elements, which are smaller and cheaper than corresponding magnetic elements. An isotope separator therefore has only one magnetic element, the magnet that takes care of the momentum analysis. All other elements, deflectors and quadrupoles, are energy-focusing and all settings are independent of mass, i.e. they are at a fixed proportion of the acceleration voltage, the absolute value of which is unimportant to a first approximation. The main design features of the ISOLDE isotope separator were taken from the machine that Andersson had constructed for the Nuclear Chemistry Group some years before [and57a] based on fringing-field focusing with a 55° bending angle. The mean radius of curvature was chosen as 1.5 m leading to a dispersion at the focal plane $D=1500/A$ mm, where A is the mass number of the central beam. In order to be able to run stable mass markers of a given element simultaneously with a far-unstable isotope the instrument had a very large momentum bite: $\pm 15\%$ relative to the central mass.

The vacuum tank had been constructed so that the dispersion chamber could be taken out without moving the magnet and collection chamber, which would have changed the alignment of the spectrometer. This feature was useful because radiation safety imposed periodical removal of disposable linings placed inside the dispersion chamber to collect radioactive contamination. The almost parallel beams (of different masses) on the exit side and with a small convergence favoured simultaneous experiments on different mass components in the beam. Another feature of the geometry was that the focal plane was at an oblique angle (about 30°) to the beam direction, but as the arm of the spectrometer was long this did not lead to any practical difficulties, the beams being practically parallel and pencil-shaped. This can easily be appreciated

from the drawings given by Camplan and Sundell [cam70a]. The analyzing magnet of the separator was constructed as an H-type magnet with the yoke closed on both sides of the pole gap so that the magnetic stray fields were very small. This was done [and70a] so that it would be possible to operate magnetic spectrometers for beta rays and conversion electrons in the vicinity of the isotope separator.

The design called for an acceleration voltage of 100 kV, somewhat higher than is usual for laboratory isotope separators. This choice had been made in order to reduce the effects of space charge (a more serious problem at ISOLDE because of the 3 m long transfer tube for the unseparated beam) and also to reduce the blow-up by scattering on residual gas. During the beam tests carried out by Camplan and Sundell [cam70a] it turned out to be difficult to reach 100 kV and the tests and, in fact, all future running took place at 60 kV, and probably not much was lost by that. Part of the problem in going to higher voltages was condensation on water-cooled parts of the high-voltage end; the humid Geneva summers always brought with them a new high in high-voltage break-downs. The beam mapping showed that the machine worked well; Camplan and Sundell curiously enough refrained from citing a resolving power and only said that more measurements "will be undertaken to give more representative data", probably to avoid offending sensible souls by admitting that the nominal resolution of 1500 had not yet been reached. From Figure 6 in [cam70a] the resolution ($M/\Delta M_{\text{FWHM}}$) may be estimated to be around 700, not far from the typical value for the next many years of operation of ISOLDE. The cross-contamination was very low, with mass number 133 as the central mass of the order of $2 \cdot 10^{-4}$ at mass positions 132 and 134.

4.4 THE HEART OF THE MATTER: TARGETS AND ION SOURCES

The creation of ISOLDE as told in Sub-section 4.2 was essentially an application of known techniques in experimental physics, except for one link in the chain. It was, to take one specific example, known technology to let the 600 MeV proton beam of the SC hit a kilogram of molten lead and produce the mercury isotope with mass 179 and half-life of 1 second at a rate of a few atoms per second. (This isotope was, of course, unknown before ISOLDE). Once this activity appeared as ions $^{179}\text{Hg}^+$ in the acceleration gap of the machine it was also known technology to make a beam, to momentum-analyze it in a magnet and to steer it to experiments. The problem was the step that lies in between: To liberate the interesting atoms from all bulk material (which would suffocate the ion source) and from interfering radioactive impurities, to transport the interesting atoms and to ionize them, all within a second. This was the kind of problem the Nuclear Chemistry Group and its collaboration partners had offered to solve, and for which each chemical element, in principle, posed a separate problem.

The Group had set up a "Working Party on Target and Chemical Separation Methods" which coordinated the Research in the different groups. It held a first meeting at CERN on 21 April 1966 with 32 participants from 9 laboratories and later a "Seminar on ISOLDE Chemistry Problems" two weeks after the first ISOLDE runs [che66a]. The result was a considerable number of ideas and attempts; early in 1967 there were a total of 11 different systems under investigation. It was also clear that under Kjelberg's and Rudstam's able direction great care was taken that the most promising devices, the so called "cold" and "hot" systems, would advance quickly and be ready in time. The ion sources were not at that stage of ISOLDE's programme a subject of research; essentially all the work was done with a plasma ion source with oscillating electrons trapped in a

magnetic field and with axial extraction of the ions, the so-called Nielsen source [nie57a]. We outline some of the results from the target chemistry.

The noble gases were produced from the cold targets. Since emanation (radon) from radium is a noble gas, it is not surprising that the release of this element from various compounds had been studied by the old radiochemists. Paul Patzelt, a radiochemist from Günter Herrmann's group in Mainz, was familiar with the classical studies, which had shown that organic salts such as palmitates and stearates of the alkaline earths were the best emanators. He quickly realized that these compounds were too vulnerable to radiation damage and also too bulky to do the job and that hydrous oxides of elements with valency four (zirconium, cerium and thorium) were better, as could be shown in off-line investigations. Once put on-line these systems turned out to give excellent yields of krypton, xenon, and radon, respectively [pat70a] from (p,5pxn) reactions, which posed no problem at the high energy available at the SC. These targets then were little else than a vacuum container containing the pre-dried hydroxide, connected to and pumped through the ion source.

The hot targets, primarily developed by Einar Hagebø and Stig Sundell [hag70a], were contained in a small vacuum oven surrounded by a number of heat screens. The oven could be heated to 1500°, enough to melt a number of metals and to distil some volatile metals. This arrangement was used for producing isotopes of mercury from (p,3pxn) spallation reactions on a target of molten lead, which was contained in graphite trays, and also for producing the chemical homologue cadmium from molten tin. To prevent the products from condensing, the oven had to be connected to the ion source via a heated transfer tube as shown in Fig. 6, which gives a delightful impression of the pioneering spirit behind this research. The hot systems also performed well on line and together with the cold systems provided ISOLDE the main workhorses for the coming years: Cd, Hg, Kr, Xe, and Rn, and, of course, all isotopes that could be formed as radioactive decay products of these five.

During the first on-line tests the Nuclear Chemistry Group studied [hag70b] the important question of the delay between production of an isotope and its ionization. Since the flight time of the ion can be neglected being about 50 microseconds, the delay came from the target-ion-source unit and could be studied on-line by interrupting the proton beam and observing the continued arrival of a long-lived product. For both types of systems the delay curve was a single exponential. In the case of the noble gases the mean delay time of 15-30 seconds for, apparently determined by geometrical factors, while for the hot systems, the delay times were minutes or longer, and strongly temperature-dependent. In the latter case the corresponding activation energy for the release of cadmium from molten tin was 19 kcal/mole, similar to the heat of evaporation of cadmium of 24 kcal/mole, which suggested that the rate-determining step was the release of cadmium from the tin surface. (It is maybe useful to point out that these relatively long average delay times t_d do not exclude the observation of much shorter average lifetimes t . The experiments then work on the small part of the radioactivity that comes out early, the yield being of order t/t_d .)

4.5 THE FIRST EXPERIMENTS

An overview of the achievements of ISOLDE for the whole period in question in this paper will be given later (Sect. 6); the following paragraphs represent an attempt to give an impression of the style and flavour of the early experiments.

Although ISOLDE had been created by a large collaboration involving external laboratories and CERN, it had from the beginning been clear that a joint scientific utilization was unthinkable. Many different experiments were possible at the new facility, each requiring the effort of no more than a few scientists, the typical nuclear-physics group of the sixties. This meant that there were untold possibilities for internal competition and rivalries at ISOLDE, an aspect that had made the question of interfacing ISOLDE with CERN's committee system both so important and so delicate (Sect. 4.2). Bearing in mind the problem of possible internal feuds the ISOLDE Committee had already before the start of the experiments made considerable efforts to identify potentially explosive issues and to defuse them. In the ISOLDE Committee Meeting on 17 September 1965 it had even been discussed whether it was necessary to provide ISOLDE with a "convention". Later, an internal agreement had been reached on individual areas of main scientific interests, so to say protected hunting grounds, and the ordering of the names of authors on future papers became the subject of a Committee decision¹ on 4 December 1967 [iso91a], just after the start of the experiments.

Some of the younger scientists in the Collaboration including the present writer tended at the time to regard the Committee's interest in rules and regulations as a sign of fussiness, but in retrospect the avoidance of unnecessary conflict was probably one important factor in shaping the extraordinary internal coherence that made it possible for ISOLDE to survive for such a long time at CERN. There may be something in this that reflects the strong Scandinavian² contribution to ISOLDE during its early years.

The start of ISOLDE was characterized by great enthusiasm and also by a keen feeling of pressing internal competition. At the same time it was essential that all scientific and technical experience gained should be communicated immediately to the other groups. To this end the participating groups were requested to stay behind after the end of their machine time for what was called "After-Run Meetings". In these, all new observations were discussed and written summaries were later mailed to all ISOLDE users. (Note that many groups, maybe most, did not have permanent residents at CERN.) This was possible since the data taking was still simple (mainly multichannel analyzers) and key results such as half-lives of newly discovered isotopes would usually have been extracted already during the experiment.

The conditions at ISOLDE during the early years is well illustrated by the following extracts from a preamble, which accompanied an information newsheet concerning the experiments in January-February 1970. The Nuclear Chemistry (NC) Group Leader, Arve Kjelberg, wrote [kje70b] under the heading "Rules of the Game":

- 1. Too many participated simultaneously during some of the experiments! This made it next to impossible to keep the operation going in an effective, not to say safe, way.*
- 2. Many of the participants were unknown to me when they came - and some when they left...*

¹ The ordering was to be alphabetical by home institute, a system that has persisted until quite recently. From the same meeting dates the tradition, still surviving to this day, to let the name ISOLDE Collaboration figure, usually as a "corporate author", on the title page of papers.

² Scandinavians view themselves as shy of conflict. They, in general, prefer to suffer silently rather than to run the risk of being branded troublemakers. In addition, they are able to remember their grievances over remarkably long periods, somewhat like the Pope's mule in Daudet's short story.

6. *There was a radiation incident during the repair of the ion source during the last day of the radon run...*

8. *...And do not bring any more equipment for the main ISOLDE room... Too many people try to do too many things at the same time without making sure that we all know the intention, with the result that none work well - and that temperaments get short - even mine!*

9. *The noise in the experimental hall has now reached insupportable levels...*

10. *The number of keys to the NC laboratories is restricted. Rotraud Mohr tries her best, by alternatively smiling and shouting, to get the keys back before the culprits leave. So, give back your key - or no more smile next time!*

From this you may deduce that you, dear visitors, are not loved so much as you were used to. I am afraid this is reflecting the problems foreseen on a global scale as a consequence of the population explosion. These days there are ways to improve such situations.

We now turn to the experimental arrangements that were on the floor when ISOLDE went into operation in October 1967. Only one experimental set-up [lin70a], labelled #24 on Fig. 5, was mounted at an external beam line. This technique, which gives maximum freedom for shaping the detector arrangement, was in just a few years to become the one and only way to do experiments at ISOLDE. The radioactivity contained in the mass-separated beam was collected on a tape that could be moved by a step-motor system to allow precise control of the collection and measuring times. Furthermore any long-lived daughter products or impurities associated with the beam were continually moved away with the tape and hence prevented from building up near the detectors. For a fixed setting of the isotope separator, the apparatus covered three mass numbers near $A=100$ and five near $A=200$, but larger movements required shifting the central mass of the separator, which could only be done freely if there were no other users working in parallel.

Three other experimental arrangements, situated near #19,23 on Fig. 5 and described in [kje70a], had been prepared to operate inside the collector chamber of the separator and hence had considerably more freedom to intercept any potentially interesting mass number. The fact that much ingenuity had gone into building these collection systems reflected the competitive situation as it was anticipated by the individual groups. The so-called tape collector system intercepted the beam in the focal plane of the separator and moved the radioactivity to beta and gamma detectors placed outside the vacuum chamber. A second system built by the Gothenburg group [alp70a] also operated in the focal plane and was designed for discovering particle emission (alphas and protons). Named the tau-meter, this instrument collected the radioactivity on tiny metal discs which subsequently were dropped in front of a cooled solid-state detector. The third system, the so called end-strip collector, consisted of an aluminium foil that covered the area where the comb of mass-separated beams hit the rear end of the collector chamber. This arrangement was originally meant as a disposable lining that would allow radioactive contamination to be removed periodically from the separator, but soon it was modified so that the foil could be exchanged via an air-lock during runs. These foils were a prolific source of samples for off-line experiments with radioactivity with half-lives longer than a few minutes. In the early years of ISOLDE operation it was probably foils from the end-strip collector that supported the largest volume of research.

Although the experimental arrangements operating inside the collector chamber were a transient phenomenon, characteristic of the early ISOLDE, they were important

in augmenting the scientific output in a period when machine time was scarce. The point was that once a beam of one particular mass was in use at a given beam port, then all other masses were available essentially for free inside the chamber. A fourth arrangement of this kind was added a little later by an Aarhus-CERN-Gothenburg team [hag73a]. In this several masses of presumed short-lived proton emitters were intercepted on a spinning disc and carried in front of nuclear emulsion plates. The half-life of a new emitter could now be deduced from the angular distribution of the protons on the plate and the proton energy spectrum from an analysis of the track lengths. This experiment must surely represent one of the last applications of emulsions as spectrometers in nuclear spectroscopy.

The first tests with proton beam used the cold targets (Sect. 4.4) to produce isotopes of xenon (16 October 1967) and radon (5-6 December 1967). The performance of ISOLDE was extremely satisfactory. During the radon run, the yields were scanned across the mass range from $A=201$ to $A=226$ and were found to be distributed as shown in Fig. 7. The bell-shaped yield curve with a fast drop to zero at low and high mass values is typical of isotope production in high-energy reactions, and we give here a brief explanation of it. The dominant reaction of 600 MeV protons on a heavy element such as thorium ($Z=90$) is fission, which cannot produce radon. About one in hundred of the struck thorium nuclei escapes fission and undergoes the so-called spallation process, which may be thought of as proceeding in two stages. The first step is a fast collision cascade in which nucleons and larger fragments are removed from the target nucleus. The residue from this will usually be in a highly excited state and will cool primarily by the evaporation of neutrons, whereas proton evaporation is suppressed by the Coulomb barrier. When the composition of the residue approaches that corresponding to the proton drip line, the neutron binding energies have increased to 12-14 MeV and the proton binding energies decreased to almost zero. Proton evaporation now sets in and halts the production of more neutron-deficient isotopes, thus creating the sharp drop in the yields at the low-mass end of the distribution. The neutron-rich products can only be produced in rare events in which primarily protons have been knocked out in the fast cascade, and in which the residue is left with very little excitation energy, so that it cannot loose neutrons by evaporation. This explains the fast drop on the high-mass side. For spallation on lighter targets the fission competition is absent and the spallation yields are higher, see the yields for producing caesium ($Z=55$) from lanthanum ($Z=57$) shown in Fig. 7 (The measured yields are also influenced by decay losses in the target. This aspect was touched upon in Sect. 4.4.)

The first year of operation of ISOLDE was spent surveying the isotopes of six elements, four noble gases (Ar, Kr, Xe, and Rn) from the cold targets and mercury and cadmium from the hot targets. During that period about 15 new isotopes were discovered and significant new information was found for about 40 other isotopes. One interesting observation was that beta-delayed emission of protons, impeded by the Coulomb barrier, turned out to be detectable for extremely neutron-deficient isotopes of elements as heavy as xenon ($Z=54$) and mercury ($Z=80$) [hor71a]. It was decided first to publish jointly all this information on gross decay characteristics. ISOLDE's first research paper [han69b], had 29 authors, a number that nobody would find strange today, but which caused some lifted eyebrows when it appeared in Physics Letters in January 1969. The publication was, of course, nothing but a comprehensive collection of raw data with no explicit physics point attached to it, but it was a powerful

demonstration of the strength of the new experimental facility that had come into being at CERN.

The topical orientation of the research was now left to the individual collaborations. Several concentrated on gamma-spectroscopic studies of level schemes of the many radioactivities that had come within reach. There were studies of decay schemes in the xenon and cadmium regions and a French collaboration headed by Roger Foucher concentrated on decays in the platinum-mercury isotopes and emphasized particularly the "soft" (or transitional) aspect of the nuclear shapes in this region. (These isotopes are situated between the strongly deformed (elongated) rare earths and spherical ^{208}Pb .) Others aimed at more specialized information such as nuclear masses from beta-decay Q-values and alpha energies, the systematics of alpha widths and the search for new cases of proton emission from nuclei following beta decay, the particular interest of the Gothenburg group headed by Göran Andersson. Finally, the Aarhus-Copenhagen collaboration had suggested that in view of the high energy release in the beta decay of far-unstable nuclei and the ensuing complexity of the spectra, it would be interesting to study the strength-function³ aspects of nuclear beta decay. This line of research soon merged with the studies of highly excited levels via proton emission. Some examples of results from the different lines of research will be given in Section 6.

4.6 RADIATION-DETECTED OPTICAL PUMPING (RADOP) COMES TO ISOLDE

The early scientific programme was, apart from the work on targets and ion sources, based on well-established techniques from nuclear physics and chemistry. A seminal event in the further development of ISOLDE's research programme was the presentation in 10 May 1968 of a new proposal [ott68a] entitled "Determination of spin and nuclear moments of mass-separated, short-lived isotopes by optical pumping". Its author, Ernst W. Otten from the Physikalisches Institut der Universität Heidelberg, was a new face in the ISOLDE Collaboration, but he had already the previous year completed two somewhat similar experiments at the Heidelberg cyclotron. These experiments, based on an ingenious mixture of atomic, nuclear and weak-interaction physics, could for certain elements give precise information on the spins, moments and charge radii of nuclear ground states and long lived isomers. Otten was now looking for a clean and abundant source of short-lived radioactive atoms, and his Heidelberg colleague Bogdan Povh, who on some occasions had substituted for Gentner on the ISOLDE Committee, had drawn his attention to the experimental possibilities at ISOLDE.

The proposed experiment was based on optical pumping, an important technique in atomic physics, which had been introduced by Alfred Kastler⁴ (Nobel prize in 1966). The principle of optical pumping is that the interaction of the atom with polarized light at the resonance energy leads to a transfer of angular momentum to the valence electron

³ This concept is taken from statistical nuclear physics, see e.g. Lynn [lyn68a]. A strength function is the product of the level density with the average reduced transition probability to (or from) the family of closely spaced levels in question.

⁴ In 1987 Ernst W. Otten became the first German recipient of the Gentner-Kastler Prize awarded jointly by the French and German physical societies [bul87a]. The role played by Wolfgang Gentner in the early development of ISOLDE has been mentioned in Sects. 2.2, 2.3 and 4.2.

and hence via the hyperfine-structure (hfs) coupling to a polarization of the nuclear spin. Otten's idea (see [ott89a]) was to detect nuclear polarization via the celebrated beta asymmetry originating from parity non-conservation. The light source for the experiment was a mercury lamp equipped with a $\lambda/2$ birefringent plate. The resonance condition could be met by placing the lamp inside a magnet, so that the magnetic Zeeman splitting shifted the frequency of the light. (One polarization component, say σ^+ corresponding to $\Delta m_F = +1$, goes up in frequency, the other goes down. This also provided a way to control systematic errors, simply by shifting from σ^+ to σ^- , which inverts the polarization.) The experimental apparatus is shown in Fig. 8.

