

Stockholm revisited

Stockholm 1984 — Physics Nobel prizewinners Carlo Rubbia (left) and Simon van der Meer.

(Photo Dagens Nyheter)



The Nobel Physics Prize for 1984 was shared by Carlo Rubbia and Simon van der Meer of CERN for their decisive contributions to the project which led to the discovery of the W and Z field particles which carry the weak interaction. Five years before, Sheldon Glashow, Abdus Salam and Steven Weinberg had made the same trip to receive the prize for their crucial roles in piecing together the ele-

gant 'electroweak' gauge theory which unified weak interactions with the electromagnetic force and predicted just where the W and Z particles could be found.

In December, the three theorists returned to Stockholm to participate with the 1984 laureates in a specially arranged panel discussion for Stockholm students. Introducing the proceedings, Bengt Nagel of Stockholm's Royal Institute of

Technology played an extract from a recording made in 1979 which vividly demonstrated just how unexpected it was at the time that the predicted particles would be found so soon. Nagel paid tribute to the achievements of the CERN Collider.

Seldom have theoretical predictions and experimental discoveries followed each other so closely in the Nobel annals, and Weinberg, speaking first, thanked the Nobel Physics Prize Committee for 'the marvellous idea of inviting back the class of 1979 so that they could bask in the reflected glory of Rubbia and van der Meer'.

'By the mid-nineteen seventies we appeared to have a good understanding of the fundamentals of the particles which can be created in the laboratory,' continued Weinberg. 'However this understanding, as usual in physics, was incomplete. There were, and still are, a large number of loose ends — undetermined parameters whose values have to be taken from experiment — masses, scales, couplings, mixings, etc. In addition there was an obvious lack of total unification between the different sectors — electroweak and strong interactions, gravity still left out, quarks and leptons treated separately.'

There has been a tremendous effort by theorists throughout the world which has proved almost entirely frustrating. While our experimental colleagues were going ahead and dazzling us with new discoveries, we theorists have been exploring one new idea after another — ideas of remarkable beauty and subtlety — without the slightest success to show for our pains.

For instance there was the effort (in which my colleagues Sheldon

Steven Weinberg: 'a tremendous effort by theorists has proved almost entirely frustrating'.

(Photo CERN 356.12.79)



Glashow and Abdus Salam participated very actively) to make a grand unified theory to bring together the strong and electroweak interactions along the same lines as the electroweak unification. These grand unified theories led to a general expectation of an unstable proton with a lifetime just barely accessible to experiment. But so far, to our great sorrow, this has not been seen.

Many alternative models have been proposed, but none so far have crystallized as candidates for the 'true' theory. Despite all this theoretical activity, we have virtually nothing to show in the way of concrete predictions to compare with experiments.

Despite the fact that many of these new ideas look forward to a grand synthesis at extremely high energies, I think it is still possible that new things will turn up

in experiments using current technology, and there are even signs of this happening.

The theorists may have their direction set for them by the experimentalists. But it is also possible that new theoretical ideas will be so successful that they will be able to explain all the missing numbers.'

In reply to a question by discussion co-moderator Celia Jarlskog on the new drive to link basic physics ideas with cosmology, Weinberg replied: 'Despite the great success in formulating qualitative solutions to cosmological problems, there has not been one quantitative prediction to rival, for example, the 1960s prediction of the helium abundance which gives any confidence that these cosmological scenarios have any reality.

I think the work is fascinating (I participated in some of it myself) but I don't see any hope that we

will be able to confirm any of these ideas. Unfortunately they do not lead to precise quantitative predictions because long after its various initial phases, the Universe passed through a long period of statistical equilibrium in which all the interesting phenomena of the earlier epochs got washed out. In the 'grand cooking' of the Universe, we are now left with the final cosmic 'stew' without any idea of the individual ingredients that went into it.'

Simon van der Meer of CERN began by spelling out to the largely student audience some 'facts of life' about present-day particle accelerators. Larger and larger such machines are needed to probe smaller and smaller constituents of matter.

