Prestigious Discoveries at CERN

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Prestigious Discoveries at CERN

1973 Neutral Currents
1983 W & Z Bosons

With 114 Figures, Including 88 in Color

Springer

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Cover picture One of the first Zo events observed by UA1 in 1983 in the electron-positron decay channel.
CERN Photo reference: CERN-EX-8704168

ISBN 978-3-642-05855-4 ISBN 978-3-662-12779-7 (eBook)
DOI 10.1007/978-3-662-12779-7

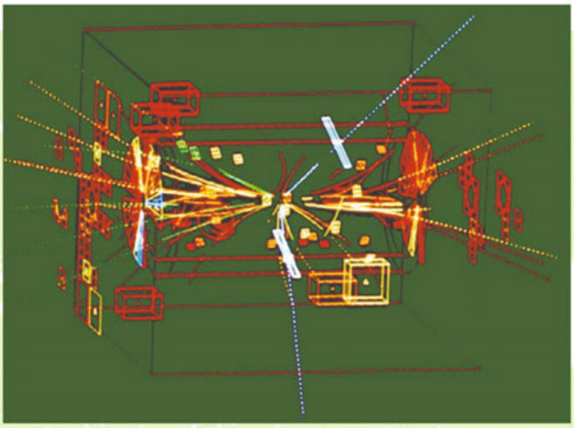
Library of Congress Control Number: 2004107595

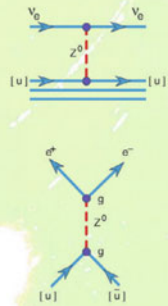
First published in Eur. Phys. J. C, Vol. 34/1 (2004)

springeronline.com

Typesetting: Steingraeber, Dossenheim
Cover design: Erich Kirchner, Heidelberg

Printed on acid-free paper 55/3141/ba 5 4 3 2 1 0





1973: Neutral Currents
1983: $W^{\pm}$ & Z^{0} Bosons

The anniversary of CERN's discoveries and a look into the future

Tuesday, September 16, 2003 at 9 am
Main Auditorium

- **Welcome**: Luciano Maiani
- **The making of the Standard Model**: Steven Weinberg
- **CERN's contribution to accelerators and beams**: Giorgio Brianti
- **The discovery of neutral currents**: Dieter Haidt
- **The discovery of the W & Z, a personal recollection**: Pierre Darriulat
- **W & Z Physics at LEP**: Peter Zerwas
- **Physics at the LHC**: John Ellis
- ***Challenges of the LHC***:
  - **The accelerator challenge of the LHC**: Lyn Evans
  - **The detector challenge of the LHC**: Jos Engelen
  - **The computing challenge of the LHC**: Paul Messina
- **Particle detectors and society**: Georges Charpak
- Closing of the Symposium: **The future for CERN**: Luciano Maiani

Panel discussion: Future of Particle Physics
With the participation of: Robert Aymar, Georges Charpak, Pierre Darriulat, Luciano Maiani, Simon van der Meer, Lev B. Okun, Donald Perkins, Carlo Rubbia, Martinus Veltman, Steven Weinberg

Organizers: Roger Cashmore and Jean-Pierre Revol

www.cern.ch/cerndiscoveries

Foreword

The discoveries of neutral currents and of the W and Z bosons marked a watershed in the fortunes of CERN. Over the 20 years that CERN had existed before the neutral current discovery in 1973, the experimental groups here had carried out some remarkable and beautiful experiments – for example, the precision measurements of the (g-2) value of the muon. The stage was set for the great discoveries to come from 1973 onwards, that would transform our views of the fundamental particles and their interactions and establish CERN as a leading high-energy laboratory.

The significance of these discoveries is hard to overestimate. Not only did they put CERN truly "on the map" and establish the validity of the electroweak theory, but for the first time convinced everyone of the importance of renormalisable non-Abelian gauge theories of the fundamental interactions. This in turn paved the way for the precision experiments to be carried out at the LEP collider, which would establish the Standard Model and test its predictions in exhaustive detail. After this interval of ti me, it is important not only to celebrate these discoveries, but also to view them from today's perspective. Indeed, as we see them now, the implications of these discoveries are even more profound than we thought at the time they were made.

We are fortunate that some of the main contributors to the making of the Standard Model are with us today, so that we can hear from them directly how such achievements were made possible, and perhaps extract some wisdom relevant to the future of our field.

The discoveries in particle physics which we are recalling today were testimony not only to the greatly increasing experience and know-how of the experimental groups using CERN, but also to the long-established technical excellence of the CERN laboratory. The neutral current discovery depended not only on the existence of the large heavy liquid bubble chamber Gargamelle, developed and built at Saclay, but also on the greatly improved neutrino beam intensity, provided by the fast-cycling PS booster and the new two-component magnetic horn. And the discovery of the W and Z bosons was only possible as a result of the technical tour de force of the proton–antiproton collider staff, based on an idea of Simon van der Meer and the experience gained in the previous programme of beam cooling at the ISR, combined with the determination and enthusiasm of Carlo Rubbia who made the proposal.

The idea of the hermetic detector pioneered by UA1 at the collider was taken up in the LEP detectors and became a driving concept in the design of the LHC detectors, while the UA1 collaboration was the "prototype" of the now well accepted very large international scientific collaborations. Towards the end of this meeting, we shall look forward to the future programme of CERN. We are able to do so with the confidence engendered by our discoveries of long ago.

Luciano Maiani
(CERN Director General)

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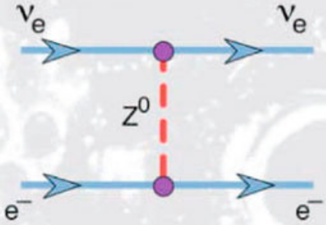
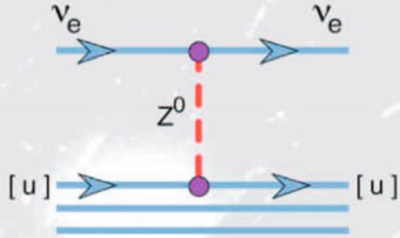
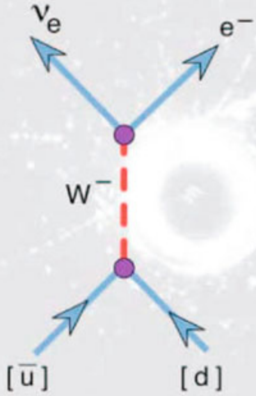




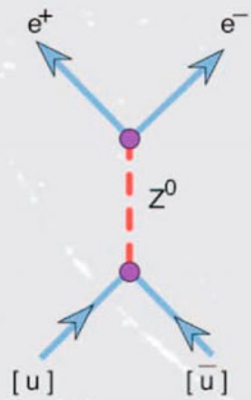
1973: Neutral Currents
1983: $W^{\pm}$ & Z^0 Bosons

The anniversary of CERN's discoveries and a look into the future

Tuesday 16th of September 2003



CERN
Main Auditorium



Symposium Programme
Morning sessions

9:00	***Welcome***	***L. Maiani***

First session chaired by Professor Antonino Pullia

9:10	The making of the Standard Model	***S. Weinberg***
9:55	CERN's contribution to accelerators and beams	***G. Brianti***
10:25	The discovery of neutral currents	***D. Haidt***
10:55	***Coffee break***	

Second session chaired by Professor Samuel C. C. Ting

11:30	The discovery of the W & Z, a personal recollection	***P. Darriulat***
12:10	W & Z Physics at LEP	***P. Zerwas***
12:40	***Lunch***	

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Symposium Programme
Afternoon sessions

Third session chaired by Professor Luciano Maiani

14:00	Physics at the LHC	***J. Ellis***
14:30	Challenges of the LHC:	
	– The accelerator challenge of the LHC	***L. Evans***
	– The detector challenge of the LHC	***J. Engelen***
	– The computing challenge of the LHC	***P. Messina***

Fourth session chaired by Professor Sheldon Glashow

15:30	Particle detectors and Society	***G. Charpak***
16:00	***Coffee break***	
16:30	The future for CERN	***L. Maiani***

Panel discussion chaired by Professor Carlo Rubbia

17:00 Future of Particle Physics, with:
Robert Aymar, Georges Charpak, Pierre Darriulat, Luciano Maiani, Simon van der Meer, Lev Okun, Donald Perkins, Carlo Rubbia, Martinus Veltman, and Steven Weinberg

*There will be two contributions, from **Fabiola Gianotti** [experimentalist] and **Ignatios Antoniadis** [theorist], introducing the views of young physicists*

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General Outline of Programme including social events

9:00–12:40 ***Symposium in CERN Main Auditorium***
Retransmission in AB Auditorium II (bldg 864) in Prevessin, IT Auditorium (bldg 31), AT Auditorium (bldg 30).

12:40–14:00 ***Lunch***
Speakers and panel members are invited to a lunch at the CERN Restaurant #1 (Glass Box, in main building).