Otten's proposal was quite brief and in addition to a summary of the general principles of the technique it gave simply a listing of the groups in the periodic table that would be suitable for such experiments: 1a, 1b, 2a, 2b, 5a, and the elements with half-filled electron shells. To this he added the for him characteristic remark, that it was not a coincidence that the list largely coincided with those elements that seemed favourable for ISOLDE, since atoms in S states are those most easily evaporated. The point in the remark is that for non-S states the polarization is destroyed by collisional relaxation.

Confronted with this remarkable collection of tricks, the ISOLDE Committee was preoccupied, maybe understandably so, with the feasibility of the experiment. What yields, count rates could one hope for, and which cases were the most promising? It ended up with requesting a more detailed analysis of the feasibility of the experiment. Oral tradition at CERN has it that P. Preiswerk, head of CERN's NP Division and not a Committee member, remarked after the meeting that he personally had found that the experiment was so beautiful that it ought to have been accepted, even if one had possessed the certain knowledge that it would fail. A second and more detailed proposal [ott68a] supplied the missing numbers and pointed to cadmium and mercury as the best early candidates for the experiment. The programme was accepted in the meeting on 17 December 1968.

The experiment turned out to be very difficult. After some initial attempts with the cadmium isotopes it was decided that mercury would offer better possibilities and the effort was concentrated on ^{187}Hg , which was produced at ISOLDE in good yields. When at long last an anisotropy signal was obtained [bon71a] it was very weak, which seemed a bad omen for experiments on lighter (and more rare) isotopes. Furthermore the main result, the nuclear charge radius, was no surprise at all as it continued the almost linear trend from the heavier isotopes. A completely different picture emerged from the next experiments [bon72a], dealing with the isotopes $^{181,183,185}\text{Hg}$ as may be seen from the more complete systematics given in Sect. 6.2. Not only did the lighter isotopes give a much better anisotropy signal but the isotope shifts indicated that the three nuclei were very much larger than ^{187}Hg ; they had roughly the same charge radius as ^{197}Hg . This was (correctly) seen as the discovery of a new and unexpected class of deformed nuclei. The whole picture, which emerged later from new data from atomic and nuclear spectroscopy and from a host of theoretical papers, was even more interesting and involved the phenomenon of shape co-existence, see the brief comments in Sect. 6.2 and for more detail the review by Otten [ott89a].

The RADOP experiment represented the opening guns for a research effort that was to become a major line at ISOLDE after the completion of the SC Improvement Programme. It also furnished an instructive example of a purely experimental discovery and maybe a lesson for those who in committee meetings clamour for theoretical

arguments. When Otten presented his proposal no theoretical predictions pointing to the light mercury isotopes existed although theoretical explanations became available in large numbers after the event. It will be clear from the above that the Heidelberg Group selected the experiment because it was one of the few, possibly the only one at all, that was feasible at the time. It should be remembered that at the time ISOLDE had not yet developed techniques for separating alkali and alkaline-earth metals.

4.7 WHY AT CERN?

It should be emphasized that ISOLDE was far from alone in the field nuclei far from stability. The period 1965-90 saw a large number of experiments by different techniques in many laboratories all over the world; Table 2, based on information from [ele81a,rav89a], lists those using on-line mass separation. It is characteristic of most of these that they were set up to serve the scientific interests of a small group of people. Many of the experiments produced excellent physics but it is probably fair to say that, in general, they did not aim for a very broad and continually renewed programme such as was the case for ISOLDE. They were usually also limited by their accelerators.

Table 2: On-line mass-separation^{a)} programmes at different machines

At high-energy proton accelerators	At heavy-ion accelerators
Orsay - CERN PS (MS)	Montreal (MS)
IRIS - Leningrad (LCS)	Orsay (MS)
ISOLDE 2 - CERN SC (LCS)	Tokyo (MS)
ISOCELE - Orsay (MC)	DOLIS-Daresburg (LCS)
ISOLDE 3 - CERN SC (MC)	GSI-Darmstadt (LCS)
TISOL - TRIUMF (MC)	Jyvaskulä (LCS)
At reactors or neutron generators	LISOL - Leuven (LCS)
GODIVA - Los Alamos (MS)	OASIS - Berkeley (LCS)
SOLAR - Richland (MS)	PINGIS - Stockholm (LCS)
OSTIS - Grenoble (MS)	Stockholm (LCS)
Copenhagen (LCS)	POLARIS - Princeton (LCS)
ARIEL - Grenoble (LCS)	RAMA - Berkely (LCS)
Kyoto (LCS)	SARA - Grenoble (LCS)
OSIRIS - Studsvik (LCS)	Tohoku (LCS)
SOLIS - Soreq (LCS)	Tokai (LCS)
TRISTAN I - Ames (LCS)	UNISOR - Oak Ridge (LCS)
TRISTAN II - Brookhaven (LCS)	Chalk River (MC)
Mainz (LCS)	BEMS 2 - Dubna (MC)
INEL - Idaho Falls (LCS)	EMSONHIB - Dubna (MC)
IAELE - Buenos Aires (MC)	
SIRIUS - Strassbourg (MC)	

- a) MS: mass spectrometer; LCS: low-current isotope separator.
MC: medium current isotope separator.

At this point we may ask why the world's largest installation for producing secondary beams of radioactive nuclei in their ground states came into existence at a laboratory dedicated to particle physics at the highest energies⁵ and why laboratories of a similar capacity were not created elsewhere? We shall have more to say about the second question in Sect. 7.3, but give here the simple answer that the result was due to a coincidence of several fortunate circumstances. CERN first of all possessed a high-energy proton machine almost ideally suited to the task and permitting a much broader programme than did reactors and smaller accelerators. It also had developed the organization and tradition that could assemble the necessary specialists in subjects such as target chemistry, ion sources and mass spectroscopy together with research teams from atomic, nuclear and solid-state physics. This second point is important for understanding why in principle free and independent external groups could maintain the sense of duty and loyalty to CERN and the Collaboration that ensured the stability of the programme over many years. And finally CERN provided an in-house group with engineers, technicians and some research posts for staff⁶, associates and fellows, a group that had as its first priority serving the external users.

⁵ The late Sven Gösta Nilsson, mentioned previously in Sect. 2.4, occasionally referred to ISOLDE as "an underground church in the heathen temple of high-energy physics", a joke referring mainly to the location of the main experimental area (Figs. 5 and 10).

⁶ The fact that the research staff frequently was on loan from the home laboratories and never permanently appointed probably also helped to avoid some of the friction that occasionally plagued the relations between CERN and its users [pes90c].

5. The SC Improvement Programme (SCIP)

In 1967, the year that the new underground hall for the SC was completed and ISOLDE went into operation, the SC Machine Division (MSC) headed by Giorgio Brianti was far advanced with the planning of major improvements in the performance [cer67a] of the synchro-cyclotron. The main aims were to obtain an increase of the internal beam by a factor 10, to about 10 μA , and the highest possible increase in the extraction efficiency leading to an external beam of at least 1 μA . These studies were continued under the new MSC Division leader, Ernst G. Michaelis, who took office in 1968.

In the following is given first a brief description of the physics and technology of SCIP. Many details can be found in a paper presented at the Cyclotron Conference in Vancouver in 1972 by Michaelis [mic72a], who also prepared a complete list [sci72a] of SCIP literature including internal reports and specifications for the use of the SCIP Advisory Panel (see Sect. 5.4). The rest of this Section is dedicated to the technical and political turmoil that this project gave rise to.

5.1 THE PLANS FOR THE SC UPGRADING

One essential ingredient in the SCIP programme was to replace the radiofrequency system based on a tuning fork with a rotary condenser, a solution that had, in fact been considered in the early planning of the SC [mer90c]. For the new project a small-scale prototype had been built, and further ideas for improvements concerned the central region and the ion source.

The rotary condenser as it looked when it had reached completion is shown in Fig. 9. The swing of the cyclotron radiofrequency (RF) during one cycle, from 30.0 MHz at injection to 16.8 MHz at extraction, was produced by the change in capacitance when the rotor blades, 3 rows of 16 teeth each, passed through the 4 rows of peculiarly shaped stator blades. In order to obtain the highest possible beam intensity, the frequency as a function of time had to follow closely the ideal curve, which could be approximated by tapering the stator blades. One advantage of this system of modulation is that the back-swing of the frequency is fast, so that most of the time is used for acceleration.

The acceleration cycle could be made even shorter by having for a high peak voltage in the RF power. This brings as an additional gain a larger distance between each turn of the spiral orbit, and hence the possibility of single-turn extraction so that beam losses inside the machine are reduced. This helps both the machine (radiation safety) and its users (more protons available). For these reasons the design voltage between stator and rotor was as high as 18 kV at a minimum gap distance of only 1 mm, reached at the end of the cycle. To have such a high voltage gradient would clearly be impossible with a static high voltage; it can be achieved with radiofrequency if the surfaces are so clean that a discharge does not have time to build up during one cycle. The one impurity that high-voltage experts fear the most is traces of hydrocarbons, so the final machining of the rotor and stator parts was to be done without lubrication oil and the parts assembled under very clean conditions. The construction furthermore had to be such that the rotor vacuum was isolated by a separately pumped duct from the vacuum of the rotor bearings, which were oil-lubricated, and from the acceleration chamber, which contained traces of oil-vapour. The stator blades were held by an outer support ring, which via a set of insulators held an inner ring, which via further sets of

insulators held the drive motor, the latter sitting in the very centre of the unit to provide mechanical equilibrium. The motor (under atmospheric pressure!) and various other parts had to be provided with cooling circuits, so the final construction was a complex intermingling of various seals, ducts, pipes, pressures, and voltages, the engineer's delight and the mechanic's nightmare. It is very much to the credit of the people¹ in charge that this device in the end worked well and served the SC for 15 years.

A contract for the construction of the radiofrequency system was signed [cer69a] with the company A.E.G.-Telefunken in February 1969 with delivery foreseen for August 1971. In the light of the events that followed, one may ask why the most complex, delicate and essential piece in the SC upgrading was placed in the hands of a private manufacturer? After all, experience, for example with the bubble chambers, had shown that CERN was capable of doing excellent technological development work. The explanation was that CERN was following a policy of farming advanced projects out to industry in the member countries in order to allow the firms to develop their know-how. The accelerator branch of the company in question had originally solicited an open-ended research contract in connection with the ISR, but the final decision by the CERN management was to let them handle the rotary condenser for the SC and at a fixed price [zil91a].

Other important improvements concerned the orbit dynamics of the cyclotron and in particular the central region, the domain of the Orbit Dynamics Group under N. Vogt Nilsen. The original SC used an open ion source dictated by the rather small energy gain per turn. The absence of axial focusing and the slow acceleration allowed space-charge forces to blow up the beam leading to a poor beam quality and hence low beam intensity and low extraction efficiency. The condition for using a hooded ion source giving a well-defined beam is then that the beam must go clear of the ion source on its first turn. The Group, with R. Galiana and R. Giannini working on the ion source and central region, found that with the improved RF system and a modified central region, which had been studied in models, this could be reached so that the cyclotron would have separated orbits. Another modification was that the ion source was inserted through a 20 cm diameter hole drilled from below to the centre of the magnet. The extraction channel was in the hands of B. Hedin, S. Lindbäck, and A. Susini, who hoped that the well-defined orbits would allow single-turn extraction with high efficiency, maybe 5-7%, an order of magnitude over the old value. The actual values achieved in the end were yet an order of magnitude higher: 50% in slow extraction and 70% in fast extraction. The occasion was also used to replace the field coils and the vacuum tank.

The SC upgrading was, however, beginning to be dangerously close in time to a new generation of proton accelerators, called the meson factories because their primary purpose was to deliver very intense secondary beams of muons and pions for experiments like those described in Chapter 3. The leading contenders in this field were: (i) LAMPF, the Los Alamos Meson Physics Facility, nominal 800 MeV protons at 1 mA and negative hydrogen at 100 μ A, completion foreseen 1972, (ii) the Canadian TRIUMF in Vancouver with 530 MeV H^- at 100 μ A (1973), and (iii) the Swiss Institute for Nuclear Research, SIN with 580 MeV at 100 μ A (1974), all target dates cited from the documents in [sci72a]. Any delay would therefore again raise the question whether the

¹ The engineering behind the rotary condenser was in the hands of H. Beger (RF group leader), S. Talas (mechanical design), A. Fiebig, R. Hecken and R. Hohbach (theory and models), and C. Hill (vacuum).

SC was needed. It will be remembered [mer90e] that this issue had already come up in Weisskopf's time; in his reply to a memorandum from the MSC Division Leader P. Lapostolle dated 29 January 1962 Weisskopf wrote: "I was also very interested in your remarks regarding future plans with the SC. It seems to me that these problems can perhaps be divided into two. One comprises attempts to improve the present machine by major alterations and the other (...) ideas of meson factories. It seems to me that the first kind of things would certainly fall within the terms of reference of your Division; about the second one I am not so clear myself." (Copy in [sci72a]). Weisskopf's remark is interesting as an indication that CERN already as early as January 1962 was ready to leave a large part of the subject of muon and pion physics to whoever wanted to take over.

In view of the new and more powerful accelerators appearing on the horizon and, no doubt, further stimulated by the technical and commercial difficulties, to which we shall come back in a moment, the debate about the future of the SC had resumed in CERN's inner circles towards the end of 1970. This can be seen from a draft document [mic70a] from 9 November 1970 that Michaelis discussed with Ericson and Kofoed-Hansen and that apparently never was given any wider circulation.

5.2 SCIP IS DELAYED: POLITICAL, COMMERCIAL AND TECHNICAL DIFFICULTIES

By 1970 technical difficulties began to appear, heralded by an announcement by E.G. Michaelis at the Physics III Committee meeting on 2 November 1970 of a 6-8 months delay in the delivery of the new radiofrequency parts. By 30 June 1971 CERN had received a revised estimate of the time plan from the manufacturer, and Michaelis informed the Director General that the delay had grown to a total of 47 weeks, so that the new delivery date for the equipment would be 10 July 1972 [mic71a]. Some of this time could be recuperated by starting the dismantling of the cyclotron before the parts had been delivered and tested, but at the risk of a long, unplanned and very uncomfortable shut-down if further delays occurred or if the high-voltage tests were unsatisfactory. According to Michaelis this solution would also make CERN's threats of canceling the contract after its "ultimate delivery date" look less credible. He himself favoured avoiding any major dismantling operation until the test results were known, foreseeably in April 1972.

A small meeting [not71a] to discuss these options was held on 8 July 1971 at the initiative of J. Steinberger, Director of Physics I. It is characteristic, however, that the meeting was not with engineers and administrators to discuss the technical, legal and commercial aspects of the problem but with the users² of the SC. Steinberger opened the meeting by asking "if, under these conditions, the users maintained their interest." They did and asked that all should be done to reduce the length of the shutdown in preference to advancing the start-up date. The meeting agreed that the physics programme should be scheduled to run to 1 April 1972 with a further programme prepared "to continue operation into 1972 if a failure of the tests necessitated an abandonment of the Improvement Programme". The future of the machine was clearly

² The three users present were G. Backenstoss, J. Deutsch and P.G. Hansen, and two more, V. Soergel and E. Zavattini, had been invited but were unable to come. Also present were the Director General W. Jentschke, the NP and MSC Division Leaders H. Schopper and E.G. Michaelis, and O. Kofoed-Hansen, who had replaced Ekspong as Chairman of the Physics III Committee.

now very much in question in the minds of those present; the wording of the conclusion implies that CERN was ready to let the quality of the workmanship of one commercial supplier decide the future of one of its programmes. This might appear to be in contradiction to CERN's usual record of superb technical control, but it should be remembered that the SC by then was used almost exclusively by physicists from CERN's member countries; this was one machine that the aristocracy of CERN, to borrow an expression coined by Pestre [pes90b], could well be without. It would also not have been the first time at CERN when technical arguments were used to obscure the real issues.

One who had no difficulty in seeing what the real issues were was Sir Denys Wilkinson from the University of Oxford, the chairman designate of the Physics III Committee. Kofoed-Hansen's term as chairman was coming to its end and in a letter of May 25 Jentschke had asked Wilkinson to take over. Sir Denys replied [phy91a] from his sabbatical retreat in Seattle (Washington): "I am very pleased to accept and shall look forward to doing my best for the SC and nuclear structure programme of CERN (and to defend them against predators)."

In the spring of 1972 a small conference "on the future of medium-energy physics" [lec72a] was held at CERN in order to prepare the ground for the coming battle. Meanwhile, during the autumn and winter 1971-2 new delays in the manufacture of the rotary condensers had occurred and the new target date of April 1st was long past when Michaelis reported to the Physics III Committee [phy91a] in its open meeting on 14 June 1972. He said among other things that "we cannot hope for decisive information before (...) the end of September. (...) Past experience suggests that the earliest date is unlikely to be met (...) In conclusion I regret that I am still unable to give a definite date for the SCIP shut-down." Wilkinson, commented that the continuing delays and uncertainties were bound to raise the question of the wisdom of continuing the SC Improvement Programme and it was necessary for the members of the Physics III community to express their opinions on this point. In Wilkinson's view it was not right to arrive at conclusions immediately as many of the people concerned were not present at the meeting, but he proposed to send out a letter asking for written statements.

In the discussion that followed Hans Hofer (Bern) and Jean Pierre Stroot (Bruxelles) both suggested that CERN should take over the responsibility and finish the job at CERN, the solution that, as we shall see, emerged in the end. Ernst Otten, the Chairman of the ISOLDE Committee, pointed to the plight of ISOLDE that was already rebuilding its facility in preparation for the SCIP. Others felt that more pressure should be brought to bear on the manufacturer of the rotating condensers, to which the Director of the Administration Department, G.H. Hampton, said he had found the company utterly impervious to all threats, legal or commercial.

The letter [wil72a] from the Physics III Chairman, dated 17 July 1972, listed a number of technical options, competing accelerators etc. and asked for consideration of the action to take. It was now up to the SC users to provide input for the next Physics III Committee Meeting, in which recommendations would be made.

5.3 CONFLICTING VIEWS: SHOULD THE SC BE SHUT DOWN OR UPGRADED?

Whilst the debate in the Physics III Committee with a few honourable exceptions had followed the line that CERN had taken, namely that the issue was commercial and

technical problems in connection with the manufacture of the two rotary condensers, other committees that met during the same week had gone straight to the political aspects

The Scientific Policy Committee had met on 13 June 1972 [spc72a] and "the eventual³ closure of the SC was examined again". It became clear during the discussion that, if groups now working on the SC wanted to work at SIN around 1976, they would be expected to contribute to the operating costs and such a contribution would be of the same order of magnitude as the cost of SC operation. According to a statement made by B. Hahn, the SIN authorities would welcome if any move of this kind were coordinated by CERN. The Director General of Laboratory II (J.B. Adams, head of the SPS machine construction) remarked to this that it was "difficult not to consider priorities". In the following discussion E. Amaldi, L. Leprince-Ringuet, V.F. Weisskopf and D.H. Wilkinson emphasized that it would be very bad for CERN to exclude low- or medium-energy physics from its activities, in particular with an eye to the way CERN was used by the smaller member countries. They argued that it was in CERN's interest not to separate high-energy physics from low- or medium-energy physics and the question was not which priority should be allotted to low-energy physics but that of fixing priorities between the various parts of the high-energy physics programme.

At the Council Meeting on 15-16 June 1972 the British Delegate, Sir Brian Flowers, questioned the conclusion of the SPC; he "remained unconvinced that the work done on nuclear structure at the intermediate energies was of the same quality and significance as high-energy particle physics. (...) The question was important not only in its own right but also in view of the need for economy in order to finance an extremely important⁴ venture" (i.e. the SPS). [cou72a].