'Current thinking has concluded that it is no longer useful to use fixed target accelerators with most of the energy lost in hitting the stationary target particles. Instead the fashion is now colliders, with all the energy being effectively used. With colliders, people prefer to use electrons and positrons rather than protons. Proton and antiproton collisions are a mess, as the particles are full of quarks and gluons. The collision energy has to be shared between the constituent particles, and is effectively reduced.

On the other hand, protons are easier to handle in a circular machine, as light particles like electrons lose energy by radiation as they move in a circle. At CERN, we had the possibility of using protons and antiprotons in the existing SPS ring. Handling contra-rotating beams in a single ring this way is relatively cheap — much cheaper than building a new ring. However when we think about plans for higher energies in bigger

Simon van der Meer — 'our current dilemma'.

(Photo CERN 715.1.84)

rings, are these antiproton methods still useful?

We have done it, and it works. But only just, and the process is difficult and painful. We have to collect antiprotons all day long, and if the slightest thing happens, we lose them and have to start all over again. If we build a new machine, I would think it would be better to build two rings and handle protons. It's easier and more reliable, and the price difference is not all that big.

At CERN, we are now building the LEP machine, eventually to collide 100 GeV electrons with positrons. In the US, the new superconducting ring at Fermilab accelerates to 800 GeV, and 1000 GeV (1 TeV) is planned. A study is underway for a 20 TeV machine, and if the Americans build this, then we in Europe are in trouble. One plan currently being discussed is to use the LEP tunnel for a proton ring, which because of its limited size can at most provide about half the energy of the new US proposal. However it will be much cheaper, and if the US community doesn't get the money to build the new machine, we in Europe will be in a good position. That's our current dilemma.'

Abdus Salam began by acknowledging the contributions of the 1984 laureates and all the other people at CERN responsible for creating the proton-antiproton Collider. 'Van der Meer has talked of plans for the immediate future,' he continued, 'but I would like to look further ahead, in fact to the year 2006 — my eightieth birthday!'

Theorists look at an energy of 10^{19} GeV — the so-called 'Planck energy' given by the Einstein equations. How are we to get at this energy? Present accelerators are limited by accelerating fields of



about a tenth of a GeV per metre. On this basis we can propose machines of 10^4 GeV, but remember this figure of 10^{19} GeV. Beat-wave laser-plasma accelerators are on the horizon and could be a thousand times more powerful. Even so, 10^{19} GeV is still far away. But I am sure that by the year 2006 there will be newer ideas to exploit.'

Then Salam turned to the thorny problem of funding. 'We have to put a price on our curiosity and our search for basic knowledge. The usual amount suggested is one-tenth of two per cent of the gross national product — two per cent is the usual overall level of expenditure on science and research and development. As I come from a developing country (Pakistan), people ask me how I can justify vast expenditures on accelerators. My answer is always

that in our technological society, the 'big sciences' are the big spenders and will take the place of nuclear armaments the day these stop being made. If our technological society wants to go forward, we will be its benefactors. The required level of funding is absolutely nothing compared to the current investment in nuclear submarines, but at the same time the support for science is being eroded in some countries. I shudder to think what this means for the future.'

Finally Salam took up his long-cherished dream of unifying all the forces of Nature. 'We do not seem to have been following the right path. The proton decay which was supposed to show the unification of the strong interaction does not seem to be materializing. However there is the possibility of an incredibly beautiful symmetry — the

Abdus Salam, seen here at CERN in 1979 with Leon Van Hove (standing, left) and John Adams (right), CERN's Directors General at the time. At Stockholm last year, Salam paid tribute to their roles in making the CERN proton-antiproton Collider possible.

(Photo CERN 361.10.79)



supersymmetry between bosons and fermions. There is no firm evidence for this yet, and if this symmetry really exists we will only know when we theoreticians can 'gauge' it, and that means the theory of supergravity. This means that the interactions get mixed up, they don't show themselves neatly and individually.