14:00–17:45 ***Symposium continuation in CERN Main Auditorium***
Retransmission in AB Auditorium II (bldg 864) in Prevessin, IT Auditorium (bldg 31), AT Auditorium (bldg 30).

18:00–19:30 ***Drink***
Drink to which authors of W & Z discovery papers and the neutral current discovery papers, together with the accelerator staff and other CERN staff involved in the proton-antiproton programme are invited. The drink will take place in the Salle des Pas Perdus, on the first floor of the main building.

19:30 ***Dinner***
Speakers, panel members and a number of important scientific personalities are invited to a banquet hosted by Luciano Maiani, which will be concluded by an after dinner speech by CERN's next Director general, Robert Aymar. The dinner will take place at the CERN Restaurant #1 (Glass Box, in main building).

Please note that the proceedings will be published, and that speakers are kindly requested to provide a written version of their talk by 15th October.

Please note also that there is a possibility for speakers to visit LHC (detectors and machine) either on Monday 15th or Wednesday 17th of September. Please contact Jean-Pierre Revol to organize your visit.

Selected photographs of the event

Photograph taken in the CERN main auditorium. Front row, from left to right: D. H. Perkins, S. Weinberg, L. Maiani, M. Veltman, D. Haidt; second row, from left to right: R. Aymar, P. Messina, G. Brianti, L. Evans, J. Engelen; Third row, from left to right: I. Antoniadis, J. Ellis, F. Gianotti, P. Zerwas, P. Darriulat; fourth row: A. Pullia

Photograph taken in the CERN main auditorium. From left to right: C. Rubbia, G. Charpak, S. C. C. Ting

Photograph taken at CERN, in the Salle des Pas Perdus. From left to right: R. Cashmore, P. Messina, C. Rubbia, G. Brianti, L. Evans, L. B. Okun, F. Gianotti, J. Ellis, S. Weinberg, S. Glashow, L. Maiani, S. van der Meer, M. Veltman, P. Zerwas, P. Darriulat, A. Pullia, J. Engelen, R. Aymar, D. Haidt

Photograph taken during lunch at the CERN Glass Box. From left to right: P. Zerwas (partially hidden), G. Bellettini, S. Glashow, S. Weinberg, V. Telegdi

Photograph taken in the main auditorium: S. Weinberg and L. Maiani in conversation during a break

Some of the participants in the panel discussion on the Future of Particle Physics, from left to right: R. Aymar, L. B. Okun, L. Maiani, C. Rubbia (Chair), S. Weinberg

From left to right: Pierre Darriulat, Roger Cashmore and John Ellis enjoying the lunch break

Jean-Pierre Revol and Georges Charpak during the lunch break

CERN press release

September 2, 2003
PR12.03

CERN celebrates discoveries and looks to the future

Nobel laureates will be among the distinguished guests at a symposium at CERN on 16 September. The symposium will celebrate the double anniversary of major discoveries at CERN that underlie the modern theory of particles and forces. It will also look forward to future challenges and opportunities as the laboratory moves into a new arena for discovery with the construction of the Large Hadron Collider. The symposium will end with a panel discussion(*) on the future of particle physics, chaired by Carlo Rubbia.

Twenty years ago, in 1983, CERN announced the discovery of particles known as W and Z, a discovery that brought the laboratory its first Nobel Prize in 1984. Ten years previously, physicists at CERN had already found indirect evidence for the existence of the Z particle in the so-called "neutral currents". The charged W and neutral Z particles carry one of Nature's fundamental forces, the weak force, which causes one form of radioactivity and enables stars to shine. These discoveries provided convincing evidence for the so-called electroweak theory, which unifies the weak force with the electromagnetic force, and which is a cornerstone of the modern Standard Model of particles and forces.

This brought modern physics closer to one of its main goals: to understand Nature's particles and forces in a single theoretical framework. James Clerk Maxwell took the first steps along this path in the 1860s, when he realised that electricity and magnetism were manifestations of the same phenomenon. It would be another hundred years before theorists succeeded in taking the next step, unifying Maxwell's electromagnetism with the weak force in a new electroweak theory.

An important step towards confirming electroweak unification came in 1973, when the late André Lagarrigue and colleagues working with the Gargamelle bubble chamber at CERN observed neutral currents – the neutral manifestation of the weak force that had been predicted by electroweak theory but never previously observed. Later that decade, Carlo Rubbia of CERN proposed turning the laboratory's most powerful particle accelerator into a particle collider, an idea that received the support of the then Directors General, John Adams and Léon Van Hove. By colliding counter-rotating proton and antiproton beams head on, enough energy would be concentrated to produce W and Z particles. This was made possible, in particular, through Simon van der Meer's invention of "stochastic cooling" to produce sufficiently dense antiproton beams. By 1981 the search for the W and Z particles was on. The observation of W particles by the UA1 and UA2 experiments was announced at CERN on 20 and 21 January 1983. The first observation of Z particles by UA1 followed soon after, with the announcement on 27 May.

In 1979, three of the theorists responsible for the electroweak theory, Sheldon Glashow, Abdus Salam and Steven Weinberg, were awarded the Nobel Prize. In 1984, Carlo Rubbia and Simon van der Meer shared the Prize for their part in the discovery of the W and Z particles. The discovery also owes much to the development of detector techniques, in particular by Georges Charpak at CERN, who was rewarded with the Nobel Prize in 1992. The results ushered in more than a decade of precision measurements at the Large Electron Positron collider, which tested the predictions of the Standard Model that could be calculated due to the work of theorists Gerard 't Hooft and Martinus Veltman, who shared the Nobel Prize in 1999.

In addition to reflecting on past findings, speakers at the September symposium will also talk about the future of CERN, including the Large Hadron Collider, set to switch on in 2007. By colliding particles at extremely high energies, the LHC should shed light on such questions as: Why do particles have mass? What is the nature of the dark matter in the Universe? Why did matter triumph over antimatter in the first moments of the Universe, making our existence possible? What was the state of matter a few microsceconds after the Big Bang?

The symposium will be open to the public, and will run from 9 a.m. to approximately 6 p.m.

(*) The members of the a panel will be: CERN's Director General Luciano Maiani, together with Robert Aymar (Director General of CERN from 1 January 2004), Georges Charpak, Pierre Darriulat, Simon van der Meer, Lev Okun, Donald Perkins, Carlo Rubbia, Martinus Veltman, and Steven Weinberg.

Communiqué de presse du CERN

Le 3 septembre, 2003

Le CERN célèbre ses découvertes et se tourne vers l'avenir

Le symposium qui se tiendra au CERN, le 16 septembre prochain, comptera des lauréats du prix Nobel parmi ses prestigieux invités. Ce symposium célèbrera l'anniversaire de deux grandes découvertes réalisées au CERN sur lesquelles se fonde la théorie moderne des particules et des forces. Ce sera aussi l'occasion d'envisager les défis que le Laboratoire devra relever et les opportunités qu'il pourra saisir avec son prochain accélérateur de particules. Le Grand collisionneur de hadrons (LHC) promet en effet d'ouvrir une nouvelle ère de découvertes. Le symposium s'achèvera par une table ronde (*) sur l'avenir de la physique des particules, présidée par Carlo Rubbia.

Il y a vingt ans, le CERN annonçait la découverte des particules appelées W et Z, qui lui valut son premier prix Nobel en 1984. Dix ans auparavant, des physiciens du Laboratoire avaient déjà trouvé une preuve indirecte de l'existence des particules Z dans les "courants neutres". Les particules W (chargé) et Z (neutre), portent l'une des forces fondamentales de la Nature: la force faible, qui est à l'origine d'une forme de radioactivité et permet aux étoiles de briller. Ces observations ont solidement étayé la théorie dite électrofaible, qui unifie les forces faible et électromagnétique et constitue l'un des fondements du Modèle Standard moderne des particules et des forces.

La physique moderne a ainsi pu s'approcher de l'un de ses principaux buts: comprendre, dans un seul et même cadre théorique, les particules et les forces qui existent dans la Nature. James Clerk Maxwell fut le premier à s'engager sur cette voie dans les années 1860, lorsqu'il se rendit compte que l'électricité et le magnétisme étaient des manifestations du même phénomène. Il devait encore s'écouler une centaine d'années avant que des théoriciens ne parviennent à franchir l'étape suivante, en réunissant l'électromagnétisme de Maxwell et la force faible dans une nouvelle théorie électrofaible.