About two weeks later Denys Wilkinson was again at CERN and one senses that he was influenced by the views prevailing in the CERN Management and also by the outgoing PH III Chairman, Otto Kofoed-Hansen, always a man in favour of sweeping and radical solutions. Following the visit to CERN, Wilkinson travelled to the construction site of the Swiss meson factory SIN at Villigen near Brugg. The following is an extract of the letter [wil72b] that he wrote on 14 July 1972 to the Director of SIN, Jean-Pierre Blaser:

May I review very briefly the extremely tentative programme that I had discussed with Jentschke just before I visited you and about which I told you:

- (i) *Physics III meeting on 3 October at which, hopefully, Physics III will endorse the idea of an ultimate transfer to SIN and closure of the CERN SC if the right conditions can be obtained.*
- (ii) *Shortly following (i) informal but explicit talks between you, Jentschke, myself and interested officers on possible ways of implementing the transfer from CERN to SIN.*

³ As is most often the case when the words *eventual*, *eventually* appear in a CERN context, it is here impossible to see whether they are used in their English meaning of "ultimately resulting" or in the more loose, hypothetical meaning that similar words have in e.g. Danish, Dutch, French, German, Swedish - and that *eventual* had in English until the end of the 18th century.

⁴ It is probably evident that Adams's and Flowers's weighing of the SPS against SCIP left out the fact that the former was hundred times more expensive than the latter. This is the gambit that in scientific politics is called "setting priorities".

The most striking feature of the proposed procedure is that it decides on the closure of the SC and the transfer (of certain programmes?) to SIN *before* the economic and other consequences of this are studied. It appears that Kofoed-Hansen, who was at CERN as a personal advisor to Jentschke, was given the task of putting some substance into these ideas. On 25 July, just before going into hospital, he provided the Directorate with a hand-written draft [kof72a] for a memorandum "On the phasing out of the SC", in which he argued that Europe would be better served by having the SC available all the time up to the start of SIN. Savings in money and manpower could be used for what he calls the "internationalization of SIN" and for an early realization of SIN's plans for an improvement programme to take the machine to higher beam intensities. He pointed out that it would be important to help ISOLDE to optimal conditions for the transfer.

Kofoed-Hansen's proposals were criticized by Michaelis [mic72b, sci72b] for being unrealistic with respect to time and money. Still, in retrospect one probably has to admit that Kofoed-Hansen had one valid point, namely that European intermediate-energy physics, or maybe simply physics, could have been served better in an ideal world with a closer interaction between CERN and SIN, the latter in many respects the best of the world's meson factories. As we shall see, there was in the non-ideal world, that we live in, two opposing currents that were to prevail. The outside users were reluctant to give up their share in CERN, which they considered an important part of the European physics⁵ scene, and CERN's owners and its permanent physics staff saw little use in spending money at SIN just in order to serve outside users better.

5.4 THE USERS ARE HEARD: PHYSICS III AND THE SCIP ADVISORY PANEL

Not surprisingly there was no immediate response to Sir Denys's request (Section 5.2) for viewpoints on the future of the SC and its improvement programme. The letter of 17 July and with an implicit September deadline was sent out at a time that to university physicists means end of term, travel to conferences, private holidays etc. The present writer, who at the time was one of the few outside users actually present at CERN, has a vivid recollection of having spent a good fraction of August and September making telephone calls to colleagues in the order that they returned home and suggesting that they should begin opening their mail. In the end a total of 34 letters [let72a, sci72b] bearing the names of 57 physicists were received from the Physics III Community. They showed an overwhelming majority in favour of continuing the SCIP programme. The recurring argument was that an activity at intermediate energies was important if CERN were to be maintained as a meeting ground for high-energy and nuclear structure physicists, the original goal of Weisskopf's initiative of 1963 (Section 2.1). Many, among them Gerhard Backenstoss, made clear that they were positive to a close cooperation with SIN provided that there remained active nuclear-structure research at CERN. In a different vein, Bergström, who at the time was collaborating with Backenstoss on SC and PS experiments on exotic atoms, stated that his laboratory in Stockholm was not using CERN just because of the existence there of "a certain beam with certain properties" but rather because of the intellectual and technical stimuli that it provided to visitors and visiting groups. He asked whether CERN would be prepared

⁵ The same latent conflict is symbolized by the recent re-naming of Experimental Physics Division (EP) to Particle Physics Experiments (PPE).

to take the political consequences of a decision that made it "a rather exclusive laboratory for privileged particle physicists?"

A long debate took place at the Physics III Meeting on 3 October 1972. Its meanderings through sub- and side-issues may be followed from a lucid summary prepared by the Committee's secretary, A.J. ("Freddy") Herz [deb72a]. At the proposal of Wilkinson it was finally decided to set up a panel⁶ [mem72a] to work on a week-by-week basis with Michaelis to advise the Ph III Chairman on the date to be recommended for the start of the SC shut-down and to advise and inform both the Physics III Committee and the MSC Division on all matters of interest to intending SC users. A monthly report on the progress was to be sent out jointly by Michaelis and the Chairman of the Panel. It was decided that, initially, the Panel should meet every two weeks.

Already at the following meeting of the Scientific Policy Committee on 7 November 1972 [spc72b] the CERN Management had now chosen to maintain the SC. Jentschke announced that the shutdown, expected to last a year, would take place in March. The intention was to continue to operate the SC until it ceased to be a really modern tool for nuclear structure research. If the rotating condenser failed, the plan would have to be reconsidered. Wilkinson added to this that this decision was in line with views expressed by a large majority of users. Even in the case of a possible technical failure, he still saw strong arguments for a partial improvement programme including the acceleration of helium ions.

Barring technical mishap it had thus been decided to maintain the SC. Although the proponents of SCIP saw this as a victory, the reality was rather that the views had converged. Let us try already at this point to summarize the arguments: (i) It was in CERN's interest to hold on to its role as Europe's most prominent physics laboratory by having a broad scientific programme, and (ii) this also broadened the science-political support base in the member countries, of which especially the smaller ones (iii) were interested in institutionalized access to a big-science laboratory with its intellectual climate and technical know-how. Finally (iv) some of those initially opposed to SCIP had come to fear that a transfer of activities to SIN would imply a direct or indirect transfer of funds to SIN in an amount that would exceed the cost of continued SC operation.

The SCIP Advisory Panel held its first meetings on 3 and 10 October and when it met a third time on October 21 three members (Boschitz, Hofer and Nielsen) had visited the manufacturer of the rotary condensers in Berlin. They had come back with a clear impression that the work going on there was technically sound but also that the manufacturer had lost his motivation and feared to be trapped in the very exacting specifications for these devices. (The company's position is stated very clearly in a letter to the CERN Administration [aeg72a].) The first joint report from Michaelis and the Panel to the Physics III Committee [han72b] ended on a significant note: "Discussions are in progress both within CERN and with AEG with the aim of finding contractual solutions which will enable the technical work to go ahead undisturbed." The following meetings began to reveal that in addition to the resistance to SCIP inside CERN and the commercial dispute with Berlin there was a third problem in that the always correct and

⁶ The final composition of the Panel was: G. Backenstoss (Karlsruhe), E.T. Boschitz (Karlsruhe), C. Cernigoi (Trieste), P.G. Hansen (Aarhus and CERN, Chairman), H. Hofer (Bern, later Zürich), O.B. Nielsen (Copenhagen), V. Soergel (Heidelberg), N.W. Tanner (Oxford) and E. Zavattini (CERN).

cautious Michaelis was quite happy to see the most delicate element in the SCIP placed outside his responsibility.

By the time of the ninth meeting of the Panel on 24 January 1973 a clear picture of the situation had emerged, and in a discussion, in which Wilkinson participated, it was decided to take action. On the following day the Panel recommended to Physics III "(i) that CERN take over full responsibility for the first Rotco...Moreover we do not think it is necessary to insist on a completed full-power test before this happens. We further recommend (ii) that the shut-down should start on April 19th", a recommendation that Wilkinson endorsed and took to the Director General. On 4 April 1973 the NPRC [npr60a] decided to fix the shut-down for 31 May. The additional delay had been put in to allow two physics groups to complete their experiments, one of them being the experiment on the fine structure of muonic helium (Sect. 3.3). The shut-down finally took place on 7 June 1973 and was followed five days later by a flooding of some machine areas during a thunderstorm; some physicists took this as an indication that divine intervention had been prepared in case the MSC Division had continued to procrastinate.

The SCIP Advisory Panel continued to monitor the Improvement Programme during the following year. On 1 October 1974 protons were for the first time accelerated out to full radius with an intensity of $0.6 \mu\text{A}$ at a repetition rate of one pulse in 16 meaning that one basic goal had been reached. The first experiments began in December and scheduled operation from January 1975. On the 24th and last meeting of the SCIP Advisory Panel on 24 February 1975 it was decided to recommend the dissolution of the Panel as the main design aims of SCIP now had been attained [sci72c]. The experience over the coming years showed that the most stable operation was achieved with a reduced repetition rate (1:2) on the rotary condenser and corresponding to an internal beam of about $4 \mu\text{A}$. With 70% extraction efficiency in the fast mode this meant about $3 \mu\text{A}$ proton beam on ISOLDE's target

The success of the SCIP was due to the competence of the CERN engineers and technicians working on the floor. Although Denys Wilkinson and the members of his SCIP Advisory Panel had spent two years leaning over drawings of rotary seals, cooling ducts and other technicalities their contribution had been essentially a politico-technical one, i.e. that of de-fusing the technical arguments that were being used to stop the SCIP. The situation had many features common with the one described by Pestre [pes90c] for the time when the "truck teams" disappeared, namely that those outside physicists who were running independent programmes at CERN and not just acting as a source of auxiliary labour were forced to unite against what they felt was passive - and occasionally active - resistance by the CERN establishment.

5.5 POST-SCIP DEVELOPMENTS: THE ACCELERATION OF ^3He AND HEAVY IONS

In 1973 it was pointed out by Hohbach [hoh73a] that with relatively small modifications to the radiofrequency system it should be possible to accelerate deuterons and doubly-charged ^4He and ^3He . During 1973-75 the studies continued and were extended [gal74a] to the possibilities for accelerating multiply-charged, partially-stripped ions such as $^{14}\text{N}^{5+}$. The possible acceleration of trans-protonic projectiles was discussed repeatedly in the SCIP Advisory Panel [sci72c] and it was brought to the attention of the Physics I Department [han74a]. ISOLDE was mainly interested in ^3He with a total energy of 900 MeV as this projectile was expected to give larger cross-sections for

deep-spallation reactions than a 600 MeV proton beam, both because of the increased energy and also because a composite projectile was expected to deposit a larger part of its energy in the initial collision. Heavy ions, on the other hand, were not thought to be a competitive production tool for ISOLDE. For this reason a limited modification involving only the helium beam was discussed early in 1975 between the RF Group (H. Beger and A. Fiebig) and ISOLDE (P.G. Hansen) [beg75a]. Later in the year Beger submitted an outline [beg75b] of such a project to the Physics III Committee.

At the SCC Meeting on 1-2 December 1975 a sub-committee with P.G. Hansen, R. Klapisch and O.B. Nielsen as members was set up to state the physics case for ions and to discuss the technical aspects with the MSC staff. In their presentation on 10 March 1976, summarized in [hio78a], they emphasized that the job was cheap, essentially 150 kSfr for a new intermediate line for the radiofrequency, and that the gains could be great but that a determination of the relevant cross-sections required on-line mass separation, possible only at ISOLDE. The modification was approved and the CERN Annual Report mentions that the extension to the connection Dee-Rotco was ordered the same year, 1976.

From other groups there also emerged an interest in the helium beam, primarily for experiments attempting to observe the production of K- and pi-mesons and especially in so-called coherent reactions, in which a large part of the necessary energy is supplied by the nuclear structure, in the simplest form by the tails of the Fermi-momentum distribution in the nucleus [all76a]. Similar processes had already been seen with protons (Sect. 3.4). During the Workshop on Intermediate Energy Physics [wor77a] held at CERN in 1977, the interest in heavier ions was again voiced by several groups doing counter experiments and the acceleration of $^{12}\text{C}^{4+}$ and other ions with $Q/A=1/3$ was one of the recommendations emerging from the meeting [han77a]. This modification was also approved and the realization including changes in the ion source and in the extension line began in 1978. The workshop had also considered the possibility of studies with post-accelerated radioactive ions but concluded, as we know today wrongly, that although there were interesting applications, these were of no major interest in nuclear physics.

The ion programme was from the beginning seen as placed in a time window before more powerful beams with up to 100 MeV/nucleon would become available at the French national accelerator laboratory GANIL (for Grand Accelerateur National des Ions Lourds) in Caen and in an energy window above the 10 (later 20) MeV/nucleon available at the German heavy-ion centre GSI (in Darmstadt). The SC beams attracted a large number of good experimental groups, which made substantial contributions to the understanding of heavy-ion reactions at intermediate energies. As foreseen much of the effort was concentrated on pion production at energies well below the free NN threshold, a subject that had earlier been studied in proton-ion collisions, see Sect. 3.3. Among the laboratories that took part in the programme can be mentioned Cagliari, Copenhagen, Darmstadt, Frankfurt, Heidelberg, Lund, Saclay, Stony Brook, Strasbourg and Turin. The reviews [met88a, gel87a, bra87a] give a good impression of themes and status of the heavy-ion field and references to much of the SC work. Among the many papers on pion production we cite as examples Johansson et al. [joh82a] for work on charged pions, and Grosse et al. [gro86a] for work on neutral pions and direct photons. It gives a striking impression of the importance of collective effects in the pion-production mechanism to note that π^0 production has been observed with heavy ions with as low a laboratory energy as 20 MeV/nucleon, where the threshold for NN

collisions, also on a target at rest, would be around 290 MeV kinetic energy. The ion experiments came to an end around the mid-eighties, when more powerful machines took over.

The helium beam continued to be used by ISOLDE until 1990 and gave in some cases gains approaching an order of magnitude in production rate, see [rav89a], but it is probably fair to say that the beam did not become the magic bullet that its proponents had hoped for.

6. The evolution of the scientific programme at ISOLDE

In this Section we give examples of how the ISOLDE Facility for slow radioactive beams has permitted studies of a wide variety of topics in nuclear, atomic and solid-state physics. A much more complete picture may be obtained through the proceedings of the series of specialized conferences [ley70a, car76a, hel81a] that followed the original Lysekil Meeting of 1966 (see Section 4.2) and through review papers [han69a, kla69a, kla74a, han72a, han73a, jac79a, ham82a, neu87a, ays89a, det89a] prepared for other conferences and books. Volume 8 of Bromley's *Treatise on Heavy Ion Science* [bro89a] has been dedicated to the subject of nuclei far from stability and gives an excellent up-to-date coverage of both the physics and the experimental techniques.

The first years of ISOLDE were a learning period during which the users got to know the machine and found out what new problems it could be used for and which were the experimental techniques best suited for the purpose. On the way there were many practical problems that had to be solved, e.g. realizing that the new isotopes of argon that had been discovered in one run in fact were well-known but doubly-charged isotopes (and hence appearing at mass $A/2$) of the next heavier noble gas, krypton. It was activities like this that earned the ISOLDE physicists the (in some cases undeserved) reputation of being a collection of isotope and gamma-ray hunters.

Not unlike biological evolution, the new conditions brought about new species that for a while seemed to have an edge in the struggle for survival. A pertinent example, already mentioned in Section 4.5, was the highly mobile collection systems designed for operating upstream in ISOLDE's beams, systems that in the long run were too inflexible to allow more sophisticated experiments. (We shall see below that after SCIP it was possible to serve several users in parallel in a better way.) Conversely, there were also old established techniques that, dinosaur-like, turned out to have no future under the new conditions. An example of this was provided by the Aarhus-Copenhagen group, which during the preparations for ISOLDE had constructed a large magnetic six-gap electron spectrometer, a so called "orange" with very large transmission, one of the most powerful tools of nuclear spectroscopy of the period. It was never used and that for several reasons. First of all physics had changed and precise and detailed spectroscopy was less interesting than it had been a decade earlier. To this came a continual shortage of machine time, which made an instrument based on point-by-point scanning inefficient¹.

Starting with the machine's targets, ion sources, lay-out and beam lines and continuing with the various experiments, we shall now try to give some indication of the evolution during the period 1967-81.

6.1 THE ISOTOPE SEPARATOR AND ITS BEAMS

From 1971 ISOLDE began to give priority to the preparation of the post-SCIP experiments and about half of the available machine time was devoted to tests of new systems [han79b]. The importance of this research effort was generally acknowledged and the delay in the start of SCIP actually turned out to be a blessing in disguise since

¹ The appearance of more efficient but less selective and far less discriminating spectrometers based on silicon solid-state detectors could also be cited, but was probably less important.

it allowed the tests to be done more safely (with weaker beams). The in-house group, now called the CERN-ISOLDE Group (group leaders P.G. Hansen, 1970-78, and B. Jonson, 1978-83) had been joined by the talented experimentalist and engineer, Helge Ravn, who set out to make a wider range elements available and also to make more systematic studies of the delay between production of a radioactive nucleus and its arrival in the experiment. The first results concerned alkali elements, which because of their low first ionization potential can be ionized on a hot metal surface, a technique that had first been exploited for on-line mass separation by Bernas and his collaborators [ama67a] and since had served in a series of experiments at the PS [kla69a]. The experiments at ISOLDE incorporated a number of new developments such as the use of molten metals or very hot powders of refractory metals as target material and a new design of the ion source. The techniques were over the following years extended to give beams of the earth alkalis, of the rare earths and, later, also of the halogens produced selectively as negative ions by surface ionization. The use of various designs of plasma ion sources together with the new molten-metal or powder targets also played an important role in improving the production of the ISOLDE classics, the noble gases and mercury, cadmium and zinc. We shall here not go into details with this fascinating research, which involves a mixture of experimental physics, accelerator technology, metallurgy, engineering and chemistry, but refer simply to some review papers [kla74a, rav79a, bjo86a, rav89a]. The progress that the combination of this work with SCIP brought about is illustrated in Fig. 7, where roughly a factor 60 of the improvement in production rate arises from the increased proton beam intensity at the re-built cyclotron.

A second goal in the preparations for SCIP was to introduce more advanced engineering, a task handled by the engineers Erich Kugler and Stig Sundell in collaboration with Ravn. The new high-temperature systems had no oven and the target container, usually made of tantalum, was heated directly by Ohmic losses in the construction material - and soon also to a considerable degree by the proton beam... The targets were made from a modular design (the module is visible on Figs. 10-11) to allow many variations in the configuration to suit individual experiments. Thus, the same (mass-produced) parts could be fitted with different ion sources (surface-ionization or plasma) and with various accessories such as hot or cold connections between target and ion source (in order to transmit or not transmit less volatile elements) and devices for admitting stable isotopes as mass markers into the beam. Another aim of the standardization was to make the systems cheap - the Group knew that the high radiation levels after SCIP would not permit any repairs, so that any irradiated unit would be discarded at the first break-down. Still, the cost of expendable target units was to become a major post on ISOLDE's budget. The target area was designed so that the modules could be brought in and out by remote control, in the first version via rail to a shielded cave (Fig. 10) and a few years later by an industrial robot (Fig. 11). The supplies (voltages, cooling water, carrier gas) to the target module were all provided from the (fixed) separator front-end and acceleration stage via automatic couplings designed to serve the needs of all systems.