Going further, to extended supergravity, recent work suggested that the best dimension for the unification was eleven, and this eleven-dimensional theory was so beautiful that Stephen Hawking called it the Theory of Everything — TOE. But it didn't work and we had to backtrack.

So we are in the sad situation where we have many competing theories, but little to show for it. The moral is that physics will always be an experimental science, and we shall have to keep coming

back to van der Meer and Rubbia and the accelerators.'

This was underlined by Carlo Rubbia. 'It seems clear that present day physics is back in the hands of the experimentalists. We are all waiting for a big surprise, we do not know from where.

As mentioned by Simon van der Meer, colliding beams — hadrons as well as electrons and positrons — are taking over from fixed targets. This very important revolution changes the picture of accelerators very much from when it was started fifty years ago by Lawrence.

In parallel, there is a big development in non-accelerator science. Not that long ago, it seemed that the only way to become famous was to have your own accelerator. Now there are ways of becoming famous without one.

There are two classes of such

experiments — one looking for very small masses, neutrino mass or oscillations, and at the other end of the scale the proton decay experiments looking for effects of very big masses. The search for proton decay is still going on and there are great chances of finding it, if it is there, in the next few years.

In the accelerator field, we do not really know where new surprises are going to turn up. To me it is not obvious that we have to build new machines a hundred times more powerful than present colliders. New physics could be closer than we think. Already some glimpses of evidence at the CERN Collider suggest new things which are not completely understood (see May 1984 issue, page 139).

Remember the case of the J/psi. At the time the CERN Intersecting Storage Rings were operating at

Carlo Rubbia: 'new physics could be closer than we think'.

(Photo CERN 63.8.83)

about 60 GeV and the new particle had an energy of only 3 GeV. So it was not just a question of machine energy. It was discovered by looking more carefully at what had gone before.

The discovery of the W and Z particles brings in new mass scales, just as the discovery of the pion did. But when the pion was discovered, nobody suspected the wealth of new resonance states that would turn up. Likewise, we have no idea what the Ws can do when they get close to each other. What is the interaction of Ws like? There are lots of theories with lots of answers, but as an experimentalist I would like to see what any new spectroscopy looks like. This is an open experimental question and the only way to solve such questions is by more careful and more experiments.'

The final speaker, Sheldon Gla-



show, paid a tribute to the CERN Collider. 'Few of us believed that this machine would work so well and would find not just the W and Z but the top quark as well. It is a miracle.'

'There have been attempts to find surprises outside the high energy domain,' he continued. 'Free quarks, magnetic monopoles, neutrino masses and proton decay, and for a brief period I thought this was the direction to go. But it seems not to be the case. The surprises are coming from high energy physics and despite their modesty Carlo Rubbia and Simon van der Meer are sitting on a bomb.'

Glashow then adopted a more philosophical viewpoint. 'There

Sheldon Glashow: upwards and downwards paths.

(Photo CERN 327.12.79)

L3 — experiment for LEP

are two very distinct approaches to the mysteries of elementary particle physics. I call them the upwards path and the downwards path. Abdus Salam and Steven Weinberg, for example, follow the latter — starting from some brilliant idea, like the Einstein approach, one attempts to go from a theory of everything to the mundane silly little effects seen at accelerators. This downwards path is a difficult one which began with Einstein's brilliant discovery of general relativity in 1917 and has continued downhill ever since!

The upwards path is a dirtier business. We listen to our experimental colleagues and try to glean little bits and pieces that do not fit into our standard and arrogant picture of the universe.

As my theoretical colleagues have already said, the downwards path is not getting very far. But the upwards path is not getting very far either!

Simon van der Meer spoke about international competition in high energy physics. I don't think we should regard this as competition any more. It was when it was cheap, but today it has become very much more expensive. I hope that what has been an active competition in the past will evolve into a fruitful collaboration. The success of CERN — a collaboration of 13 nations — will be usefully expanded. Let me say today, to be rebroadcast in five years, that this Nobel Prize, awarded to these distinguished and very deserving Europeans, will mark the beginning of a high energy physics collaboration with the other countries of the world.'