Une avancée importante vers la confirmation de l'unifi-cation électrofaible fut réalisée en 1973, lorsque feu André Lagarrigue et ses collègues qui travaillaient au CERN sur la chambre à bulles Gargamelle détectèrent des courants neutres, la manifestation neutre de la force faible, qui avait été prédite par la théorie électrofaible, mais n'avait jamais été observée auparavant. Dans cette même décennie, Carlo Rubbia, du CERN, proposa de transformer le plus puissant accélérateur de particules du Laboratoire en collisionneur de particules. Son idée, qui reçut l'appui des Directeurs généraux de l'époque, John Adams et Léon Van Hove, était que des collisions frontales entre des faisceaux de protons et d'antiprotons tournant en sens opposés permettraient de concentrer suffisamment d'énergie pour produire des particules W et Z. Cette expérience fut en particulier rendue possible grâce au "refroidissement stochastique", une invention de Simon van der Meer visant à produire des faisceaux d'antiprotons de densité suffisante, et la quête des particules W et Z commença en 1981. L'observation de particules W par les expériences UA1 et UA2 fut annoncée au CERN respectivement les 20 et 21 janvier 1983, suivie peu après par la première observation de particules Z par UA1, annoncée le 27 mai.

En 1979, trois des pères de la théorie électrofaible reçu-rent le prix Nobel: Sheldon Glashow, Abdus Salam et Steven Weinberg. En 1984, le prix Nobel fut attribué à Carlo Rubbia et Simon van der Meer pour leur contribution à la découverte des particules W et Z, qui fut aussi rendue possible dans une large mesure par le développement des techniques de détection, en particulier grâce aux travaux menés au CERN par Georges Charpak, lauréat du prix Nobel 1992. Les résultats obtenus marquèrent le début de plus d'une décennie de mesures de précision auprès du Grand collisionneur électron-positon, mesures qui testèrent le modèle standard et que l'on avait pu confronter aux calculs grâce aux travaux des théoriciens Gerard 't Hooft et Martinus Veltman, tous deux lauréats du prix Nobel 1999.

Les intervenants du symposium de septembre évoque-ront les découvertes passées, mais ils parleront également de l'avenir du CERN, et notamment du grand collisionneur de hadrons, dont la mise en service est prévue pour 2007. En provoquant des collisions de particules à des énergies extrêmement élevées, le LHC devrait faire la lumière sur les questions suivantes: pourquoi les particules ont-elles une masse ? Quelle est la nature de la matière noire présente dans l'Univers ? Pourquoi la matière a-t-elle pris le dessus sur l'antimatière dans les premiers instants de l'Univers, rendant ainsi notre existence possible? Quel était l'état de la matière quelques microsecondes après le Big Bang?

La conférence sera ouverte au public et se déroulera entre 9 h et 18 h environ.

(*) La table ronde réunira le Directeur général du CERN Luciano Maiani ainsi que Robert Aymar (Directeur général du CERN à compter du 1er janvier 2004), Georges Charpak, Pierre Darriulat, Simon van der Meer, Lev Okun, Donald Perkins, Carlo Rubbia, Martinus Veltman et Steven Weinberg.

First published in Eur. Phys. J. C 34, 1–2 (2004)
Digital Object Identifier (DOI) 10.1140/epjc/s2004-01847-8

Luciano Maiani

Welcome

Welcome everybody,

We are very glad to have so many people here today and see so much interest in this Symposium which intends to recall two important discoveries of the past: neutral currents in 1973 and intermediate vector bosons in 1983.

There is of course regret for the people who are no longer with us. Above all, I want to mention André Lagarrigue who was really the driving force behind Gargamelle and Paul Musset, one of the main protagonists in the neutral current discovery.

Coming back to memories, I want to just recall the wonderful workshop in Rome, back in January 1983 (see Fig. 1), when UA1 first announced that, well, perhaps... they were seeing W bosons. I have a very good memory of this workshop because, in fact, I wasn't there: I was supposed to give a talk but I was confined to my bed with a bad backache and I was furiously listening to the telephone about the news. UA1 were very cautious in the Workshop, but more explicit in the proceedings. In any case, that was really the time when everybody understood that they had made it.

The success of particle physics is built on future challenges more than on past glories: that's why we decided to dedicate the afternoon of the Symposium to the LHC. As you have seen in the programme, there will be a discussion of various aspects of this very exciting project which will keep us occupied for the next 20 years, I guess. Also, we wanted to take advantage of the presence of so many distinguished colleagues, many former members of the Scientific Policy Committee which met yesterday, and we have organized a panel on the future of CERN and of Particle Physics. I hope this will be a good occasion to discuss what can be done at CERN and in Europe beyond the Large Hadron Collider.

We have made a special effort to find all the authors of the neutral current discovery with Gargamelle, and of the W & Z discoveries with UA1 and UA2, and many of them are here. It's a very nice occasion to see so many of our friends at the same time. If any of you desire, you can see some elements of Gargamelle, UA1 and UA2 in the Microcosm exhibit.

We have received many nice messages, in particular from Fred Bullock, Peter Kalmus, Peter Jenni and many others, who could not be with us today for various reasons, and we would like to thank them for their interest in this event.

To conclude, I would like to thank, on behalf of the Organisation, all those who made these discoveries possible, not only the physicists, but also the accelerators' staff and the staff from other parts of CERN, civil engineering, technical support, administration, computing and others.

Thank you very much.

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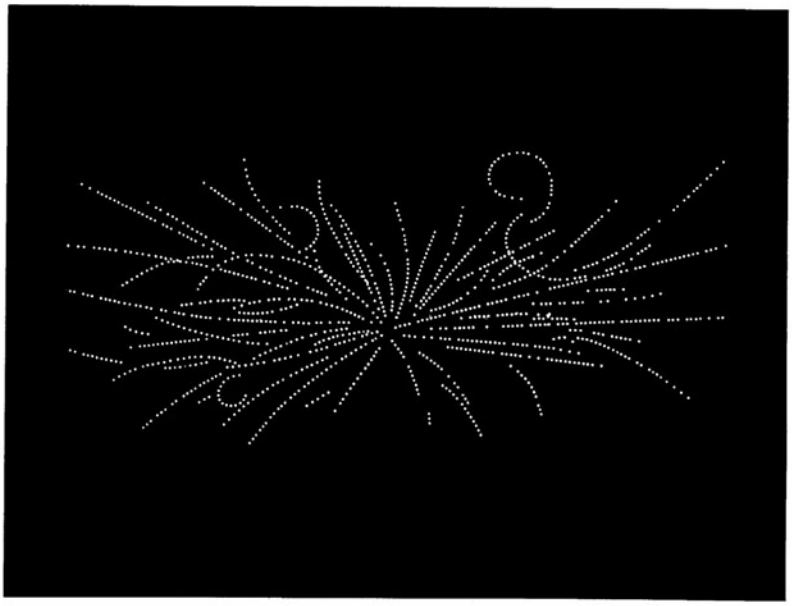
3rd TOPICAL WORKSHOP ON PROTON ANTIPROTON COLLIDER PHYSICS●



TOPICS

- REVIEW AND PERSPECTIVES AFTER ONE YEAR OF PROTON-ANTIPROTON PHYSICS.
- RECENT DATA FROM PROTON-ANTIPROTON COLLIDERS.
- INTERMEDIATE BOSONS: THEORETICAL AND EXPERIMENTAL PROGRESS.
- NEW FLAVOURS: EXPERIMENT AND THEORY.
- RECENT ADVANCES IN TECHNIQUES FOR HIGH ENERGY EXPERIMENTS.
- FUTURE COLLIDERS.

ROME JANUARY 12-14 1983

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Address request for information and invitations to:
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3-rd Topical Workshop on proton antiproton collider Physics
Istituto di Fisica «G. Marconi»
P.le Aldo Moro, 2 - 00185 Roma - Italia

● *Previous Workshops were held at College de France (1979) and at Madison (1981)*

Fig. 1. Front page of the proceedings of the 3rd Topical Workshop on Proton-Antiproton Collider Physics; Rome, Italy, 12–14 Jan. 1983; CERN Report 83-04

First published in Eur. Phys. J. C 34, 3–4 (2004)
Digital Object Identifier (DOI) 10.1140/epjc/s2004-01760-2

Steven Weinberg

The making of the Standard Model

I have been asked to review the history of the formation of the Standard Model. It is natural to tell this story as a sequence of brilliant ideas and experiments, but here I will also talk about some of the misunderstandings and false starts that went along with this progress, and why some steps were not taken until long after they became possible. The study of what was *not* understood by scientists, or was understood wrongly, seems to me often the most interesting part of the history of science. Anyway, it is an aspect of the Standard Model with which I am very familiar, for as you will see in this talk, I shared in many of these misunderstandings.

I'll begin by taking you back before the Standard Model to the 1950's. It was a time of frustration and confusion. The success of quantum electrodynamics in the late 1940s had produced a boom in elementary particle theory, and then the market crashed. It was realized that the four-fermion theory of weak interactions had infinities that could not be eliminated by the technique of renormalization, which had worked so brilliantly in electrodynamics. The four-fermion theory was perfectly good as a lowest-order approximation, but when you tried to push it to the next order of perturbation theory you encountered unremovable infinities. The theory of strong interactions had a different problem; there was no difficulty in constructing renormalizable theories of the strong interactions like the original Yukawa theory but, because the strong interactions are strong, perturbation theory was useless, and one could do no practical calculations with these theories. A deeper problem with our understanding of both the weak and the strong interactions was that there was no rationale for any of these theories. The weak interaction theory was simply cobbled together to fit what experimental data was available, and there was no evidence at all for any particular theory of strong interactions.