A third but equally essential problem in the preparations for SCIP was the layout of the whole area including the interfacing of the separator with the experiments. A

number of discussions took place involving the house group¹ at CERN, the external users and the engineers. There were two major issues of which one was space. Any extension of the underground hall would have been very costly and it was decided instead to add a large hall at ground level (top left on Fig. 10), to be used mainly for electronics and data taking and also to place some experiments in this hall and in the floor below it. Still, it was clear that it was essential to make the best possible use of the lowest level (the hall next to the separator) and also to add more shielding. It was Erich Kugler who found the solution that met these requirements. The essential elements, which can all be seen by comparing Fig. 10 with Fig. 5, were (i) to move ISOLDE's target back by about five meters to the beam foreseen for particle physics experiments and (ii) to suppress the 3 m drift tube, which the Group had come to believe was not essential. The machine, effectively eight meters shorter, now (iii) ended before the existing shield and (iv) an additional shielding wall with mid-plane iron could be constructed around the analyzing magnet and part of the dispersion chamber.

A more modern beam-handling system was designed by Kugler, who replaced the "einzel-lenses" of the Copenhagen design (Sects. 4.2, 4.3) by electrostatic quadrupoles. Finally the experience with the first ISOLDE had shown that it was important to preserve some possibility of parallel operation in order to economize beam time. Kugler and Göran Andersson (Gothenburg), who was spending a year at CERN as an associate had the idea [kug73a] that the almost parallel geometry of the fan of beams in the dispersion chamber would allow the interception of individual masses by four movable electrostatic deflectors, which would swing the beams into four external beam lines. The resulting switchyard, shown in Fig. 12, gave rich possibilities for several experiments to operate in parallel; during the first years of ISOLDE-2 operation there were on the average three experiments taking data when beam was on.

6.2 NUCLEAR MASSES, SPINS, MOMENTS AND RADII

One fundamental experimental question posed at ISOLDE was whether new structural phenomena would appear in the regions of the N-Z plane now available to experiment. The spectroscopists had believed that the proper, maybe even the only, way to answer this question was through the detailed spectroscopy of excited nuclear levels, but experience had taught them that this path was laborious and very demanding in machine time (Sect. 4.5). Otten's experiment, dealing with the ground state properties of four mercury nuclei (Sect. 4.6), had on the other hand given completely convincing evidence for a new and unexpected phenomenon, which it took many subsequent measurements, nuclear as well as atomic, to clear up. It is therefore not surprising that a number of groups from 1975 and onwards mounted dedicated experiments to scan the properties of nuclear ground states and long-lived isomers.

The fine structure of the nuclear mass surface (as a function of N and Z) provides detailed insight into nuclear structure as demonstrated e.g. by the overviews of mass data given by Zeldes in [ley70a] and [hel81a]. Early work at ISOLDE had obtained masses via the determination of the energy release in radioactive decays (see papers in [hel81a]) but a more direct and comprehensive approach was developed by a group

¹ The CERN-ISOLDE research staff was in 1971: P.G. Hansen, B. Jonson (fellow), H.L. Ravn (coordinator for the machine), W. Wätzig (fellow) and L. Westgaard (coordinator for physics).

[eph79a] from the René Bernas Laboratory¹. The experiments, carried out first at the PS and later at ISOLDE, used a (velocity-focusing) Mattauch-Herzog spectrometer and were based on the principle that if the magnetic field is kept fixed, then two ions of masses M_A and M_B will follow the same trajectory in the instrument provided that all electric potentials V obey the relation $M_A/M_B = V_B/V_A$. Thus, the measurement of the mass is converted to a precise measurement of the ratio of the two electrostatic potentials that will bring the two ions into focus. The experiments gave evidence for new regions of deformed nuclei near $N, Z=20, 11$ (although 20 is a magic number normally associated with spherical shape), 60, 40 (known from spectroscopic studies, see Sect. 6.3) and 90, 55 (slightly below the familiar deformed rare-earth region). Unfortunately this research line came to an end with the studies of alkalis, mainly for technical reasons, and it is only within the last few years that the technique that seems destined to replace the classical mass spectrometer, namely the use of ion traps, has begun to emerge [sto90a].

Another experiment, carried out by Ekström and his collaborators [eks81a], studied spins and moments in long chains of isotopes by means of the atomic-beam magnetic-resonance method (ABMR). Their spin spectrometer was designed so that for a given element and given settings only one spin value was transmitted, so that the spin value of a given radioisotope could be determined in 5-6 measurements, which makes the technique extremely fast. By scanning the transmission as a function of the radiofrequency setting at somewhat higher magnetic fields it also allowed the measurement of magnetic and electric moments. The detailed comparison [eks81a] of spin and moment data with predictions from theoretical models demonstrates very convincingly the insight that this systematic information gives.

The original optical-pumping experiment on mercury (Sect. 4.6) was based on the detection of nuclear polarization and hence could not provide any information on the radii of the (spinless) even-even isotopes. In a second experiment Otten and his group (see [ott89a]) used tunable lasers together with frequency doubling in order to get into the UV range of wavelengths necessary for mercury and measured the resonantly scattered light from a cell containing the radioactive vapour. The results together with those of later work are shown in Fig. 13 demonstrating that the $^{182,184}\text{Hg}$ do not have the anomalously large radii that characterize their (strongly deformed) odd-mass neighbours and also that the isotope ^{185}Hg has a second low-lying isomeric state of presumably spherical shape. The favoured theoretical explanation, see [ott89a], is the coexistence at low energy in these nuclei of two families of states with very different wave functions, one corresponding to almost spherical shape and one corresponding to a strong quadrupole deformation. Nuclear spectroscopic studies of ^{184}Hg have identified a low-lying 0^+ state, which has a well-developed rotational band and which must be the deformed partner to the ground state, see the review by Hamilton [ham89a].

It is only for the heavier elements that it is possible to study hyperfine structure via the optical spectroscopy of atoms in a gas cell such as in the experiments described in the previous paragraph and in Sect. 4.6. Already for somewhat lighter elements the thermal Doppler broadening of the lines exceeds the hyperfine splittings and isotope shifts, and it becomes mandatory to use Doppler-free techniques. One solution used by a collaboration involving the René Bernas and Aimé Cotton laboratories (both Orsay)

¹ Bernas, who had been one of the founders of ISOLDE (see Sect. 4.2), had died in July 1971.

was to stop the ions from ISOLDE and to re-evaporate them as a collimated atomic beam, which interacted with a laser beam crossing at a right angle. The polarization transfer was now detected with a magnetic six-pole (Stern-Gerlach analyzer) followed by an ionizer and a small mass spectrometer. This technique has given a wealth of interesting results for rubidium, francium and caesium, see e.g. [coc87a, thi81b] and also the reviews [jac79a, ott89a]. Owing to the losses connected with re-evaporation and re-ionization of the atoms, the method could not readily be adapted to other elements than the alkalis.

The general solution to the Doppler broadening problem turned out to be collinear laser spectroscopy, a technique invented by Kaufman [kau76a] and independently by Wing et al. [win76a] and developed for studies of radioactive atoms at ISOLDE by the Otten Group [mue83a]. The basic technique is to deflect the ion beam from the separator so that it travels parallel to and inside the light beam from the laser and, in the simplest version shown in Fig. 14, to measure the resonantly scattered light in the observation region. Although the fast beam obviously implies a large Doppler shift, there is the surprising and extremely important corollary that the Doppler spread becomes very small. This is because the spread in the kinetic energy E is not changed by acceleration through a constant potential. Denoting the atomic mass and velocity by m and v we can relate the energy and velocity dispersion

$$\delta E = \delta(mv^2/2) = mv\delta v = \text{constant} \quad ,$$

showing that a large velocity is associated with a small Doppler spread. The reduction factor turns out to be of the order of 1000 for an ion-source temperature of 2000^o K and an acceleration voltage of 60 kV. The collinear scheme has been used for a large number of experiments on different isotopes. It allows many modifications which are discussed in Otten's review [ott89a]; suffice it here to mention the possibility of neutralizing the ions by in-flight collisions with alkali vapour, a variant that is of prime importance because most ions in their ground states do not have resonance lines in the region of visible light.

The optical techniques have been used to measure radii, spins and moments for about 400 ground states and isomer of about 20 elements, and a similar number of spins and moments have been measured by the ABMR method. These tools from atomic physics represent without doubt the most important application of on-line mass separation until now.

6.3 NUCLEAR SPECTROSCOPY

The traditional nuclear physics allowed the exploration of nuclear structure close to the nuclear species that occur in nature, that is along the line of beta stability, see Fig. 2. As mentioned in Section 2.4, an important motivation for undertaking spectroscopic studies away from the stability line was the hope of finding new regions in the N, Z plane with "simple" properties, such as new magic regions and new regions of strongly deformed nuclei. (The word "deformed" here refers to so-called quadrupole deformations, that is cigar or pancake shapes). To the author's knowledge the first suggestion of such a possibility was made by B.R. Mottelson at a small conference in Copenhagen in the spring of 1961; he suggested among other things that the at that time unexplored region with both N and Z smaller than 82 and bigger than 50 should show well-developed rotational bands. This was soon after confirmed in an experiment by Sheline

et al. [she61a], who found a drop in the energies of the first-excited 2^+ states interpreted as indicating the onset of nuclear rotations. As we have seen in the previous Section the atomic methods for probing nuclear ground states were soon to provide a more general - and model-independent - tool for probing nuclear sizes and shapes.

The proton-rich nuclei have gradually become accessible to experiment all the way out to the limit of proton instability, the proton "drip line", which for the lighter nuclei lies close to the $Z=N$ line, fundamental for its relation to the quantum number isospin, in nuclear physics denoted T with projection¹ $T_z=(N-Z)/2$. The nuclei with negative isospin projections are of special interest in nuclear physics as they are (with a few exceptions) the only ones in which beta decay can manifest itself with maximum matrix elements, the so-called "superallowed transitions". The progress in this research has been summarized in the review [han79a]. As an example it can be mentioned that the first case of a nucleus with $T_z=-2$, the isotope ^{32}Ar with 18 protons and 14 neutrons, was discovered at ISOLDE in 1977. Some eight years later it could be produced in sufficient quantities at ISOLDE to be used in a precision experiment that served to illuminate the strength and the limitations of the nuclear shell model. This model in its modern form based on effective interactions could reproduce the spectrum of the weak-interaction decay up to 10 MeV excitation energy but not the absolute scale, which is reduced by a factor of two by sub-nuclear effects linked in various ways to the pion field in nuclei, see [bjo85a].

A large number of studies have dealt with nuclear structure of the heavier nuclei by electron- and gamma-spectroscopic methods, see especially the review by Hamilton [ham89a] and earlier work cited therein. We mention just a few examples of the intricate interplay between shapes and shells, which would not have been observed without comprehensive studies that cut across the nuclear chart. Whereas the magic nucleon numbers (2,8,20,28,50,82,126) of the classical shell model favour spherical shapes, other nucleon numbers have "chameleon character". Thus the proton numbers 38,40 (Sr,Zr) when combined with the magic neutron number 50 favour spherical shapes so that ^{90}Zr is very much like a double-closed-shell system. On the other hand the light ($N\approx 40$) and very heavy ($N>60$) isotopes are strongly deformed and have well-developed rotational bands as can be seen from the systematics shown on p. 60 of [rag78a]. In some cases (especially ^{100}Zr) it has been possible to detect the co-existence of spherical and deformed states at low excitation energies in the same nucleus analogously to the findings from studies of nuclear sizes and moments mentioned in the previous Section. Another example is provided by the proton number 64. Gamma-spectroscopic experiments have also been extremely important for the understanding of the Os, Pt, Hg, Pb region.

This is hardly the place to give any more detailed picture of the vast amount of knowledge collected in 30 years of research on the spectra of nuclei away from stability, but it is tempting to attempt to draw a sort of balance. It is the impression of the present writer that the spectroscopic research has been consistently underrated because it requires a large, systematic and often tedious effort that has little "committee appeal". It is only when all the bits of information come together that it is possible to see that a much more interesting and nuanced picture of nuclear structure has emerged; it is

¹ The sign convention for T_z is opposite from that used in particle physics.

noteworthy that many of the most interesting features had not been foreseen by theory although they were usually abundantly explained after the fact.

6.4 EXOTIC DECAYS, STRENGTH-FUNCTION PHENOMENA AND STATISTICAL ASPECTS

In Section 6.3 examples were given of how new facets of nuclear structure appear when nuclear composition is varied. We now turn to examples of how nuclei near the nuclear drip lines show properties that nuclei do not possess near stability. These phenomena are linked to the asymmetry in the binding energies. While beta-stable nuclei necessarily have roughly equal binding of protons and neutrons, typically 6-8 MeV, the nuclei at, say, the proton drip line have nearly zero binding of the protons while the neutrons are bound by 10-12 MeV. This means that even quite low-lying excited levels are unstable with respect to proton (and usually also alpha) emission, which therefore can be excited in beta decay. This process is usually referred to as beta-delayed particle emission and has already been encountered in Section 2.3 and it was used for detecting the very proton-rich light systems mentioned in Section 6.3.

Soon after the start of the first ISOLDE, beta-delayed proton emission was seen in nuclei as heavy as xenon and soon after in mercury. After the start of ISOLDE-2 the (much weaker) competing process of beta-delayed alpha emission from proton rich nuclei was also found. These spectra and the corresponding spectra of neutrons from very neutron-rich nuclei were important giving information about the density of beta transitions to very high-lying states, the so-called beta strength function, defined in a footnote to Section 4.5. and reviewed in [han73a,han79a]. These processes and similar excitation phenomena following muon capture (see Section 3.3) are interesting in that they are dominated by giant-resonances not unlike those encountered in electromagnetic excitation processes. It is also a giant-resonance phenomenon that explains the different beta decay behaviour of nuclei with positive and negative isospin projections (Section 6.3): Superallowed beta decay is encountered essentially only for $T_z \leq 0$.

The main features of the very complex particle spectra following beta decay of the heavier nuclei could be understood on the basis of a statistical theory containing reasonable estimates of the strength functions involved [hor72a]. It was soon realized that some experiments also saw a pronounced fine structure that reflected the fluctuations in the composition of the nuclear wave function of the individual levels [lyn68a], and that the classical statistical model of the nucleus due to E.P. Wigner and others gave very definite predictions for the intensity (and its variance) of the emitted particles and gamma rays. Measurements of the fine structure of particle spectra has in some cases allowed the determination of nuclear level densities at high excitation energy, see the review by Jonson [jon76a].

An interesting application of beta-delayed protons pioneered by the Canadian physicist J.C. Hardy was the use of coincidences between the protons and K x-rays (from electron-capture beta decay) for measuring lifetimes in the 10^{-16} s region. The x-ray energy is larger if the proton is emitted after the x-ray so that the branching ratio ties the proton lifetime to the (known) atomic 1s lifetime. (See e.g. [asb81a].)

The light neutron-rich nuclei [det89a] have been found to display a number of interesting exotic decay modes such as the emission of two and even three neutrons following beta decay, first observed [azu79a] in the decay of ^{11}Li with a half-life of only 8.6 milliseconds via the use of time-correlation techniques with a large neutron detector. Multi-neutron emission has also been detected and later also in the sodium isotopes $^{30-}$

^{32}Na , and recent experiments find rare decay modes involving charged particles such as deuterons, tritons and ^6He , see e.g. [lan84a, bor86a]. It was clear from the beginning that the energy spectra of these particles reflected a very special aspect of nuclear structure, namely spectra where all excited states except a few low-lying ones were unbound and decayed with very large particle decay widths. The detailed investigation of these structures, however, was very difficult and was not pursued with much energy.

In the last few years it has, however, become clear that the neutron-rich nuclei have extremely interesting properties and a large number of theoretical and experimental papers on this subject have appeared. (For a popular presentation see e.g. [han91a].) It has turned out that the extremely low neutron binding near the neutron drip line permits the last neutron or pair of neutrons to travel far away from the core via quantum-mechanical tunneling so that it forms a neutron halo that may extend to several times the normal nuclear radius. Due to the physical separation of the neutrons from the protons, these nuclei violate some of the most basic rules in nuclear physics: The mass and charge radii are no longer identical and certain electromagnetic and beta-decay processes appear with essentially their full single-particle strength and are no longer quenched by giant-resonance phenomena.

6.5 APPLICATIONS TO ATOMIC AND SOLID-STATE PHYSICS

The atomic beam and laser techniques discussed in Section 6.2 were put to the service of atomic physics in a search for optical transitions in the alkali element francium ($Z=87$). In 1978 Liberman and his collaborators [lib78a] succeeded in observing its D_2 line at a wavelength of 717.7 nm. (The D lines of francium represent the transition $7s \rightarrow 7p$, split by the fine structure into components D_1 and D_2 . Such valency-electron transitions are characteristic of all alkali elements.) Francium, appropriately named after the country of origin of this group, is a rare radioelement, which until then had remained the only element with $Z < 100$ for which no optical transition had been reported¹. The reasons why such a measurement had not been done earlier were the short half-lives of the francium isotopes (the longest is 22 minutes), and also the low production rates in most laboratories. ISOLDE's 10^8 atoms/s of Fr thus provided a decisive advantage in this experiment, which in turn opened up for studies of properties of nuclei of francium through hyperfine-structure spectroscopy. Later experiments have observed also the D_1 line [ben84a] and also the second fine-structure doublet, the D' lines (in the blue) representing the $7s \rightarrow 8p$ transition [duo87a].

In another series of atomic-physics experiments, in which the high production yields at ISOLDE also played a central role, the relative energies of some atomic K x-rays of medium-weight and heavy elements were measured with a precision of 10-100 meV (milli-electronvolts), that is to within one thousandth of the natural line width. The experiments compared the energies of x-rays from intense sources of electron-capture (EC) radioactivities produced at ISOLDE with each other and with those of sources from photo-ionization. Shifts in the energies of 0.1 to 5 eV were observed [bor77a], and turned out to arise from contributions that had not been considered previously. One of these effects was the hyperfine shift (analogous to that observed in muonic x-rays and

¹ In its presentations the group often pointed out that the experiment also represented the first discovery of a new alkali D-line since Bunsen and Kirchhoff discovered the corresponding transitions in rubidium and caesium around 1860. It is almost overwhelming to consider the number of new tools and concepts in physics that were on the "critical path" to this the next, in principle simple, step.

in optical spectra, Sects. 3.2 and 6.2). The hyperfine shift became observable in the ISOLDE experiments because the x-rays from beta decay are subject to a previously unnoted angular-momentum selection rule that leads to a non-statistical population of the hyperfine components. Another effect was a small increase in the energies of x-rays from photo-ionization due to final-state excitations ("shake-off"). A third series of experiments could demonstrate a shift reflecting the fact that an element undergoing (EC) beta decay has one valency electron more than the daughter atom. This leads to very characteristic shifts in the energies of the K-series of x-rays. The experiments showed that for the rare-earth metals the additional electron was located in the 4f level. This is exactly where chemists and solid-state physicists would like it to be in this case, but the method offers a more general tool for testing electron assignments.

In the mid-seventies several solid-state physics groups began to take an interest in implanting the radioactive beams from ISOLDE into solids with the purpose of using the radioactive nucleus as a probe for the internal fields in the host material. The main detection methods used were the Mössbauer effect and perturbed angular correlations, the latter based on the observation of an (often minute) shift in the angular correlation between successive radiations. This shift arises from the precession of the nuclear magnetic dipole and electric quadrupole moments in the ambient fields. The methods are attractive because of their high sensitivity; a total of 10^{10} impurity atoms will suffice in many cases and the signal corresponds to the conditions on a single location, something that no macroscopic method can match. The solid-state applications at ISOLDE have continued to grow in importance to this time, at which they account for about 25% of all experiments at ISOLDE. The reviews by Haas and by Weyer [haa86a] give an account of and references to much early work in this field of applications.