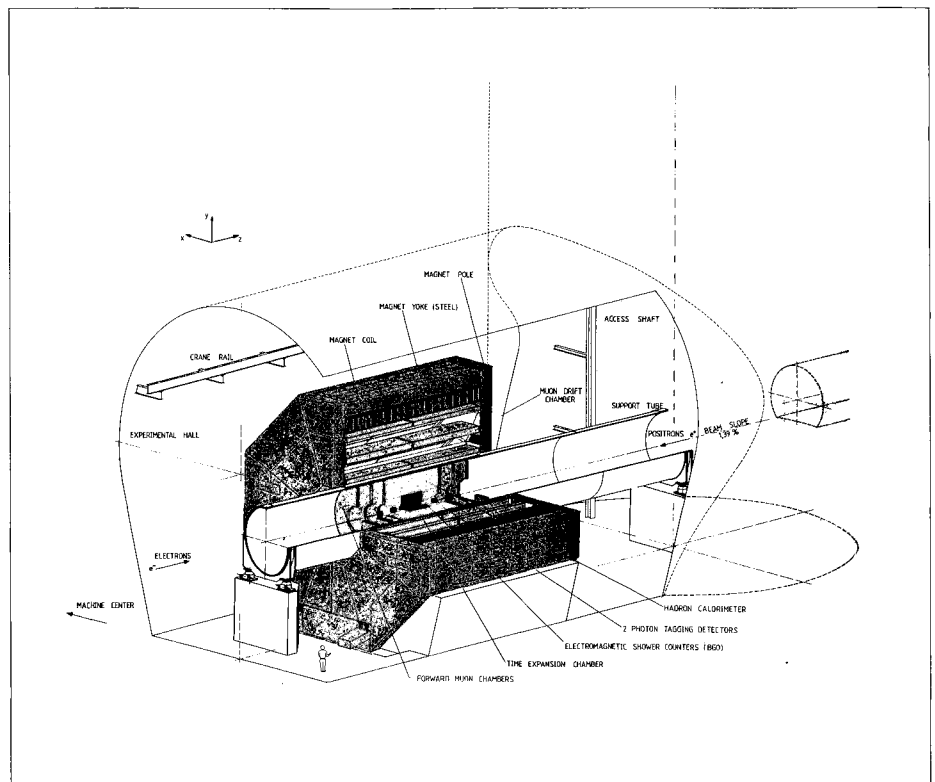
To conclude our series of articles on the four big experiments for the giant LEP electron-positron now being built at CERN, we turn to the L3 study. (The other three experiments are DELPHI — see July/August 1984 issue, page 227, ALEPH — see September 1984 issue, page 269, and OPAL — see November 1984 issue, page 375.)

While the other three experiments have concocted fancy acronyms, the title L3 simply stems from the fact it was the third letter of intent to be received back in 1982 when ideas for LEP experiments were first tabled. However the modest label is no reflection on the experiment's ambitions, and L3 head Sam Ting is still looking for a suitable name for his experiment.

Our understanding of the fundamental forces of Nature leans on

two basic pillars, gauge invariance and the mechanism of spontaneous symmetry breaking. Gauge invariance, which provides the field theories to describe electromagnetism, the electroweak unification and inter-quark forces, is now well understood.

However spontaneous symmetry breaking, which provides for example the mass scales of the electroweak theory, is on less sure ground. Underlying this mechanism are new particles, yet to reveal themselves — the 'Higgs' bosons. There are no clear-cut predictions of where these objects will be found (unlike the very precise predictions for the W and Z gauge particles). To uncover signs of these particles at LEP will require precise measurements of relatively rare processes. L3 has been designed with this firmly in mind. An additional aim is to carry out pre-



Overall layout of the L3 experiment for the LEP electron-positron collider now under construction at CERN.