There began a period of disillusionment with quantum field theory. The community of theoretical physicists tended to split into what at the time were sometimes called, by analogy with atomic wave functions, radial and azimuthal physicists. Radial physicists were concerned with dynamics, particularly the dynamics of the strong interactions. They had little to say about the weak interactions. Some of them tried to proceed just on the basis of general principles, using dispersion relations and Regge pole expansions, and they hoped ultimately for a pure S-matrix theory of the strong interactions, completely divorced from quantum field theory. Weak interactions would somehow take care of themselves later. Azimuthal physicists were more modest. They took it as a working rule that there was no point in trying to understand strong interaction dynamics, and instead they studied the one sort of thing that could be used to make predictions without such understanding – principles of symmetry.

But there was a great obstacle in the understanding of symmetry principles. Many symmetry principles were known, and a large fraction of them were only approximate. That was certainly true of isotopic spin symmetry, which goes back to 1936 [1]. Strangeness conservation was known from the beginning to be violated by the weak interactions [2]. Then in 1956 even the sacred symmetries of space and time, P and PT conservation, were found to be violated by the weak interactions [3], and CP conservation was found in 1964 to be only approximate [4]. The SU(3) symmetry of the "eightfold way" discovered in the early 1960s [5] was at best only a fair approximation even for the strong interactions. This left us with a fundamental question. Many azimuthal physicists had thought that symmetry principles were an expression of the simplicity of nature at its deepest level. So what are you to make of an approximate symmetry principle? The approximate simplicity of nature?

During this time of confusion and frustration in the 1950s and 1960s there emerged three good ideas. These ideas took a long time to mature, but have become fundamental to today's elementary particle physics. I am emphasizing here that it took a long time before we realized what these ideas were good for, partly because I want to encourage today's string theorists, who I think also have good ideas that are taking a long time to mature.

The first of the good ideas that I'll mention is the quark model, proposed in 1964 independently by Gell-Mann and Zweig [6]. The idea that hadrons are made of quarks and antiquarks, used in a naive way, allowed one to make some sense of the growing menu of hadrons. Also, the naïve quark model seemed to get experimental support from an experiment done at SLAC in 1968 under the leadership of Friedman, Kendall, and Taylor [7], which was analogous to the experiment done by Geiger and Marsden in Rutherford's laboratory in 1911. Geiger and Marsden had found that alpha particles were sometimes scattered by gold atoms at large angles, and Rutherford inferred from this that the mass of the atoms was concentrated in something like a point particle, which became known as the nucleus of the atom. In the same way, the SLAC experiment found that electrons were sometimes scattered from nucleons at large angles, and this was interpreted by Feynman and Bjorken [8] as indicating that the neutron and proton consisted of point particles. It was natural to identify these "partons" with Gell-Mann and Zweig's quarks. But of course the mystery about all this was why no one ever saw quarks. Why, for example, did oil drop experiments never reveal third integer charges? I remember Dalitz and Lipkin at various conferences showing all the successful predictions of the naïve quark model for hadron systematics, while I sat there remaining stubbornly unconvinced, because everyone knew that quarks had been looked for and not found.

The second of the good ideas that were extant in the 1950s and 1960s was the idea of gauge (or local) symmetry. (Of course electrodynamics was much older, and could have been regarded as based on a U(1) gauge symmetry, but that wasn't the point of view of the theorists who developed quantum electrodynamics in the 1930s.) Yang and Mills [9] in 1954 constructed a gauge theory based not on the simple one-dimensional group U(1) of electrodynamics, but on a three-dimensional group, the group SU(2) of isotopic spin conservation, in the hope that this would become a theory of the strong interactions. This was a beautiful theory because the symmetry dictated the form of the interactions. In particular, because the gauge group was non-Abelian (the "charges" do not commute with each other) there was a self-interaction of the gauge bosons, like the self-interactions of gravitons in general relativity. This was just the sort of thing that brings joy to the heart of an elementary particle theorist.

The quantization of non-Abelian gauge theories was studied by a number of other theorists [10], generally without any idea of applying these theories immediately to known interactions. Some of these theorists developed the theory of the quantization of Yang–Mills theories as a warm-up exercise for the problem they really wanted to solve, the quantization of general relativity. It took a few years before physicists began to apply the Yang–Mills idea to the weak interactions. This was in part because in 1954, as you may recall, the beta decay interactions were known to be a mixture of scalar, tensor, and perhaps pseudoscalar four-fermion interactions. This was the result of a series of wrong experiments, each one of which as soon as it was discovered to be wrong was replaced by another wrong experiment. It was not until 1957–1958 that it became generally realized that the weak interactions are in fact a mixture of vector and axial vector interactions [11], of the sort that would be produced by intermediate vector bosons.

Theories of intermediate vector bosons were then developed by several authors [12], but generally, except for the papers by Bludman in 1958 and by Salam and Ward in 1964, without reference to non-Abelian *local* symmetries. (For instance, with the exceptions noted, these papers did not include the quadrilinear interactions among vector bosons characteristic of theories with non-Abelian local symmetries.) I will have more to say about some of these papers later.

From the beginning, the chief obstacle to the application of the Yang–Mills approach to theories of either the weak or the strong interactions was the problem of mass. Gauge symmetry forbids the gauge bosons from having any mass, and it was supposed that any massless gauge bosons would surely have been detected. In all the papers of [12] a mass was put in by hand, but this would destroy the rationale for a gauge theory; the local symmetry principle that motivates such theories would be violated by the insertion of a mass. Obviously also the arbitrary insertion of mass terms makes theories less predictive. Finally, through the work of several authors [13] in the 1960s, it was realized that non-Abelian gauge theories with mass terms inserted by hand are non-renormalizable, and therefore in this respect do not represent an advance over the original four-fermion weak interaction.

The third of the good ideas that I wished to mention was the idea of spontaneously broken symmetry: there might be symmetries of the Lagrangian that are not symmetries of the vacuum. Physicists came to this idea through two rather different routes.

The first route was founded on a fundamental misunderstanding. Remember that for some time there had been a problem of understanding the known approximate symmetries. Many of us, including myself, were at first under the illusion that if you had an exact symmetry of the field equations of nature which was spontaneously broken then it would appear experimentally as an approximate symmetry. This is quite wrong, but that's what we thought. (Heisenberg continued to believe this as late as 1975 [14].) At first this seemed to offer a great hope of understanding the many approximate symmetries, like isotopic spin, the 8-fold way, and so on. Thus it was regarded as a terrible setback in 1961 when Goldstone announced a theorem [15], proved by Goldstone, Salam and myself [16] the following year, that for every spontaneously broken symmetry there must be a massless spinless particle. We knew that there were no such massless Goldstone bosons in strong-interaction physics — they would have been obvious many years before — so this seemed to close off the opportunities provided by spontaneous symmetry breaking. Higgs [17] in 1964 was motivated by this disappointment to try to find a way out of the Goldstone theorem. He recognized that the Goldstone theorem would not apply if the original symmetry was not just a global symmetry like isotopic spin conservation, but a gauge symmetry like the local isotopic spin symmetry of the original Yang–Mills theory. The Goldstone boson then remains in the theory, but it turns into the helicity-zero part of a gauge boson, which thereby gets a mass. At about the same time Englert and Brout [18] independently discovered the same phenomenon, but with a different motivation: they hoped to go back to the idea of using the Yang–Mills theory to construct a theory of the strong interactions mediated by massive vector bosons. This phenomenon had also been noted earlier by Anderson [19], in a non-relativistic context.

The second of the routes to broken symmetry was the study of the currents of the semi-leptonic weak interactions, the vector and axial-vector currents. In 1958 Goldberger and Treiman [20] gave a derivation of a relation between the pion decay constant, the axial vector coupling constant of beta decay, and the strong coupling constant. The relation worked better than would be expected from the rather implausible approximations used. It was in order to explain the success of the Goldberger–Treiman relation that several theorists [21] in the following years developed the idea of a partially conserved axial-vector current, that is, an axial-vector current whose divergence was not zero but was proportional to the pion field. Taken literally, this was a meaningless proposition, because any field operator that had the right quantum numbers, such as the divergence of the axial-vector current, can be called the pion field. Nature does not single out specific operators as the field of this or that particle. This idea was greatly clarified by Nambu [22] in 1960. He pointed out that in an ideal world, where the axial-vector current was not partially conserved but exactly conserved, the existence of a non-vanishing nucleon mass and axial vector coupling would require the pion to be a particle of zero mass. At sufficiently small momentum transfer this massless pion would dominate the pseudoscalar part of the one-nucleon matrix element of the axial vector current, which leads to the same Goldberger–Treiman result that had previously motivated the notion of partial current conservation. Nambu and Jona-Lasinio [23] worked out a dynamical model in which the axial-vector current would be exactly conserved, and showed that the spectrum of bound states did indeed include a massless pion.