7. Another Discussion About the Future of the SC: 1979-81

7.1 THE PLANS FOR SIN-ISOLDE

By 1978 CERN was heading towards an approval of LEP, the Large Electron-Positron Collider, which was to be built with "constant budget" meaning effectively that there should be no decrease in budget after the SPS construction period. Still, there was a need to make economies, real and symbolic sacrifices that could convince the member countries that the situation was earnest. The Directors General, John Adams and Leon van Hove, proposed to link the construction of LEP to a closing of the ISR and the SC, which permitted the happy formulation that "CERN would be closing two accelerators in order to build one" and they suggested that ISOLDE should investigate the possibilities for a continuation of its programme at the Swiss meson factory SIN.

ISOLDE responded by setting up an ISOLDE-3 Working Party¹ to study the technical and scientific aspects of this suggestion and contact was taken with the SIN Director, Jean-Pierre Blaser, who appointed John Domingo as the linkman for this project. It was realized that the possibilities were very attractive. Since the running always would be in parallel with other users, the beam itself would cost little and operation corresponding to 500 eight-hour shifts per year would be possible, thus fulfilling a long-standing wish of the Collaboration. After a planned upgrading of SIN the 600 MeV proton beam was expected to reach an intensity of 1-2 mA, so that without causing major disturbance to other users it would be permissible to divert 20 μ A of these to ISOLDE for about 400 shifts and 100 μ A for 100 shifts per year. The improvement over the existing ISOLDE would then amount to factors of 3 and 16 in the two cases. It also emerged that it would be possible to take the beam out in a pulsed mode, which would lower the background and give higher intensities in experiments in which the separator beam has to be gated off during the measurement period.

Technical studies of lay-out, radiation safety, and remote handling of the very radioactive target-ion-source units were carried out [kug79a, sin81a] during the following months. The design preferred by the Group was one with two separators and two target areas connecting directly to a handling bay with industrial robots for safe handling of very large amounts of radioactivity. This solution provided the possibility of operating the two systems alternately² (in "push-pull"), so that the radioactivity would have time to "cool off" for a longer period before interventions by personnel in the target areas. A lay-out with one target area was proposed as a cheaper but also more risky alternative. From SIN Domingo looked into the economic aspects of the move. He assumed [dom79a] that SIN would provide office and general laboratory space (at least 600 m²) for an on-site load of 50-60 persons and the use of all general services such as electronics pool and purchasing office. The in-house group of about 25 persons would be provided by CERN. It was left open who should pay for the most expensive items,

¹ Members of the ISOLDE-3 Working Party were B. Jonson (Chairman), E. Kugler, H.L. Ravn, S. Sundell, all from the CERN-ISOLDE Group, F. Blythe from the SC Engineering Office, and P.G. Hansen (Aarhus), who participated as the Chairman (1978-81) of the ISOLDE Collaboration.

² The ISOLDE Coordinator, Björn Jonson, had in 1977 studied a extension involving a second target area in the proton hall of the SC. The arrangement that finally resulted at the SC also incorporated two separators, the old one (ISOLDE-2) and one in the proton hall (ISOLDE-3, a high-resolution instrument [all87a]). A similar lay-out was chosen for the facility constructed from 1990 and on at the PS-Booster.

the building (11-15 MSFr) and its separators, hot-cells etc., but a sharing in some form between SIN, CERN and the ISOLDE Collaboration was tacitly assumed. The technical plans were presented in a condensed form in a paper by Domingo et al. [dom81a]. Fig. 15, adapted from this paper, gives a good impression of the scale of the proposed installation.

It is probably useful to look briefly at two precedents in order to understand the CERN-SIN-ISOLDE relationship that was tacitly assumed in these deliberations. SIN had obtained an agreement that quantified the participation of German and Austrian research teams in its programme and had in return obtained financial contributions towards its operation from the two countries. The question was now whether it would be justified for CERN to sponsor ISOLDE in a somewhat similar way. Technically the answer was clearly yes, since, at the time when the 70 GeV accelerator at Serpukhov (U.S.S.R.) was the world's largest proton machine, CERN had participated in the construction of equipment for experiments and had maintained a team there for an extended period. An entirely different question was whether CERN would find such a solution desirable, which introduced as an issue whether CERN held a responsibility for this area of research?

ISOLDE had not forgotten Adams's "priorities" and the final outcome of the 1972 debate (Sects. 5.3 and 5.4) and had begun to fear that it was being lured into an alley that would lead via an ambitious proposal to general praise of the proposed programme's scientific merits and finally to a flat rejection on financial grounds. This would then be the result that some, most likely, had hoped for in the first place. This explains why the final proposal [han79b], which was adopted by the ISOLDE Committee on 6-7 November 1979 and immediately forwarded to the PSCC Committee, incorporated as an alternative to the move to SIN the continued use of the SC as a dedicated injector for ISOLDE. The discussion of economy in the document shows that the problem was real enough: Although the SIN solution would become cheaper in the long run owing to its smaller operational costs, it showed a peak in investments during 1982-85, just in the middle of the LEP construction period. Both the document and its covering letter expressed the view that for the long-term future of the field, the SIN Facility would be the ideal installation, but also that in the short run, defined as the next 5-7 years, the SC-ISOLDE would produce more physics. The letter also underlined the notion of responsibility: That the existing ISOLDE programme had become possible only because of European cooperation within the framework of CERN and that it was essential for its continuation at SIN that ISOLDE should remain a CERN activity.

A couple of months later estimates of construction costs for the SIN project were submitted to the CERN Management by the EP Division Leader, Erwin Gabathuler [gab80a]. For the solution with two separators the total cost was estimated to be 23.4 MSFr of which the building cost was 15 MSFr. The total cost for a facility with one separator was 17.3 MSFr. Apparently it was only a year later when the pendulum had begun to swing the other way that the PS Division Leader, Gordon Munday, was asked to provide forecasts of SC staff and running budgets for a continued operation at different levels of exploitation [mun81a].

7.2 THE DISCUSSIONS IN CERN'S COMMITTEES

The PSCC met on 14 November 1979 under the chairmanship of Robert Klapisch. Its discussion about ISOLDE concluded (i) that the community centered around CERN had an undisputed leadership in its specialty and (ii) that CERN held a

responsibility for maintaining this field since there was no laboratory in the member states in a position to support an effort of the necessary magnitude. The PSCC therefore proposed that formal negotiations with SIN and the Swiss authorities should be undertaken immediately. It also decided to launch a call for "declarations of interest" including also laboratories outside the ISOLDE Collaboration. And finally the PSCC announced its intention to appoint external referees to review the programme that would emerge from this procedure. The last step was unusual for CERN but not unseen, and, most likely, the idea had been cleared with van Hove. The point was that essentially all physicists at CERN who were knowledgeable in matters pertaining to ISOLDE also were actively involved, including, of course, Klapisch himself. The Committee therefore felt that it would be more certain to be of service to CERN if its final recommendations were based on a peer review by nuclear physicists unconnected with ISOLDE.

On the following day the Research Board¹ accepted the PSCC recommendations and also accepted to include in the CERN planning the possibility of a continued, reduced operation of the SC for a few years in a mode dedicated to ISOLDE. The latter decision meant that the possibility of the continuation of the programme, possibly only during a transition period, was kept open.

The future of the ISOLDE was discussed in a series of meetings of the Scientific Policy Committee. On 16-17 September 1980 [spc80a] there was a presentation of the plans by Klapisch followed by a summary by Blaser of SIN's position and by a long general debate. One of the questions was whether the increase in beam intensity would have major impact on the programme, to which representatives of ISOLDE (Hansen and Jonson), who had been invited for this part of the Meeting, answered that there were several arguments in favour of a transfer to SIN: (i) The beam would be cheap because a meson factory was a very cost-efficient way of accelerating protons, and (ii) a more modern and safer facility would also produce more physics. Finally, (iii) a gain in intensity of one, maybe two, orders of magnitude was obviously important, but the scientific programme would not be changed qualitatively, at least not in the first five to seven years of operation. Klapisch added that only recently had experiments emerged at ISOLDE that required high intensity such as some of the optical experiments and the work on x-rays (see Sections 6.2 and 6.6).

Ingmar Bergström referred to a debate that had taken place in the Swedish CERN Committee shortly before and in which the conclusion had been that ISOLDE should remain at CERN. He said that there was a danger of breaking up a collaboration that it had taken two decades to create, and also, very much in line with his position in the 1972 debate (Sect. 5.4), that the contact with CERN was an important source of scientific and technical inspiration for the national laboratories in the member countries. He saw no need for any independent evaluation of ISOLDE's activities.

To the last point the Research Director General replied that he was in favour of an evaluation for two reasons. The first was that ISOLDE was used by only five of CERN's member states and the second that, to quote the minutes of the Meeting, "while in no way doubting the scientific merits of ISOLDE, one could ask why a facility of such quality had not been built elsewhere." The last argument brings to mind the

¹ We remind readers that this Committee consists of CERN Management, Committee Chairmen, Coordinators (when necessary), and some external representatives. It is essentially equivalent to its predecessor, the NPRC, mentioned earlier in this paper.

tendency of many at CERN during its first decades to have an inferiority complex towards the U.S.A.; in fact on the tape recording of the discussion one hears that van Hove asked "why didn't others do it, for example in the States?"

The Director General designate, H. Schopper, was in favour of a continuation of operation of ISOLDE at CERN at the level foreseen in the scientific activities document (CERN/SPC/456/Rev.) since it "would be many years before the costs arising from the transfer and upgrading of ISOLDE could be offset by the savings made from the closure of the SC". Not surprisingly, it was his view that was to prevail.

ISOLDE was discussed again in the SPC on 29 October [spc80b], in which Klapisch summarized the progress. He felt that it would be a mistake to miss the opportunities offered by SIN and suggested that in order to reconcile "the short term and long term¹ requirements", the new projects should be shifted in time so that any major capital outlay would be incurred after LEP construction. At the Research Board Meeting on 29 January 1981 it was decided that further information was needed, but from the notes taken during the meeting by Y. Goldschmidt-Clermont [gol81a] it was clear that Schopper favoured a continuation at CERN on short term followed on long term by SIN under other (meaning non-CERN) arrangements. This meant, as pointed out by Klapisch, the necessity of finding "a responsible third body with a budget".

The next large discussion in the SPC took place on February 25 1981 [spc81a] and was opened by a presentation by Klapisch, who reported that the external referees "had confirmed that the importance of ISOLDE was beyond doubt". Two members (Cabbibo, Bergström) requested more detailed information on the contents of the letters from the referees. The Chairman, V.L. Telegdi, said that the SPC should be given an opportunity to study them before a decision was reached although it would mean a further unfortunate delay. In retrospect it appears surprising that these letters, which had been taken to be of such key importance at an early stage, had not been made available to the SPC. It is tempting to guess that the new CERN Management, and which Klapisch himself soon was to join as a Research Director, had found the letters too positive, i.e. that they would justify larger expenditures than a Directorate, which saw LEP as its one essential task, was prepared to make.

7.3 A DECISION ON THE FUTURE OF ISOLDE

For the next SPC Meeting a collection of documents [spc81b] had been prepared including extracts of PSCC and RB documents and a list of the 31 "Letters of Interest" received from interested scientists. The most interesting part in a historical perspective, however, is the collection of letters from the eight referees², two nuclear theorists, four nuclear experimentalists, one atomic physicist and one solid-state physicist, letters which provide a rare picture of how this research was viewed by eminent outsiders, who in the past had had little or no contact to ISOLDE.

The programme had very positive comments from all the referees; three of them were in favour of an all-out effort to construct the ultimate facility, the SIN-ISOLDE.

¹ Towards the mid-eighties, as high-energy physics was becoming increasingly commercial, the same phenomenon would have been referred to as a "cash-flow problem".

² The referees were D.A. Bromley (Yale), V. Gillet (Saclay), P. Kienle (TU Munich), S. Lundquist (Chalmers), B. Mottelson (NORDITA), O. Nathan (Niels Bohr Institute), G. zu Putlitz (Heidelberg), and H.J. Specht (Heidelberg).

The others for a variety of reasons rather preferred continued activity under CERN auspices. The following are extracts from the comments given by two of the most perspicuous of the majority. D. Allan Bromley, presently science adviser at the White House to President George Bush, wrote:

(1)...On several occasions within the last years when I have been asked to provide a review of the most challenging and significant developments in worldwide nuclear science, I have had occasion to take a look at the ISOLDE output and, in each case, found examples for which I was searching in that work. Obviously, the facility was well conceived initially, and it has attracted to it an outstanding international group of nuclear scientists who have, on the whole, selected very wisely, in terms of problems for study...

(2) The question sometimes arises as to why other major activities of the scope of ISOLDE have not been mounted in the U.S. and elsewhere. I believe that the answer is rather simple and is connected to my comment above. The ISOLDE group got such a head start on the rest of the world activity in this field that people were very reluctant to attempt to mount a competitive operation. The largest such activity in the U.S. is clearly the UNISOR project associated with Oak Ridge National Laboratory accelerators, but this is very much smaller than that at ISOLDE...

(3,4) ...it is clear that almost without exception one can find some aspect of the program that would benefit from the proposed move to SIN, (but) I would not consider that the change, in any case, would represent a quantum jump,...(and) I have not been overwhelmed by examples of totally new science that the move would make possible...

(5) It is also clear (...) that there is still excellent science to be done with the existing facility at CERN, particularly if it were possible to obtain more running shifts and more research space...

(7) ...I do not find evidence of the dedicated, experienced, group of people who would actually accomplish the move to SIN... Without such a dedicated group of people, all of whom are prepared to devote several years of their professional careers to this move, largely for the public good, the whole project would appear doomed. It may well be that such a group exists. I have not, however, seen any evidence for it...

(9) ...(I) would suggest that ISOLDE should stay at CERN...

(10) ...The experimental work shows real elegance and expertise, and I have the feeling that the science could be even more sharply pointed toward questions of central topical interest, if more theorists were directly associated with the project...

It will be seen that Bromley inadvertently answered the question that had been raised by van Hove (Sect. 7.2). The following are extracts from the comments made by Hans J. Specht:

... if the nuclear physics history of the seventies will be written, the wealth of results obtained on nuclei off stability at ISOLDE will take a very prominent place, comparing with those from the seemingly more glamorous field of heavy ion physics... I therefore strongly plead for a continued and substantial support of the project... Any geographical change should not lead to a years long interruption... Reading through the impressive number of letters of interest it is noteworthy that the majority of them argues about the virtues of an increased current in rather vague terms (quite often beam time seems to be the problem). ...only (the x-ray experiment) makes a convincing case... Much higher gain factors than 3-16 may be buried in future developments of the target-ion-source system...(to) put it in radical terms - if the same manpower (...) needed for the

installation at SIN were put into the target-source problem, where would be the greater pay-off? ...

I favour, in conclusion, a continuation of the ISOLDE 2 physics programme for the next few years... a decision on the site could be delayed by another 1-2 years without hurting progress in physics. ...detailed thoughts could be given to an integration of an accelerator for radioactive ions into the concept right from the start...

Specht elaborated his views on acceleration of radioactive ions in a footnote, in which he suggested that a workshop should be arranged on the subject. In hindsight it is regrettable that this suggestion was neglected by all who saw it. As has been mentioned in Sect. 5.5, the idea had been considered and rejected at the 1977 Workshop on Intermediate Energy Physics. It came to life again in the late eighties under the impact of important experiments in nuclear physics carried out with the Bevalac (Berkeley) and GANIL (Caen) and also influenced by the realization that these techniques had important applications in nuclear astrophysics. At the time of writing there is a considerable number of working experiments and projects in this field.

In its Meeting on 28 April 1981 [spc81c] the SPC then as a final conclusion "approved the idea to postpone moving ISOLDE outside CERN, specifically to SIN, and persisted in its previously formulated recommendation, that the beam time of 4000 hours at the SC be preferentially used by ISOLDE". The Management interpreted this in favour of a continuation of heavy-ion and μ SR activities "at a modest level" to be decided by the PSCC [psc81a].

7.4 EPILOGUE

The final decision came as no surprise to those involved; both at SIN and at ISOLDE the feeling had for some time been that the whole procedure was a ritual dance and that the SIN-ISOLDE project was doomed. Doomed at CERN because neither the old nor the new Directorate were willing to invest in strengthening this field, and doomed at SIN, if I may permit myself to conjecture, because no major Swiss physics groups had shown serious interest in joining ISOLDE, so that the Federal Government in Bern for this reason alone was unlikely to sponsor a costly construction programme. In any case the communication lines between ISOLDE and SIN had been silent already since the autumn of 1980.

The LEP construction period 1981-88 was a lesson for those at ISOLDE and SIN who still felt that an opportunity had been missed. CERN's economy was stretched to the limit: It took up large loans, cut services to physics users to a minimum, stopped maintaining buildings and equipment, and took risks on the operational safety of all machines so that it was no longer uncommon for an outside team to lose important parts of its machine time because stand-by duty for some CERN accelerator technician, who could have fixed a technical mishap, no longer was paid for. ISOLDE managed to scrape through, but would have been badly off with a costly and ambitious new programme at a moment when the internal parole was, to quote a senior member of the administration, "Let the last person leaving switch off the lights".

The long discussion about the future of ISOLDE had cost much effort for everybody involved, but it had also cleared the air for a long time to come. The main immediate consequence was that ISOLDE was allowed to build up a second experimental area in the area designated "proton experimental room" in Fig. 4.3 in [mer90a] and to place the target for the second isotope separator, a high-resolution spectrometer referred to as ISOLDE-3, inside the cyclotron hall [all87a]. This solution

had been a long-standing wish of ISOLDE, but had always been refused because of the non-negligible risk of a major radioactive contamination in the area of the cyclotron itself. With ISOLDE taking over the role as the principal user, this risk was now acceptable. This installation served a large number of users during the eighties, and shortly before the final shutdown of the installation in 1990 the high-resolution separator was for the first time used to separate mass doublets, namely the pair ^{37}K and ^{37}Ca , where the latter, produced in minute quantities, happened to be the isotope of interest [gar91a]. The careful weighing of all arguments that had taken place during 1979-81 probably also facilitated the decision making when the eventual transfer of ISOLDE to the PS Booster was decided in 1989 [psc89a].

8. Concluding Remarks

The present account of how some low- and medium-energy physicists interacted with CERN is not impartial. I have chosen to view the events from the angle from which I saw part of them some years back, that of an external physicist, who has come to CERN not as a volunteer but to work on his own projects. I hope that it is absolutely clear that I consider the opposite angle both legitimate and necessary; in other words that a management must ask "Do we really need these people? Is it interesting what they are doing?" I also hope the vehemence of the external users will be viewed as an indirect tribute to the important role that CERN has played and continues to play in European university physics.

The science policy and the making of decisions around the SC take up a major part of the present chapter because, as I said above, this is an essential part of modern science, which cannot be pursued by free individuals in their spare time and at their own cost. Those wishing to participate must accept the conditions: that conflicts of interest exist, that a case must be made, that independent experts will be consulted, and that arguments will be considered and decisions taken. Many university physicists will only with much hesitation admit that such a procedure, in their words "bureaucratic", maybe is necessary in big science. In my experience the principles of documentation, openness and an impartial hearing are valuable at all levels. As far as I can see these principles allow for a more fair and better balanced making of decisions than is usual in civil service and at the universities, where quick decisions and nepotism have a freer run. I believe that CERN, in addition to the direct scientific contributions that it has made to European physics, also has had some beneficial influence on the style of physics management in some of the member countries. There is maybe a side benefit in that the transparent structure of a modern scientific organization makes it possible to retrace the events as has been done in the present paper. The pessimist will add that the closed system also has its advantage: The mistakes are forgotten as discretely as they were made.

Fig. 16 is a quantitative illustration of how the utilization of the SC has developed over the years, but it also serves as a reminder that there are many fundamental activities in science that are so difficult that the appropriate unit of time is one generation. To take two famous examples, we have not finished with the boson that lies behind Fermi's theory of beta decay, nor with the transuranic elements for which he was awarded the Nobel Prize. As an example from ISOLDE's area it may reasonably be argued that the research on far-unstable nuclei began 30 years before the start of the time scale in Fig. 16, namely with the discovery [bje36a] in Copenhagen in 1936 of ${}^6\text{He}$, the first case of a nucleus situated at the neutron drip line. The second nucleus at the neutron drip line appeared exactly 30 years later, and it is almost another 30 years later again that it is becoming clear that this class of nuclei has a very special character, see the remarks in Sect. 6.4.

The SC, with 33 years of active service, [fid92a] is one of the accelerators to have served the longest in front-line research. Very early it made essential contributions to particle physics and it continued to support important programmes in intermediate-energy, nuclear, atomic and solid-state physics until the day when it was worn out. What happened was not that it no longer could be repaired, every piece being individually designed and manufactured, but rather that it represented a technology that had become outdated and consumed too much manpower. The SC was one of the last and largest of

a type of accelerator that will no longer be built, and it was representative of the engineering and science of the fifties, based on mechanics (see Fig.9) and on analogue regulation, where modern technology favours electronics and digital control. For these reasons it was briefly discussed whether it would be possible to preserve the cyclotron in its vault as a museum, see the correspondence in [sin81a]. Very much in line with other events told in the present chapter, it seemed for a short while that the museum solution had a chance because it would be cheaper than a "de-commissioning" as this process is called in officialese. On second thoughts, however, the metal value of the iron in the cyclotron yoke got the upper hand, and the machine will go the same way as most European Medieval castles, which ended by serving as quarries for the villages around.

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Fig. 1

The success of the Nilsson-Strutinsky method in accounting for the energies and shapes of nuclei is commemorated in this ashtray, which Sven Gösta Nilsson had produced and distributed to friends. It shows the ground-state energy of the nucleus ^{236}U as a function of an elongation (cigar-shape) parameter, running from upper left to lower right, and as a function of an asymmetry (pear-shape) parameter running from the mid-line to both sides. The calculation accounts for the following features: (i) The minimum to the upper left is the deformed ground state. The almost equally deep second minimum at larger deformations corresponds to a fission isomer, as discussed in the text. (ii) Along the symmetry line, the road to scission is blocked by a hill, but two symmetrically placed low passes favour the division of the nucleus into a small and a large fission fragment. This finally provided (see [nil72a]) an explanation for the asymmetry in the mass distribution in fission, which at that time had been known for about 30 years.

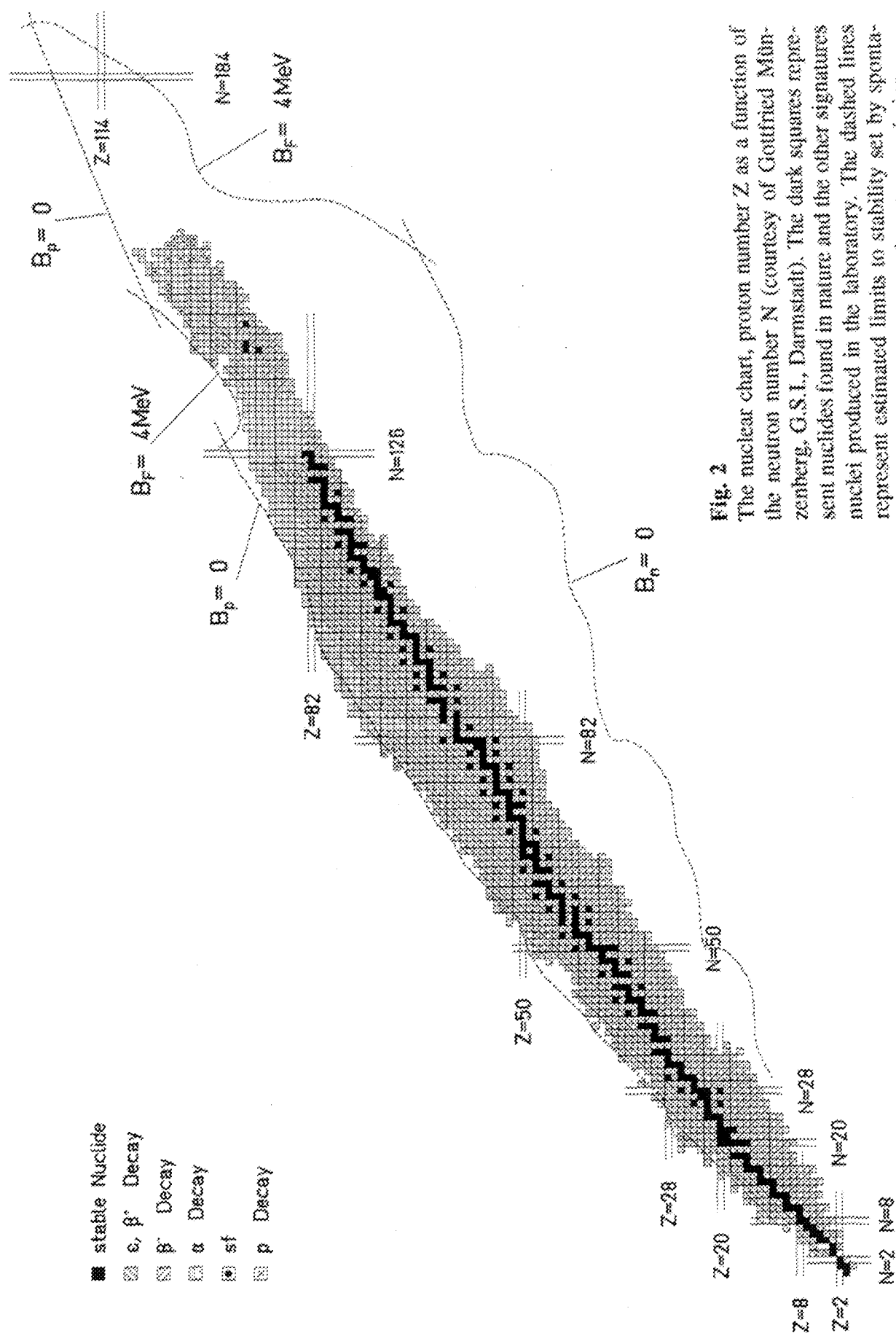


Fig. 2
The nuclear chart, proton number Z as a function of the neutron number N (courtesy of Gottfried Münzenberg, G.S.I., Darmstadt). The dark squares represent nuclides found in nature and the other signatures nuclei produced in the laboratory. The dashed lines represent estimated limits to stability set by spontaneous fission and proton and neutron emission.

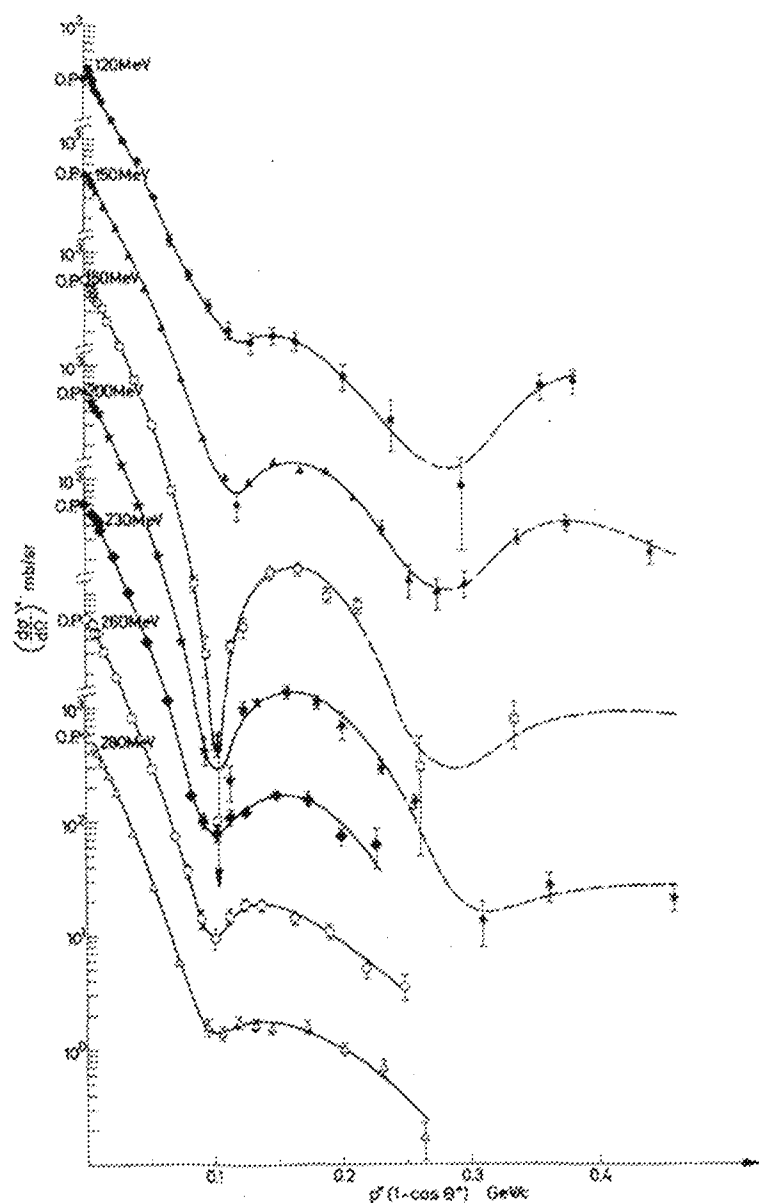


Fig. 3

Elastic differential cross sections for the scattering of π on ^{12}C as a function of the longitudinal momentum transfer in the centre-of-mass system and for pion kinetic energies from 120 to 280 MeV. The points indicated by OP represent the optical point calculated from the measured total cross-section. (From [bin70a].)

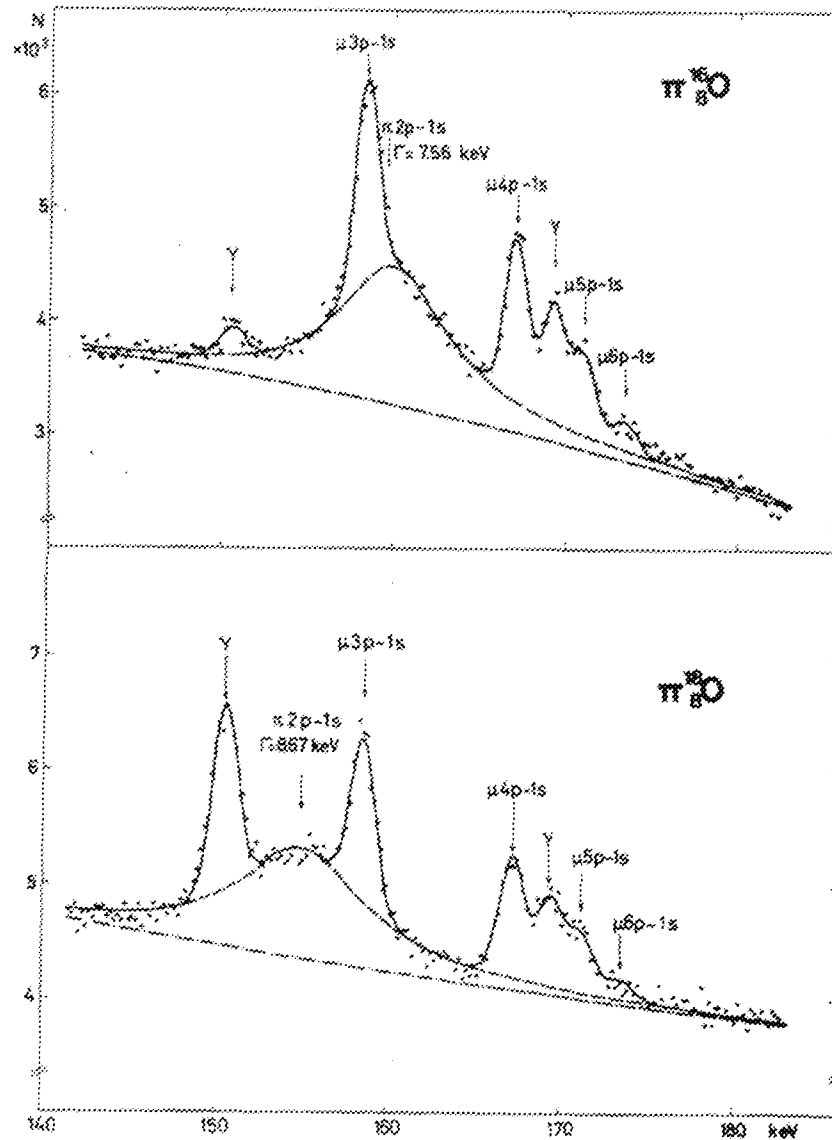


Fig. 4

The $2p \rightarrow 1s$ x-ray transitions in pionic $^{16,18}\text{O}$ appear as broad lines ($\Gamma = 7.6 \pm 0.5$ and $8.7 \pm 0.7 \text{ keV}$ for the two cases). The pionic lines are strongly reduced in intensity by the competing process of nuclear absorption, which explains why the background of muonic x-rays is so prominent. The muon contamination in the beam was actually only a few percent. Note also that the pionic lines do not appear at the same energy, which implies that the interaction depends on isospin [bac67b].

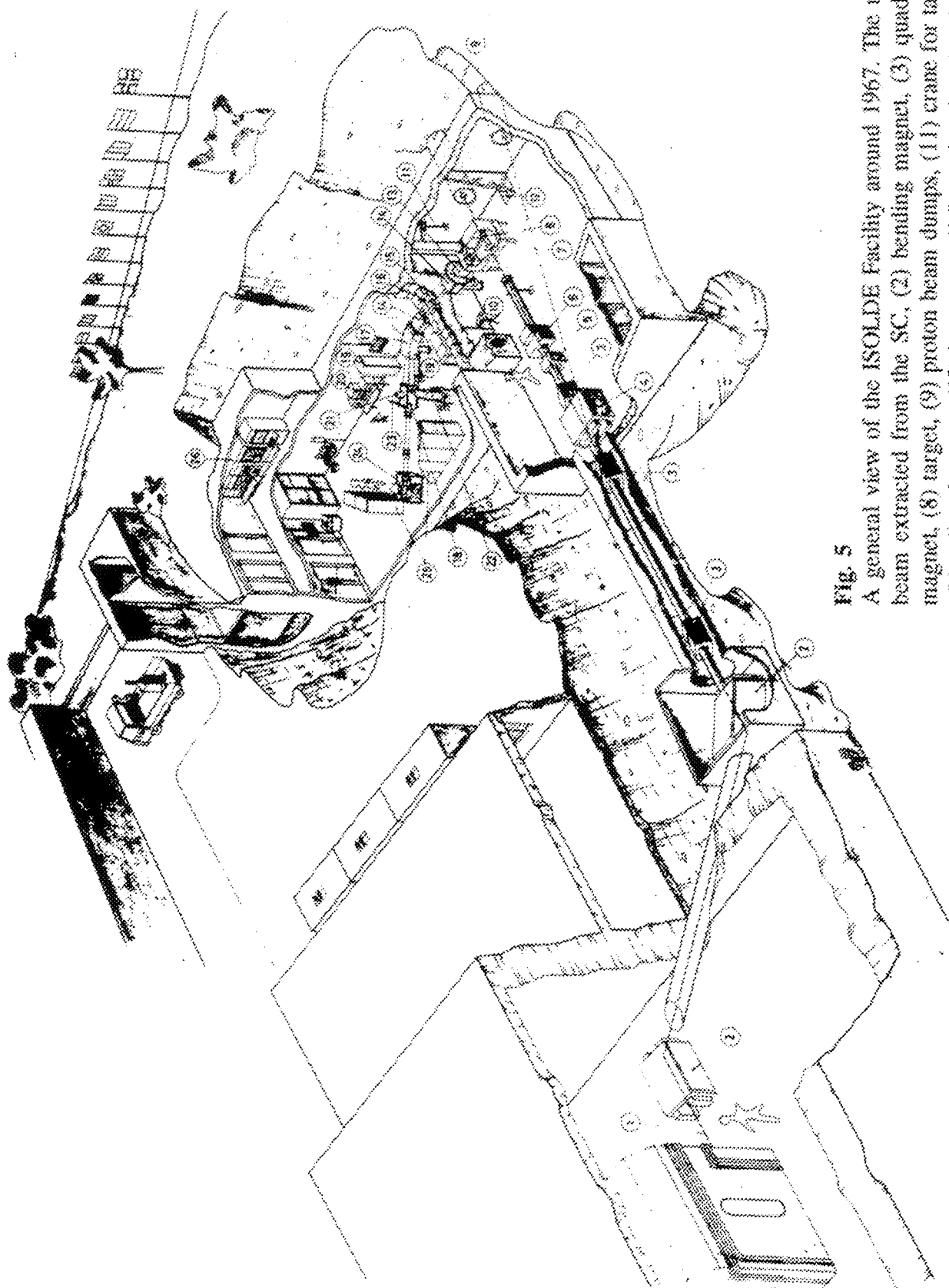


Fig. 5

A general view of the ISOLDE Facility around 1967. The numbers denote (1) proton beam extracted from the SC, (2) bending magnet, (3) quadrupole lens, (4) switching magnet, (8) target, (9) proton beam dumps, (11) crane for target, (12) cave for storage of irradiated targets, (13) ion source, (14) acceleration chamber, (15) shielding wall, (17) separator magnet, (18) collector chamber, (20) control desk, (21-23) experiments inside the collector chamber, (24) experiment at external beam line, and (26) computer.