In this work there was little discussion of spontaneously broken symmetry. In particular, because the work of Nambu and his collaborators [24] on soft-pion interactions only involved a single soft pion, it was not necessary to identify a particular broken symmetry group. In much of their work it was taken to be a simple U(1) symmetry group. Nambu *et al.* like Gell-Mann *et al.* [21] emphasized the properties of the currents of beta decay rather than broken symmetry. Nambu, especially in the paper with Jona-Lasinio, described what he was doing as an analog to the successful theory of superconductivity of Bardeen, Cooper and Schrieffer [25]. A superconductor is nothing but a place where electromagnetic gauge invariance is spontaneously broken, but you will not find that statement or any mention of spontaneously broken symmetry anywhere in the classic BCS paper. Anderson [19] did realize the importance of spontaneous symmetry breaking in the theory of superconductivity, but he was almost the only condensed matter physicist who did.

The currents of the semi-leptonic weak interactions remained the preoccupation of Gell-Mann and others, who proposed working with them the way Heisenberg had worked with atomic electric dipole transition matrix elements in his famous 1925 paper on quantum mechanics, that is, by deriving commutation relations for the currents and then saturating them by inserting sums over suitable intermediate states [26]. This was the so-called current algebra program. Among other things, this approach was used by Adler and Weisberger to derive their celebrated formula for the axial-vector coupling constant of beta decay [27].

Sometime around 1965 we began to understand all these developments and how they were related to each other in a more modern way. It was realized that the strong interactions must have a broken symmetry, SU(2) $\times$ SU(2), consisting of ordinary isotopic spin transformations plus chiral isotopic spin transformations acting oppositely on the left- and right-handed parts of nucleon fields. Contrary to what I and others had thought at first, such a broken symmetry does not look in the laboratory like an ordinary approximate symmetry. If it is an exact symmetry, but spontaneously broken, the symmetry implications are found in precise predictions for the low-energy interactions of the massless Goldstone bosons, which for SU(2) $\times$ SU(2) would be the pions. Among these “soft pion” formulas is the Goldberger–Treiman relation, which should be read as a formula for the pion–nucleon coupling at zero pion momentum. Of course SU(2) $\times$ SU(2) is only an approximate symmetry of the strong interactions, so the pion is not a massless particle, but is what (over Goldstone’s objections) I later called a pseudo-Goldstone boson, with an exceptionally small mass.

From this point of view one can calculate things having nothing to do with the electroweak interactions, nothing to do with the semi-leptonic vector and axial vector currents, but that refer solely to the strong interactions. Starting in 1965, the pion–nucleon scattering lengths were calculated independently by Tomozawa and myself [28], and I calculated the pion–pion scattering lengths [29]. Because these processes involve more than one soft pion, the results of these calculations depended critically on the SU(2) × SU(2) symmetry. This work had a twofold impact. One is that it tended to kill off the S-matrix approach to the strong interactions, because although there was nothing wrong with the S-matrix philosophy, its practical implementation relied on the pion–pion interaction being rather strong at low energy, while these new results showed that the interaction is in fact quite weak at low energy. This work also tended for a while to reduce interest in what Higgs and Brout and Englert had done, for we no longer wanted to get rid of the nasty Goldstone bosons (as had been hoped particularly by Higgs), because now the pion was recognized as a Goldstone boson, or very nearly.

This brings me to the electroweak theory, as developed by myself [30], and independently by Salam [31]. Unfortunately Salam is not with us to describe the chain of reasoning that led him to this theory, so I can only speak about my own work. My starting point in 1967 was the old aim, going back to Yang and Mills, of developing a gauge theory of the strong interactions, but now based on the symmetry group that underlies the successful soft-pion predictions, the symmetry group SU(2) × SU(2) [32]. I supposed that the vector gauge boson of this theory would be the rho-meson, which was an old idea, while the axial-vector gauge boson would be the $a1$ meson, an enhancement in the pi–rho channel which was known to be needed to saturate certain spectral function sum rules, which I had developed a little earlier that year [33]. Taking the SU(2) × SU(2) symmetry to be exact but spontaneously broken, I encountered the same result found earlier by Higgs and Brout and Englert; the Goldstone bosons disappeared and the $a1$ meson became massive. But with the isotopic spin subgroup unbroken, then (in accordance with a general result of Kibble [34]) the rho-meson would remain massless. I could of course put in a common mass for the $a1$ and rho by hand, which at first gave encouraging results. The pion now reappeared as a Goldstone boson, and the spontaneous breaking of the symmetry made the $a1$ mass larger than the rho mass by a factor of the square root of two, which was just the ratio that had come out of the spectral function sum rules. For a while I was encouraged, but the theory was really too ugly. It was the same old problem: putting in a rho-meson mass or any gauge boson mass by hand destroyed the rationale for the theory and made the theory less predictive, and it also made the theory not renormalizable. So I was very discouraged.

Then it suddenly occurred to me that this was a perfectly good sort of theory, but I was applying it to the wrong kind of interaction. The right place to apply these ideas was not to the strong interactions, but to the weak and electromagnetic interactions. There would be a spontaneously broken gauge symmetry (probably not SU(2) × SU(2)) leading to massive gauge bosons that would have nothing to do with the $a1$ meson but could rather be identified with the intermediate vector bosons of the weak interactions. There might be some generator of the gauge group that was not spontaneously broken, and the corresponding massless gauge boson would not be the rho meson, but the photon. The gauge symmetry would be exact; there would be no masses put in by hand.

I needed a concrete model to illustrate these general ideas. At that time I did not have any faith in the existence of quarks, and so I decided only to look at the leptons, and somewhat arbitrarily I decided to consider only symmetries that acted on just one generation of leptons, separately from antileptons – just the left-handed electron and electron-type neutrino, and the right-handed electron. With those ingredients, the largest gauge group you could possibly have would be SU(2) × U(1) × U(1). One of the U(1)s could be taken to be the gauge group of lepton conservation. Now, I knew that lepton number was conserved to a high degree of accuracy, so this U(1) symmetry was presumably not spontaneously broken, but I also knew that there was no massless gauge boson associated with lepton number, because according to an old argument of Lee and Yang [35] it would produce a force that would compete with gravitation. So I decided to exclude this part of the gauge group, leaving just SU(2) × U(1) gauge symmetry. The gauge bosons were then the charged massive particle (and its antiparticle) that had traditionally been called the W; a neutral massive vector particle that I called the Z; and the massless photon. The interactions of these gauge bosons with the leptons and with each other were fixed by the gauge symmetry. Afterwards I looked back at the literature on intermediate vector boson theories from the late 1950s and early 1960s, and I found that the global SU(2) × U(1) group structure had already been proposed in 1961 by Glashow [12]. I only learned later of the independent 1964 work of Salam and Ward [12]. I think the reason that the four of us had independently come to the same SU(2) × U(1) group structure is simply because with these fermionic ingredients, just one generation of leptons, there is no other group you can be led to. But now the theory was based on an exact though spontaneously broken gauge symmetry.

The spontaneous breakdown of this symmetry had not only to give mass to the intermediate vector bosons of the weak interactions, it also had to give mass to the electron (and also, in another lepton doublet, to the muon.) The only scalar particles whose vacuum expectation values could give mass to the electron and the muon would have to form SU(2) × U(1) doublets with charges $+e$ and zero. For simplicity, I assumed that these would be the only kind of scalar fields in the theory. That made the theory quite predictive. It allowed the masses of the W and the Z as well as their couplings to be calculated in terms of a single unknown angle theta. Whatever the value of theta, the W and Z masses were predicted to be quite large, large enough to have escaped detection. The same results apply with several scalar doublets. (These predictions by the way could also have been obtained in a "technicolor" theory in which the electroweak gauge symmetry is spontaneously broken by strong forces, as realized twelve years later by Susskind and myself [36]. This is still a possibility, but such technicolor theories have problems, and I'm betting on the original scalar doublet or doublets.)

In addition to predicting the masses and interactions of the W and Z in terms of a single angle, the electroweak theory made another striking prediction which could not be verified at the time, and still has not been. A single scalar doublet of complex scalar fields can be written in terms of four real fields. Three of the gauge symmetries of SU(2) $\times$ U(1) are spontaneously broken, which eliminates the three Goldstone bosons associated with these fields. This leaves over one massive neutral scalar particle, as a real particle that can be observed in the laboratory. This particle, which first made its appearance in the physics literature in 1967 [30], has so far not made its appearance in the laboratory. Its couplings were already predicted in this paper, but its mass is still unknown. To distinguish this particle from the Goldstone bosons it has come to be called the Higgs boson, and it is now a major target of experimental effort. With several doublets (as in supersymmetry theories) there would be several of these particles, some of them charged.