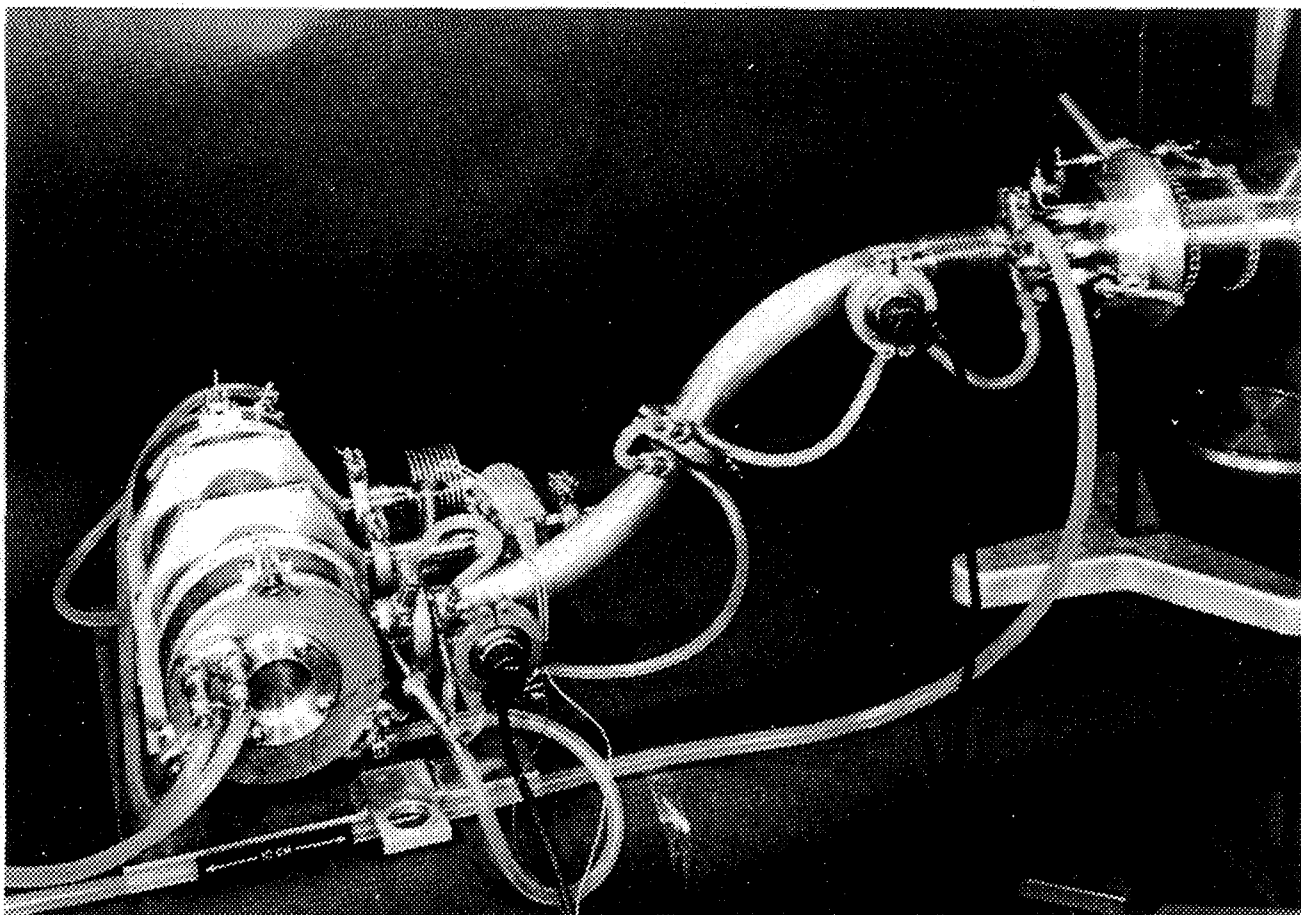


Fig. 6

Laboratory test of the molten-metal ("hot") target system [hag70a] with the oven containing the target trays to the left. The heated transfer line (middle) connects the target to the ion source (right). The proton beam is intended to traverse the target axially. (Photo CERN).

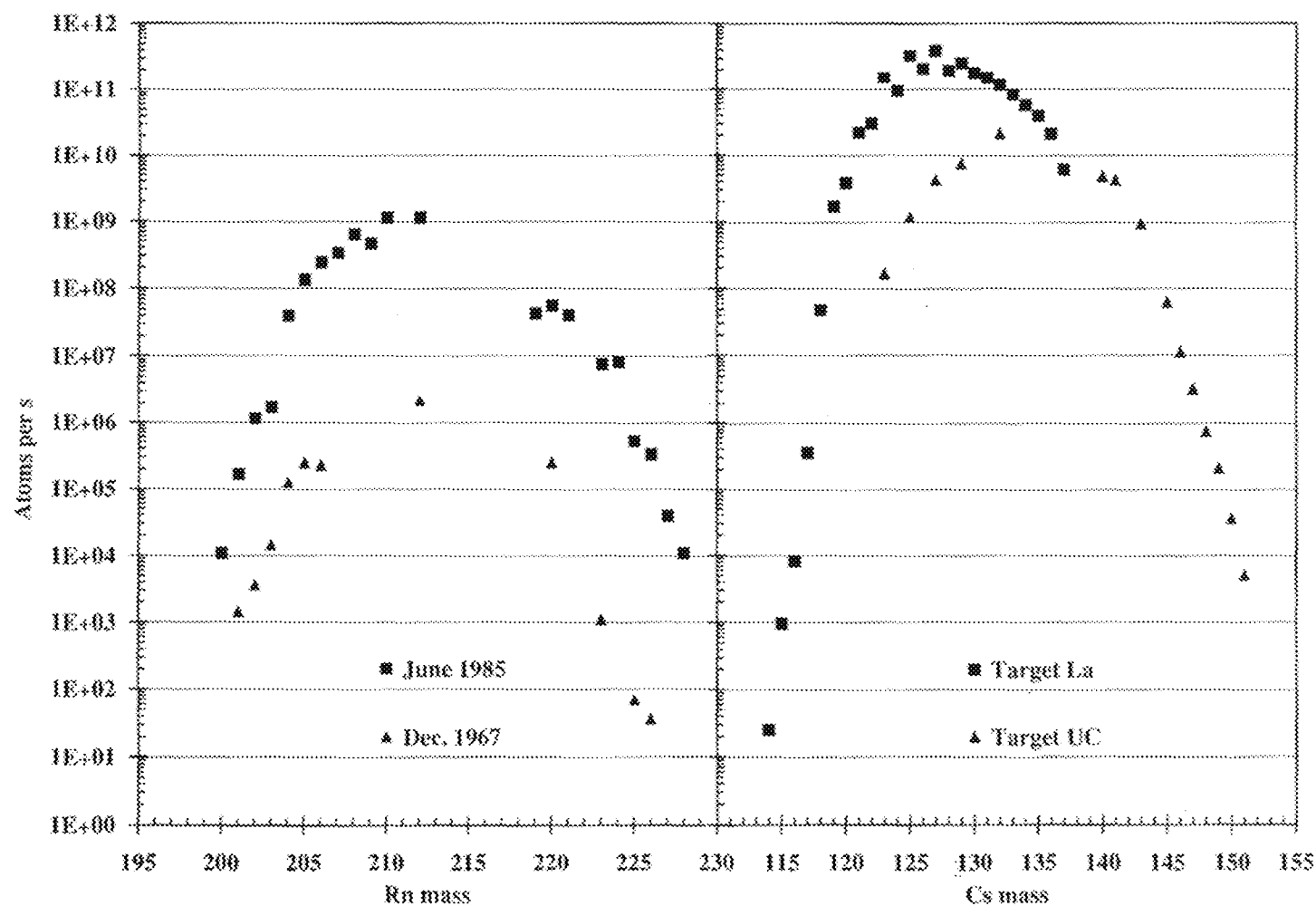


Fig. 7

Beam intensities in atoms/s of mass-separated isotopes of radon ($Z=86$, left) measured on 5 December 1967 (lower left). For comparison is shown the corresponding yield curve measured at ISOLDE-2 and, to the right ISOLDE-2 yields of a lighter element (caesium, $Z=55$) produced from spallation of molten lanthanum metal and from high-energy fission of uranium carbide at high temperature. (Courtesy of H.L. Ravn, CERN).

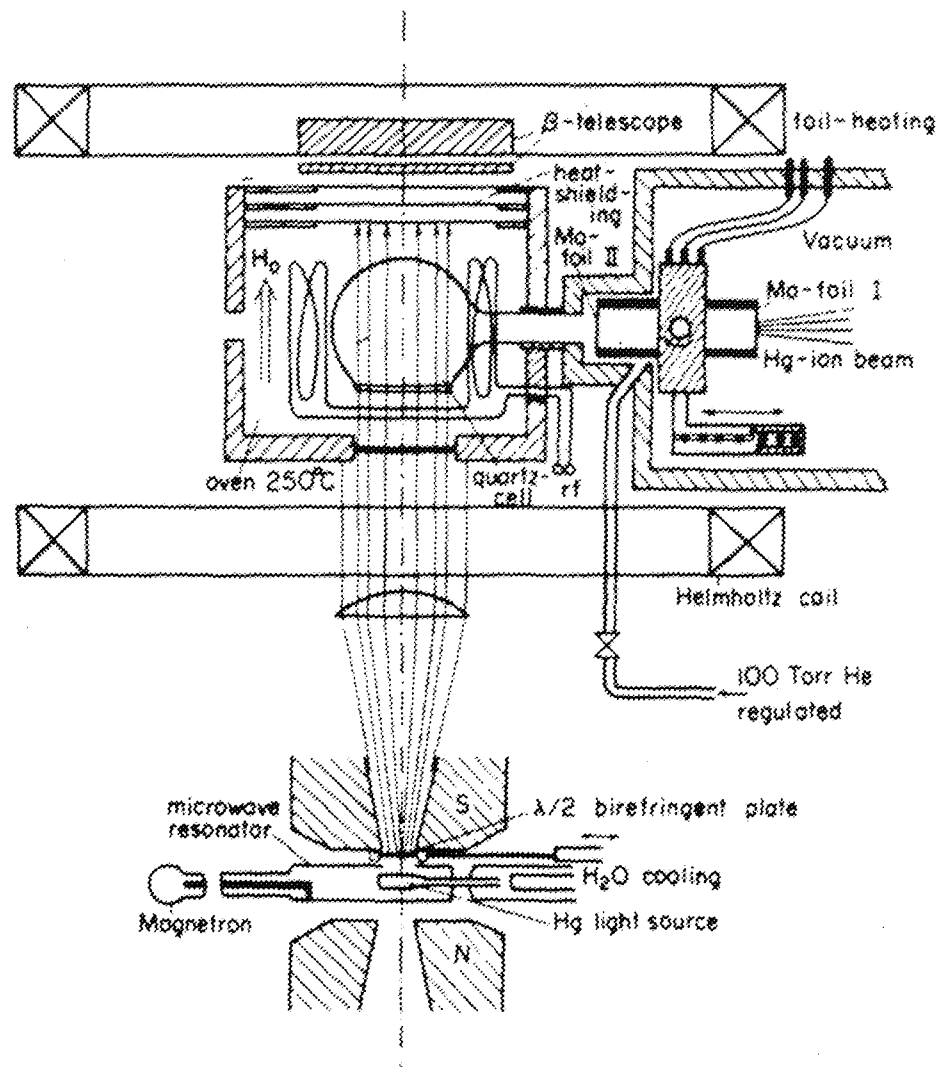


Fig. 8

Experimental arrangement for the RADOP experiments. (From [hub76a].) The upper part of the figure shows to the right the collector for the radioactive mercury atoms from ISOLDE. After each collection it is turned and the mercury is evaporated into a buffer gas contained in a quartz cell, which is heated to prevent condensation of mercury on the walls. The holding field from the Helmholtz coils serves to maintain the polarization. The lower part of the figure shows the mercury lamp placed in a magnet to allow Zeeman scanning, see text.

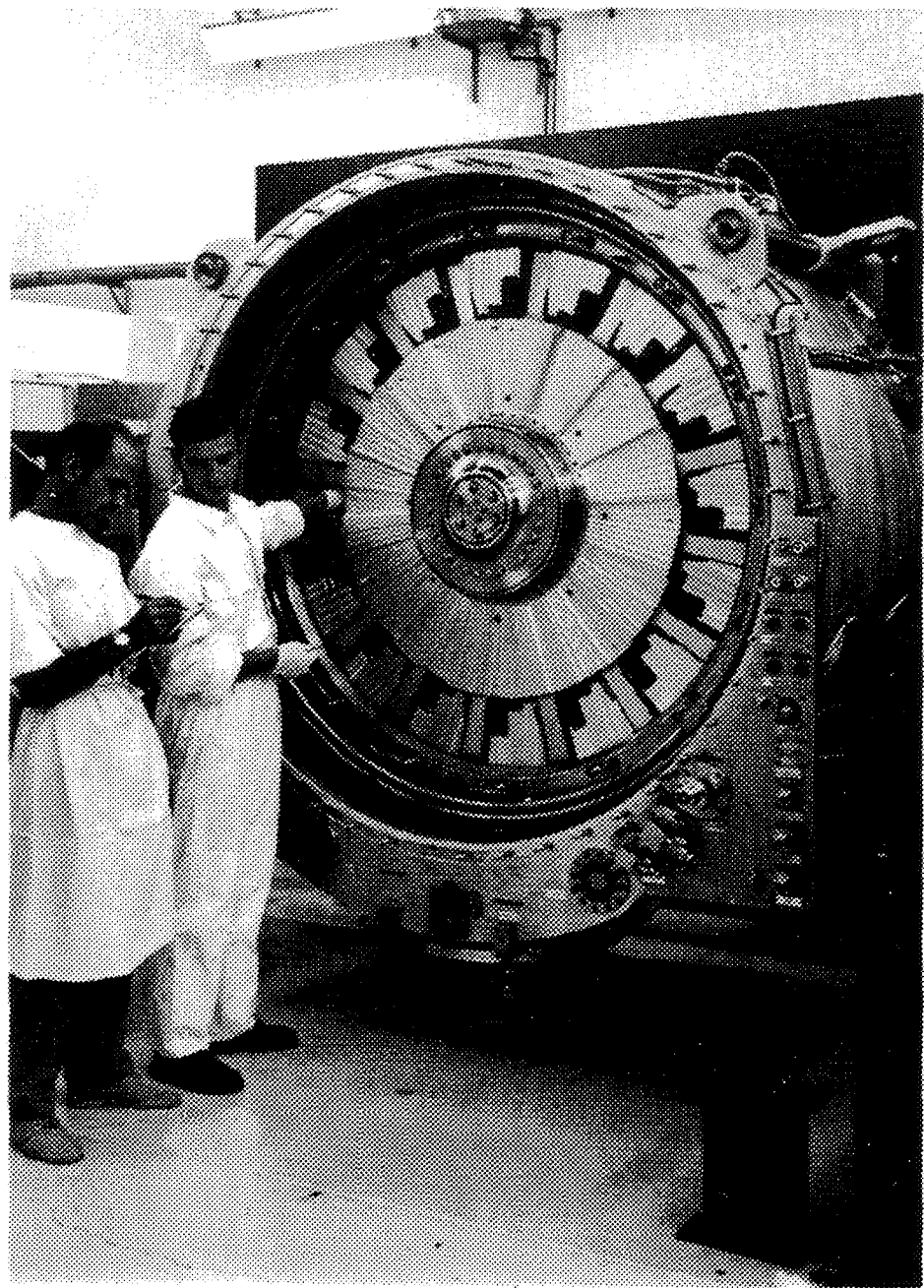


Fig. 9

The rotary condenser for the SC radiofrequency system with the end-plate removed. Note the 16 sets of three rotor blades that pass through the peculiarly shaped stator blades (16 sets of four). (Photo CERN).

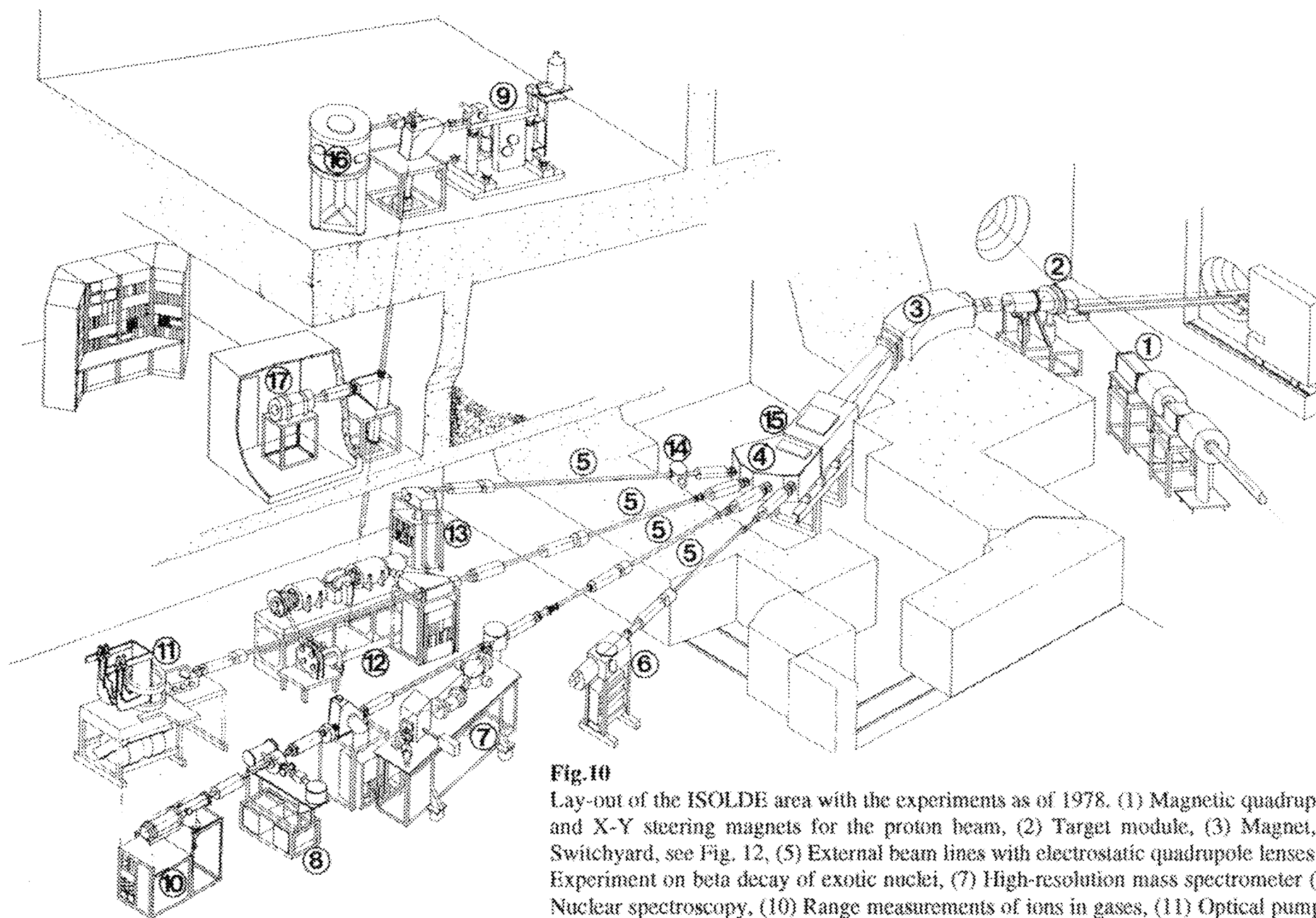


Fig.10

Lay-out of the ISOLDE area with the experiments as of 1978. (1) Magnetic quadrupoles and X-Y steering magnets for the proton beam, (2) Target module, (3) Magnet, (4) Switchyard, see Fig. 12, (5) External beam lines with electrostatic quadrupole lenses, (6) Experiment on beta decay of exotic nuclei, (7) High-resolution mass spectrometer (8,9) Nuclear spectroscopy, (10) Range measurements of ions in gases, (11) Optical pumping and laser spectroscopy, (12) Atomic-beam magnetic resonance, (13,14,15) Collection stations for long-lived radioactive samples for off-line work, (16,17) Experiments on beta decay and neutron detection. The control console of the separator can be seen to

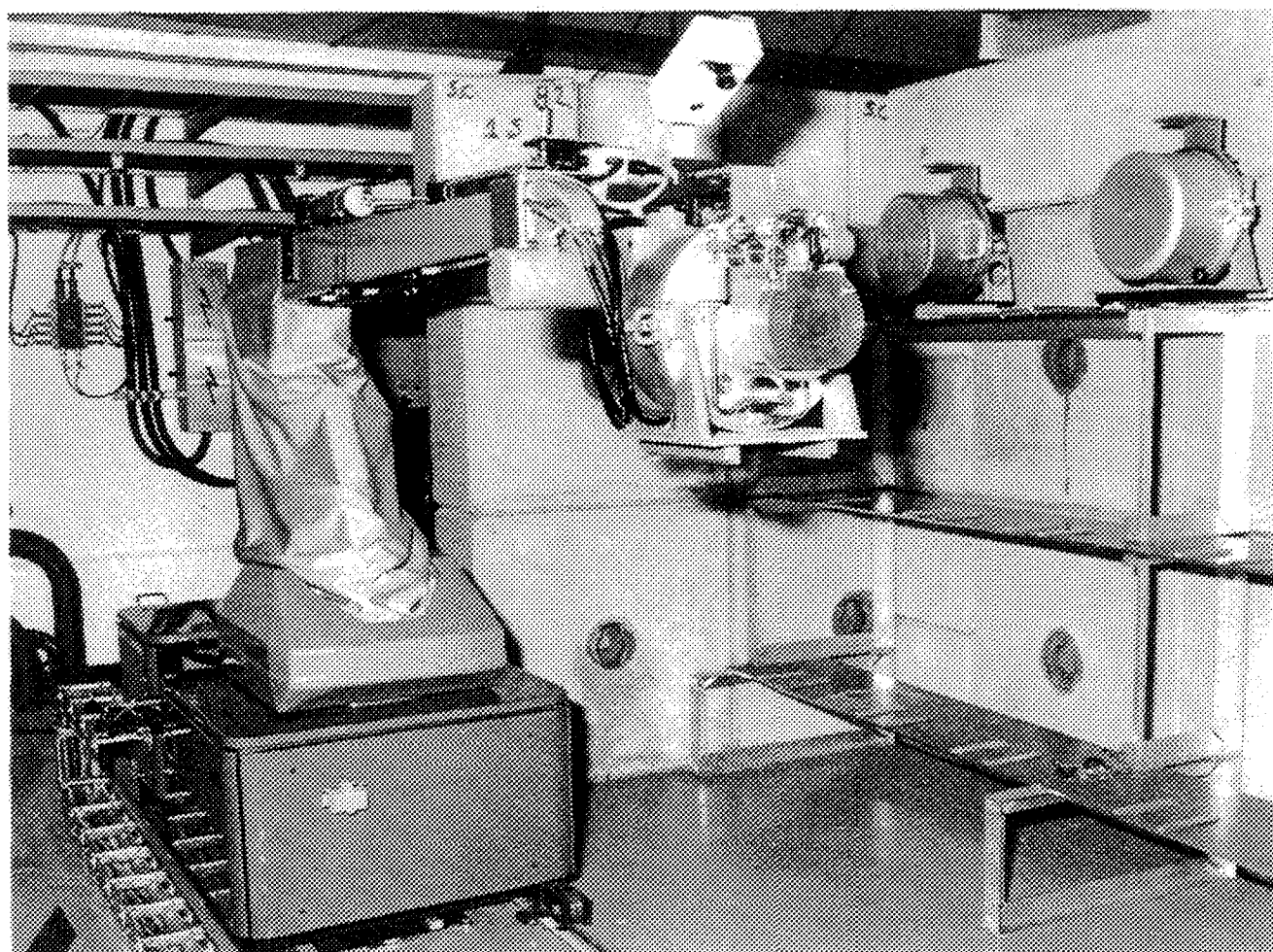


Fig.11

The industrial robot, height about 160 cm above floor level, holding a target module. The operation is surveyed by the TV camera placed on the arm of the robot. Note on the target the vacuum lock and the set of standardized quick-coupling connectors for supplies. Irradiated targets and spares are stored on the shelves behind. The resemblance of the target vacuum container with an aluminium cooking pot is not accidental; to save money the target containers were mass-produced by welding a flange onto a 40 SFr kitchen utensil bought in large numbers directly from the manufacturer. (Photo CERN.)

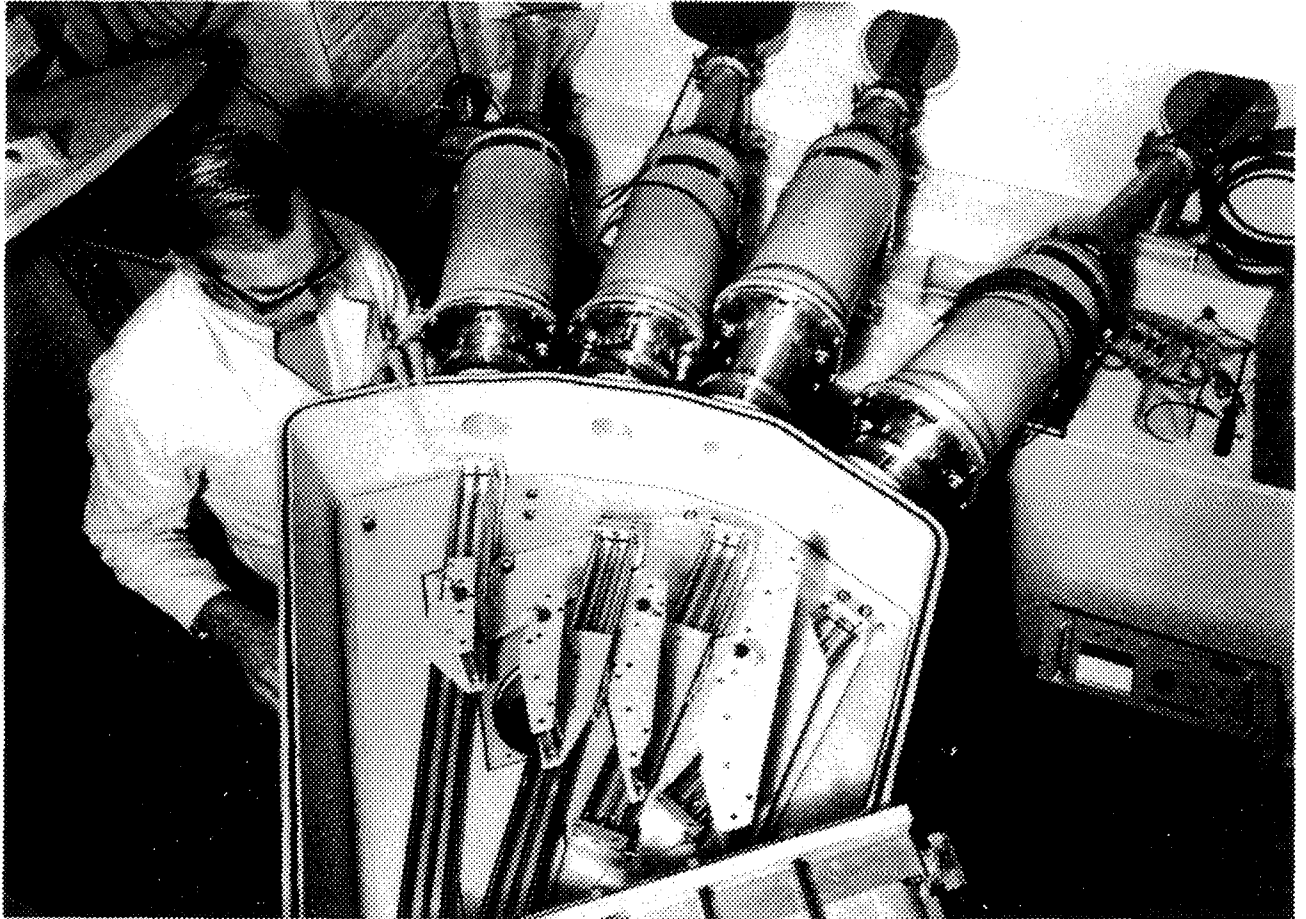


Fig.12

The switchyard of the isotope separator with the top flange removed to show the four electrostatic deflectors, which are movable along the spindles. The exit ports and the quadrupole lenses can also be seen. (Photo CERN.)

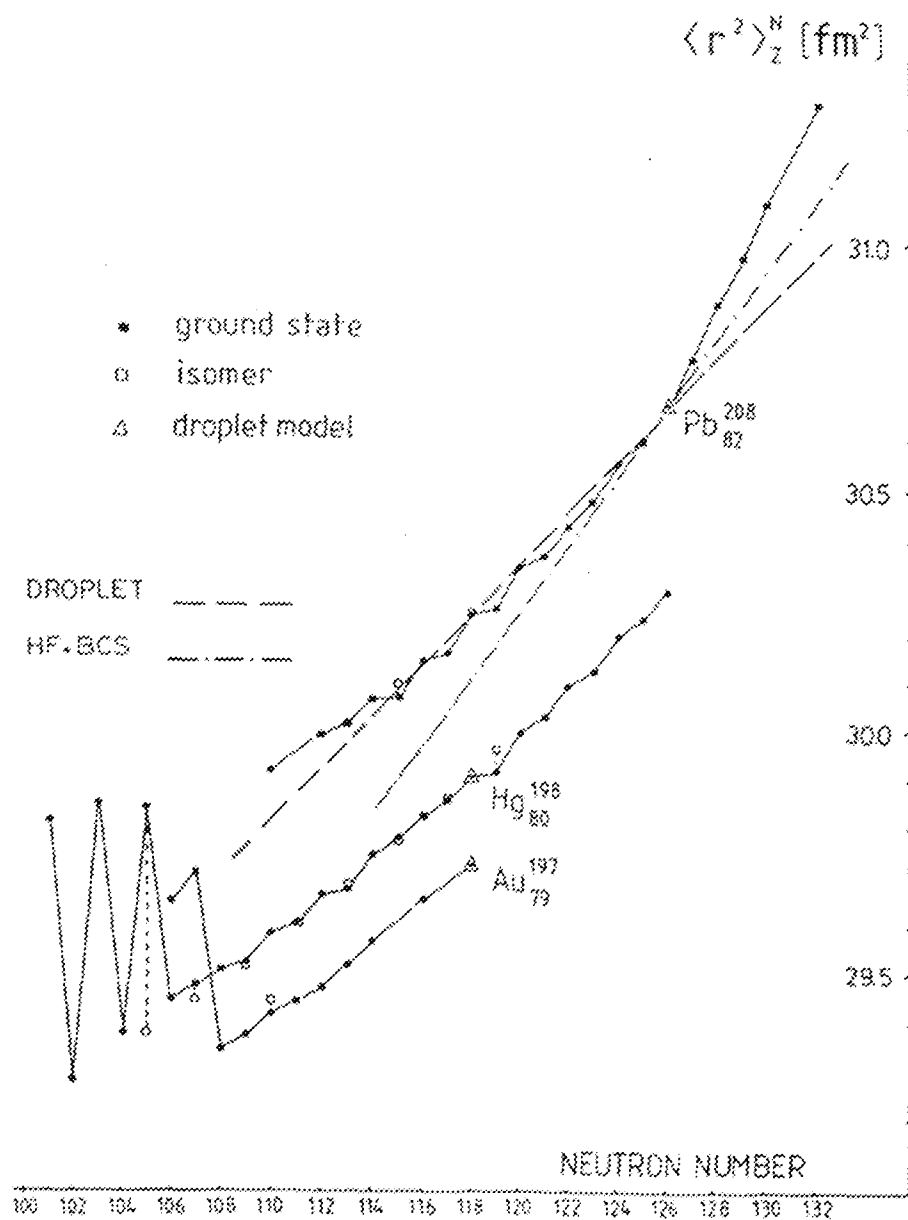


Fig.13

The mean value of the nuclear charge radius in the isotopic sequences of Au, Hg and Pb. The absolute scale (the experiments determine the differences in $\langle r^2 \rangle$) has been fixed from the droplet model. From [ott89a].

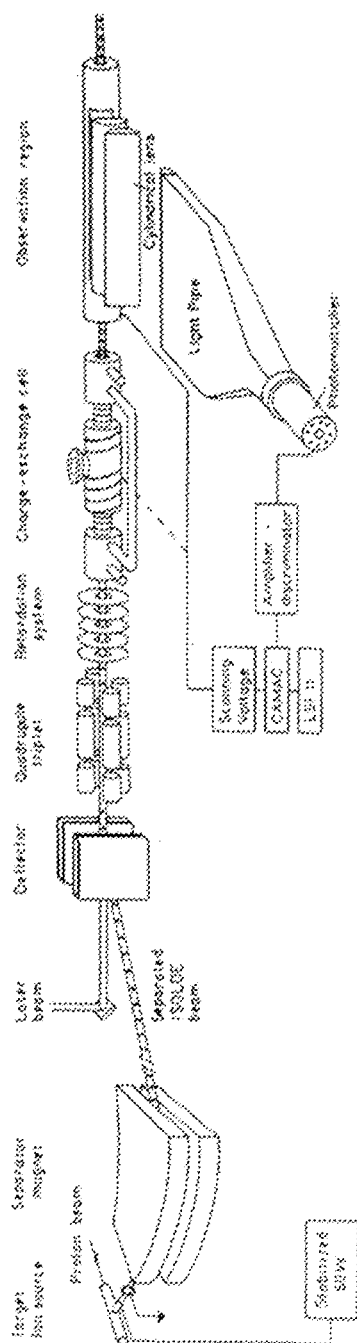


Fig.14

The principle of collinear-laser spectroscopy with fluorescence detection. The ion and laser beams are merged at the deflector and the ions neutralized in collisions with caesium vapour in the charge-exchange cell. The resonantly scattered light is detected with the photomultiplier. From [ou89a].

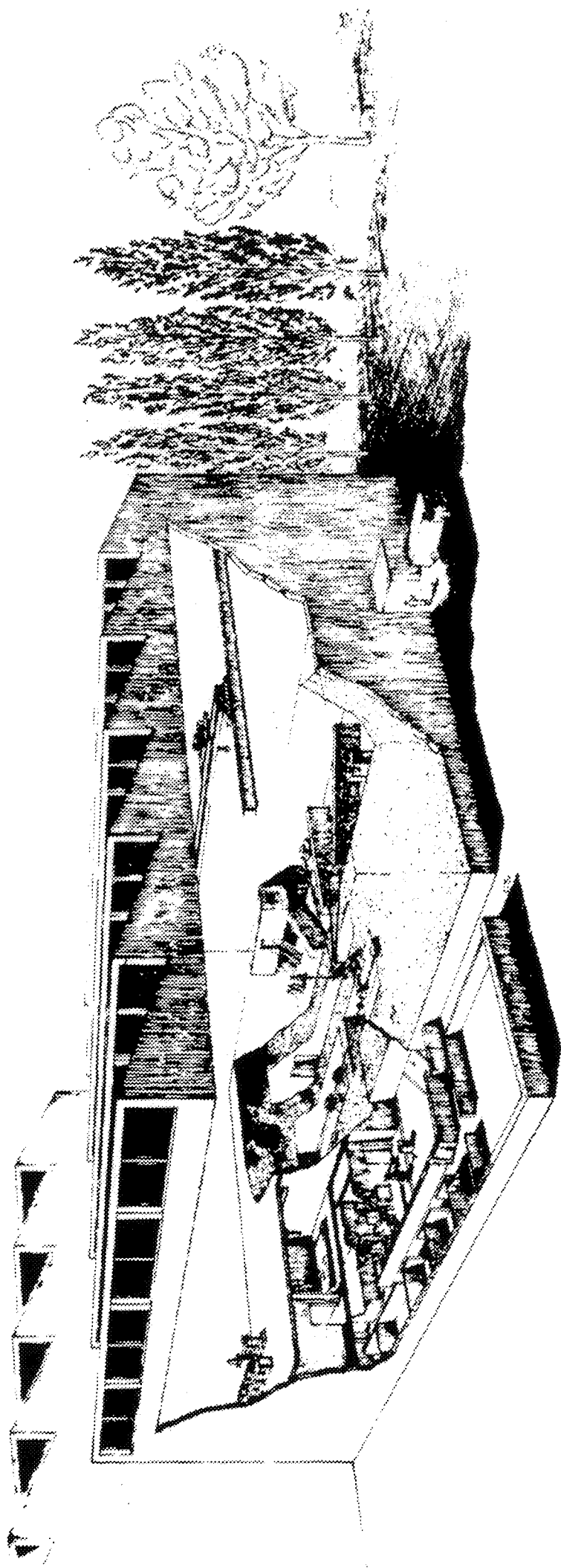


Fig.15

View of the SIN-ISOLDE Building, planned as a side wing to the accelerator hall at SIN (Villigen, CH). The hot bay for handling of very large radioactivities (left) has separate access and is completely separated by large amounts of shielding from the secondary-beam area and electronics huts (right). See [dom81a].

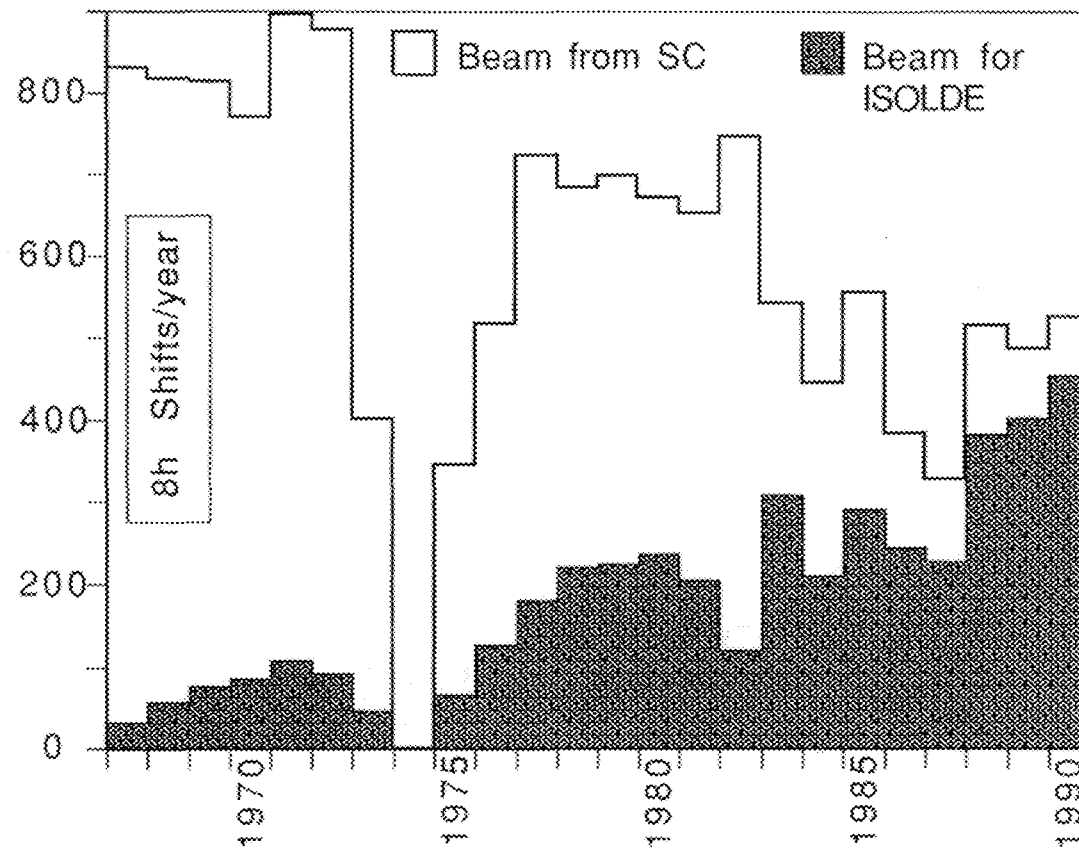


Fig.16

Operating statistics for the SC from 1967 to the final shut-down on 17 December 1990 (Courtesy H. Haas, CERN). The operating time for ISOLDE was during the later years limited by the capacity of the operating group rather than by machine time available. This was the main motivation behind the wish for a second, independent area and "push-pull" operation, see Sect. 7.1

STUDIES IN CERN HISTORY

This report is intended for inclusion, in a modified form, in Volume III of the History of CERN. Volume I was published in 1987, and covered the launching of the European Organization for Nuclear Research. Volume II, concerned with building the laboratory and running it until 1965, was published in 1990. They are available from

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