Both Salam and I guessed that the electroweak theory is renormalizable, because we had started with a theory that was manifestly renormalizable. But the theory with spontaneous symmetry breaking had a new perturbative expansion, and the question was whether or not renormalizability was preserved in the new perturbation theory. We both said that we thought that it was, but didn't prove it. I can't answer for Salam, but I can tell you why I didn't prove it. It was because at that time I disliked the only method by which it could be proved – the method of path integration. There are two alternative approaches to quantization: the old operator method that goes back to the 1920s, and Feynman path integration [37]. When I learned the path-integration approach in graduate school and subsequent reading, it seemed to me to be no more powerful than the operator formalism, but with a lot more hand-waving. I tried to prove the renormalizability of the electroweak theory using the most convenient gauge that can be introduced in the operator formalism, called unitarity gauge, but I couldn't do it [38]. I suggested the problem to a student [39], but he couldn't do it either, and to this day no one has done it using this gauge. What I didn't realize was that the path-integral formalism allows the use of gauges that cannot be introduced as a condition on the operators in a quantum field theory, so it gives you a much larger armamentarium of possible gauges in which gauge invariant theories can be formulated.

Although I didn't understand the potentialities of path integration, Veltman and his student 't Hooft did. In 1971 't Hooft used path integration to define a gauge in which it was obvious that spontaneously broken non-Abelian gauge theories with only the simplest interactions had a property that is essential to renormalizability, that in all orders of perturbation theory there are only a finite number of infinities [40]. This did not quite prove that the theory was renormalizable, because the Lagrangian is constrained by a spontaneously broken but exact gauge symmetry. In the 't Hooft gauge it was obvious that there were only a finite number of infinities, but how could one be sure that they exactly match the parameters of the original theory as constrained by gauge invariance, so that these infinities can be absorbed into a redefinition of the parameters? This was initially proved in 1972 by Lee and Zinn-Justin [41] and by 't Hooft and Veltmann [42], and later in an elegant formalism by Becchi, Rouet, and Stora, and by Tyutin [43]. But I must say that after 't Hooft's original 1971 paper, (and, for me, a subsequent related paper by Ben Lee [44]) most theorists were pretty well convinced that the theory was renormalizable, and at least among theorists there was a tremendous upsurge of interest in this kind of theory.

From today's perspective, it may seem odd that so much attention was focused on the issue of renormalizability. Like general relativity, the old theory of weak interactions based on four-fermion interactions could have been regarded as an effective quantum field theory [45], which works perfectly well at sufficiently low energy, and with the introduction of a few additional free parameters even allows the calculation of quantum corrections. The expansion parameter in such theories is the energy divided by some characteristic mass and as long as you work to a given order in the energy you will only need a finite number of coupling types, so that the coupling parameters can absorb all of the infinities. But such theories inevitably lose all predictive power at energies above the characteristic mass. For the four-fermion theory of weak interactions it was clear that the characteristic mass was no greater than about 300 GeV, and as we now know, it is actually of the order of the W mass. The importance of the renormalizability of the electroweak theory was not so much that infinities could be removed by renormalization, but rather that the theory had the potentiality of describing weak and electromagnetic interactions at energies much greater than 300 GeV, and perhaps all the way up to the Planck scale. The search for a renormalizable theory of weak interactions was the right strategy but, as it turned out, not for the reasons we originally thought.

These attractive theories of the electroweak theory did not mean that the theory was *true* – that was a matter for experiment. After the demonstration that the electroweak theory is renormalizable, its experimental consequences began to be taken seriously. The theory predicted the existence of neutral currents, but this was an old story. Suggestions of neutral weak currents can be traced back to 1937 papers of Gamow and Teller, Kemmer, and Wentzel [46]. Neutral currents had appeared in the 1958 paper by Bludman and in all the subsequent papers in [12], including of course those of Glashow and of Salam and Ward. But now there was some idea about their strength. In 1972 I looked at the question of how easy it would be to find semi-leptonic neutral current processes, and I found that although in the electroweak theory they are somewhat weak compared to the ordinary charged-current weak interactions, they were not too weak to be seen [47]. In particular, I pointed out that the ratio of elastic neutrino–proton scattering to the corresponding inelastic charged-current reaction would have a value between 0.15 and 0.25, depending on the value of the unknown angle theta. A 1970 experiment [48] had given a value of 0.12 plus or minus 0.06 for this ratio, but the experimenters didn't believe that they were actually seeing neutral currents, so they didn't claim to have observed a neutral current reaction at a level of roughly 12% of the charged current reaction, and instead quoted this result as an upper bound. The minimum theoretical value 0.15 of this ratio applies for sine-squared theta equal to 0.25, which is not far from what we now know is the correct value. I suspect that this 1970 experiment had actually observed neutral currents, but you get credit for making discoveries only when you claim that you have made the discovery.

Neutral currents were discovered in 1973 at CERN [49]. I suspect that this will be mentioned later today, so I won't go into it here. At first the data on neutral current reactions looked like it exactly fit the electroweak theory, but then a series of other experiments gave contrary results. The most severe challenge came in 1976 from two atomic physics experiments [50] that seemed to show that there was no parity violation in the bismuth atom at the level that would be expected to be produced by neutral current electron–nucleon interactions in the electroweak theory. For most theorists these experiments did not challenge the basic idea that weak interactions arise from a spontaneously broken gauge symmetry, but they threw serious doubt on the specific SU(2) × U(1) implementation of the idea. Many other models were tried during this period, all sharing the property of being terribly ugly. Finally, parity violation in the neutral currents was discovered at the expected level in electron–nucleon scattering at SLAC in 1978 [51], and after that most physicists took it for granted that the electroweak theory is essentially correct.

The other half of the Standard Model is quantum chromodynamics. By the early 1970s the success of the electroweak theory had restored interest in Yang–Mills theory. In 1973 Gross and Wilczek and Politzer independently discovered that non-Abelian gauge theories have the remarkable property of asymptotic freedom [52]. They used renormalization group methods due to Gell-Mann and Low [53], which had been revived in 1970 by Callan, Symanzik, Coleman and Jackiw [54], to define an effective gauge coupling constant as a function of energy, and they showed that in Yang–Mills theories with not too many fermions this coupling goes to zero as the energy goes to infinity. (‘t Hooft had found this result and announced it at a conference in 1972, but he waited to publish this result and work out its implications while he was doing other things, so his result did not attract much attention.) It was already known both from baryon systematics and from the rate of neutral pion decay into two photons that quarks of each flavor u, d, s, etc. must come in three colors [55], so it was natural to take the gauge symmetry of the strong interactions as an SU(3) gauge group acting on the three-valued color quantum number of the quarks. Subsequent work [56] by Gross and Wilczek and by Georgi and Politzer using the Wilson operator product expansion [57] showed that the decrease of the strong coupling constant with increasing energy in this theory explained why “partons” had appeared to be weakly coupled in the 1968 Friedman–Kendall–Taylor experiment [7].

But a big problem remained: what is one to do with the massless SU(3) gauge bosons, the gluons? The original papers [52] of Politzer and Gross and Wilczek suggested that the reason why massless gluons are not observed is that the gauge symmetry is spontaneously broken, just as in the electroweak theory. The gluons could then be assumed to be too heavy to observe. Very soon afterwards a number of authors independently suggested an alternative, that the gauge symmetry is not broken at all, the gluons are in fact massless, but we don’t see them for the same reason that we don’t see the quarks, which is that, as a result of the peculiar infrared properties of non-Abelian gauge theories, color is trapped; color particles like quarks and gluons can never be isolated [58]. This has never been proved. There is now a million dollar prize offered by the Cray Foundation to anyone who succeeds in proving it rigorously, but since it is true I for one am happy to leave the proof to the mathematicians.

One of the great things that came out of this period of the development of the electroweak and the strong interaction theories is an understanding at long last of the old approximate symmetries. It was now understood that these symmetries were approximate because they weren't fundamental symmetries at all; they were just accidents. Renormalizable quantum chromodynamics must respect strangeness conservation and charge conjugation invariance, and, aside from a non-perturbative effect that I don't have time to go into, it must also respect parity and time reversal invariance. You cannot introduce any renormalizable interaction into the theory that would violate those symmetries. This would not be true if scalar fields participated in the strong interactions, as in the old Yukawa theory. This result was not only aesthetically pleasing, but crucial, because if there were possible renormalizable interactions that violated, say, strangeness conservation, or parity, then even if you didn't put such interactions in the theory, higher order weak interactions would generate them at first order in the fine structure constant [59]. There would then be violations of parity and strangeness conservation in the strong interactions at a level of a percent or so, which certainly is not the case.

If one makes the additional assumption that the up, down and strange quark masses are small, then without having to assume anything about their ratios it follows that the theory has an approximate SU(3) $\times$ SU(3) symmetry, including not only the eightfold way but also the spontaneously broken chiral SU(2) $\times$ SU(2) symmetry that had been used to derive theorems for low-energy pions back in the mid 1960s. Furthermore, with an intrinsic SU(3) $\times$ SU(3) symmetry breaking due to small up, down and strange quark masses, this symmetry gives rise to the Gell-Mann–Okubo mass formula [60] and justifies the symmetry-breaking assumptions made in the 1965 derivation of the pion–pion scattering lengths [29]. Finally, it is automatic in such theories that the semi-leptonic currents of the weak interactions must be symmetry currents associated with this SU(3) $\times$ SU(3) symmetry. This was a really joyous moment for theorists. Suddenly, after all those years of dealing with approximate symmetries, it all fell into place. They are not fundamental symmetries of nature at all; they are just accidents dictated by the renormalizability of quantum chromodynamics and the gauge origin of the electroweak interactions.

Before closing, I must also say something about two other topics: the problem of strangeness nonconservation in the weak interactions, and the discoveries of the third generation of quarks and leptons and of the W and Z.

The charge exchange semileptonic interactions were long known to violate strangeness conservation, so any charged W boson would have to have couplings in which strangeness changes by one unit. It follows that the exchange of pairs of Ws could produce processes like K-antiK conversion in which strangeness changes by two units. With an ultraviolet cut-off of the order of the W mass, the amplitude for such processes would be suppressed by only two factors of the inverse W mass, like a first-order weak interaction, in contradiction with the known magnitude of the mass difference of the K_1 and K_2. A way out of this difficulty was discovered in 1970 by Glashow, Iliopoulos and Maiani [61]. They found that these strangeness-violating first-order weak interactions would disappear if there were two full doublets of quarks, entering in the same way in the weak interactions. This required a fourth quark, called the charm quark. They also showed that with the fourth quark in the theory, in an SU(2) gauge theory the neutral currents would not violate strangeness conservation. In 1972 I showed that the GIM mechanism also works for the Z exchange of the SU(2) $\times$ U(1) electroweak theory [62]. The introduction of the fourth quark also had the happy consequence, as shown independently by Bouchiat, Iliopoulos, and Meyer and by myself [63], that the triangle anomalies that would otherwise make the theory not really gauge invariant all cancelled. The K_1-K_2 mass difference was calculated as a function of the charm quark mass by Gaillard and Lee [64], who used the experimental value of this mass difference to estimate that the mass of the charm quark would be about 1.5 GeV. Further, using the new insight from quantum chromodynamics that the strong coupling is not so strong at energies of this order, Applequist and Politzer in 1974 (just before the discovery of the J/psi) predicted that the charm–anticharm bound state would be rather narrow [65]. This narrow bound state was discovered in 1974 [66], and immediately not only provided evidence for the existence of a fourth quark, but also gave vivid testimony that quarks are real.

The only thing remaining in the completion of the Standard Model was the discovery of the third generation: the tau lepton [67] (and the corresponding neutrino) and the bottom [68] and top [69] quarks. This provided a new mechanism for CP violation, the complex phase factor in the Cabibbo–Kobayashi–Maskawa matrix [70] appearing in the semi-leptonic weak interactions. The fact that the third generation of quarks is only slightly mixed in this matrix with the first and second generations even makes it natural that the CP violation produced in this way should be rather weak. Unfortunately, the explanation of the masses and mixing angles in the Cabibbo–Kobayashi–Maskawa matrix continues to elude us.

These developments were crowned in 1983 with the discovery [71] of the W and the Z intermediate vector bosons. It has proved possible to measure their masses with great precision, which has allowed a stringent comparison of the electroweak theory with experiment. This comparison has even begun to give hints of the properties of the as yet undiscovered scalar particle or particles.

Well, those were great days. The 1960s and 1970s were a time when experimentalists and theorists were really interested in what each other had to say, and made great discoveries through their mutual interchange. We have not seen such great days in elementary particle physics since that time, but I expect that we will see good times return again in a few years, with the beginning of a new generation of experiments at this laboratory.

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First published in Eur. Phys. J. C 34, 5–13 (2004)
Digital Object Identifier (DOI) 10.1140/epjc/s2004-01761-1

Giorgio Brianti

CERN's contribution to accelerators and beams

1 Introduction

Looking back at happy events of the past, the temptation is to see them with the magnifying glasses of the "good old times". In the cases of the discoveries that we celebrate today, I dare say that CERN's contributions to accelerators and beams were objectively important and even essential. Indeed, it was more than competent mastering of well proven techniques of beam acceleration, beam storing and beam handling. Of course, it needed all this, even brought to extremes, but the additional decisive touch was due to real inventions and to techniques used for the first time.

Today, I am proud to represent here the CERN accelerator community of that time. I am one of many, who, particularly in the case of the proton–antiproton project, worked enthusiastically for supplying the beams leading to the discovery of W's and Z's.

I will concentrate on:

- particle focusing, the Magnetic Horn;
- beam intensity enhancement, the PS Booster;
- proton–proton collisions, the ISR;
- proton–antiproton collisions, made possible by the stochastic beam cooling, and the entire proton–antiproton complex;
- LEP and LHC.

2 Magnetic horn

In 1961 S. van der Meer (Fig. 1) invented a device called the "Magnetic Horn", which helped a great deal to focus the particles emerging from a target, with the

Fig. 1. S. van der Meer describing the Horn to visitors

result of vastly enhanced flux at the detector, in particular of neutrinos. One can call it a "current sheet lens" since it produces a highly focusing magnetic field in a space of cylindrical symmetry by a kind of coaxial line with a hollow central conductor, made of a thin aluminium sheet (Fig. 2). The current is in the range 100 to 400 kA in order to reach magnetic fields of several Tesla. Therefore, the horn must be pulsed (half-sine wave of about 15 μs) to avoid excessive heating. The geometrical configuration and wall thickness can be easily adapted to the beam energy and to the application. Horns have been in use for 40 years, mainly for neutrino beams and for collecting antiprotons. In the case of neutrino beams, one usually uses two horns to collect efficiently pions and kaons of one given electric charge (Fig. 3). Switching the polarity of the horn system allows to switch between neutrino and antineutrino beams.

The photograph in Fig. 4 shows the Horn used for focussing the antiprotons at the entrance of the Antiproton Accumulator (AA).

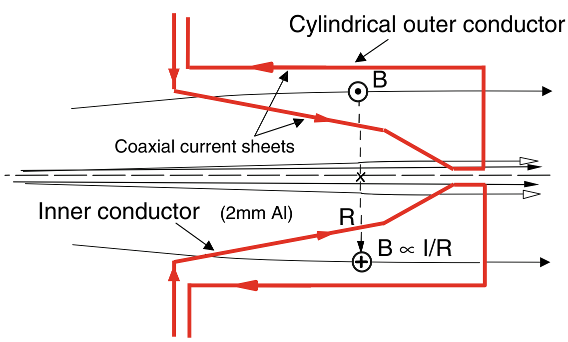

Fig. 2. Cross-section and principle of the Horn. The target produces charged particles: positively charged pions and kaons are emerging at various energies and angles (Courtesy of J.-M. Maugain)

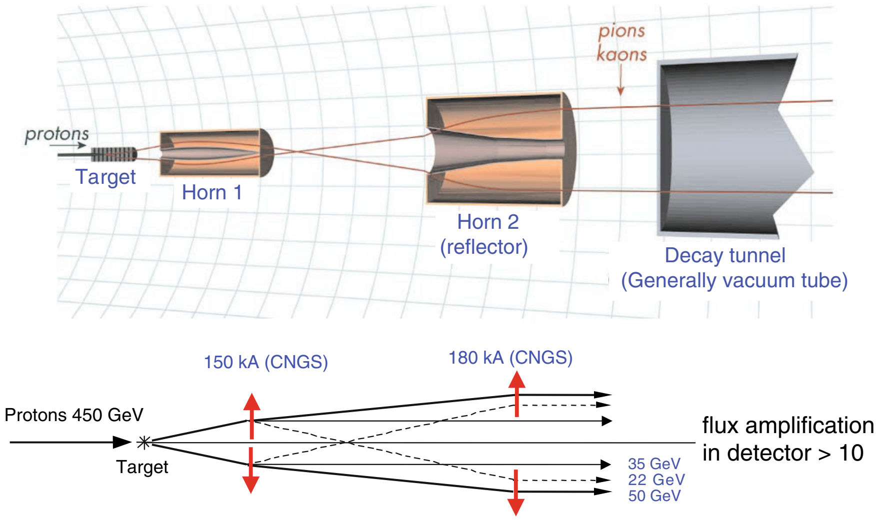

Fig. 3. Set of two horns as used for the CERN Neutrino Beam to Gran Sasso (CNGS). Usually, two horns are needed to produce a parallel wide band beam where a much larger number of particles emerging at various angles and energies are collected (Courtesy of J.-M. Maugain)

Fig. 4. Photograph of the Horn used for focusing antiprotons

3 PS Booster

In the mid sixties, it became clear that the best way to increase the PS intensity to the level required by the experiments and the ISR (10^{13} p/pulse) was to increase substantially the injection energy. Indeed, the main phenomenon limiting the intensity was incoherent or single particle tune shift, which scales like (beta)^2(gamma)^3. P. Germain launched the study of different alternatives (linacs and synchrotrons), which was led by H.K. Reich. The final choice favoured a multi-channel synchrotron, named the PS Booster [1].

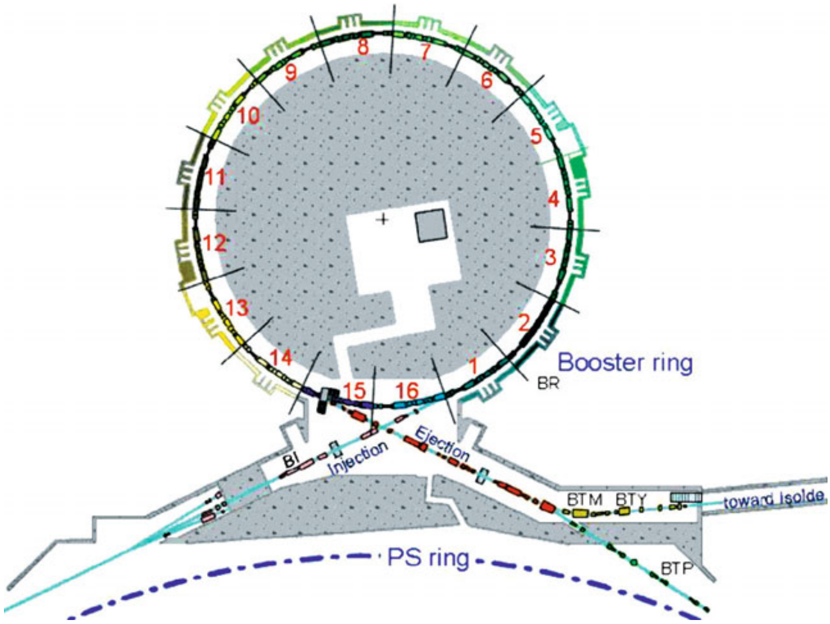

Fig. 5. PS Booster layout (From Sven De Man, 28/03/2000)

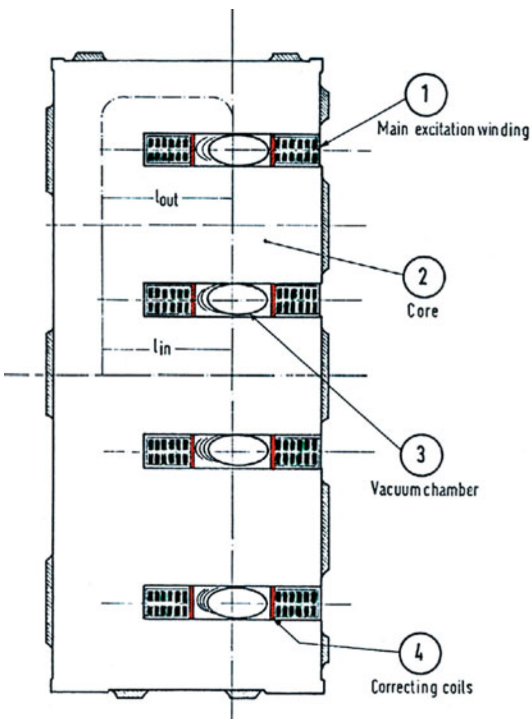

Fig. 6. Cross-section of Booster Dipole

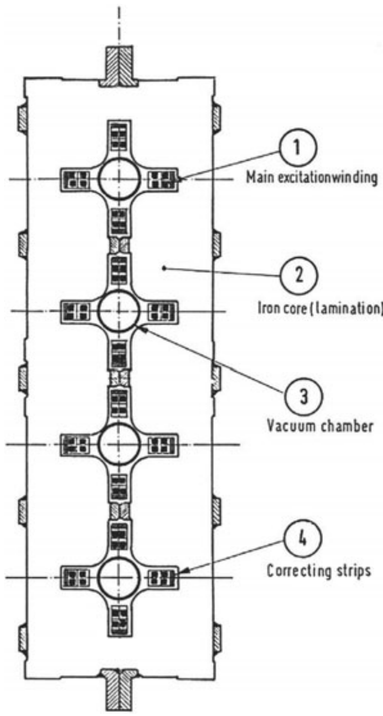

Fig. 7. Cross-section of Booster Quadrupole

The number of vertically stacked synchrotrons was set at four, each of them able to obtain a 2.5 intensity increase with respect to the PS without a substantial increase in emittance (particularly important for the ISR). The energy was set at 800 MeV (a momemtum of 1463 MeV/*c*), which ensured more than a ten-fold increase of the incoherent (or individual) particle limit at injection into the PS. The ring radius was chosen to be one quarter of the PS radius (Fig. 5). The cross-sections of the dipoles and of the quadrupoles are shown in Figs. 6 and 7.

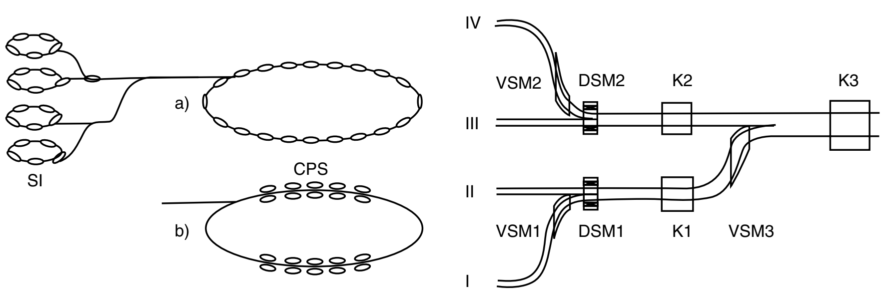

Fig. 8. On the left is shown the arrangements of bunches ejected from Booster: a) twenty sequential bunches; b) two times five vertically stacked bunches; on the right is shown the injection into the PS ring

After more than 30 years of good and reliable service, one can point out that the four channels allow the combination of beam bunches in the way suiting best the served machine. For example, 20 sequentially ejected bunches (Fig. 8*a*) or 2×10 bunches by vertically stacking bunches from 2 Booster rings (Fig. 8*b*) for the production of antiprotons or even a single bunch per ring as required by the LHC.

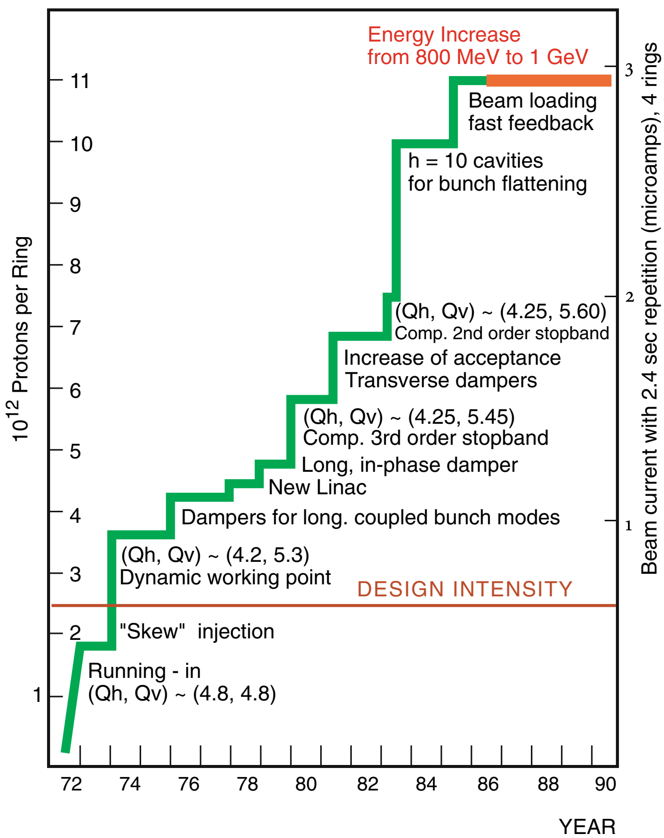

Fig. 9. Evolution of Booster intensity over time

Figure 9 shows the evolution of the Booster intensity in one ring due to successive improvements. The Booster provided a substantial and very welcome increase in proton intensity of the PS, in particular, for neutrino physics with Gargamelle in 1973 and for CHORUS and NOMAD in the late 1990's. The performance increase was also essential for the anti-proton programme at the SPS. The energy was increased firstly to 1 GeV and then to 1.4 GeV, as required by the LHC, without any change of the magnets. Figure 10 is a photograph of the Booster taken from the injection/ejection region.

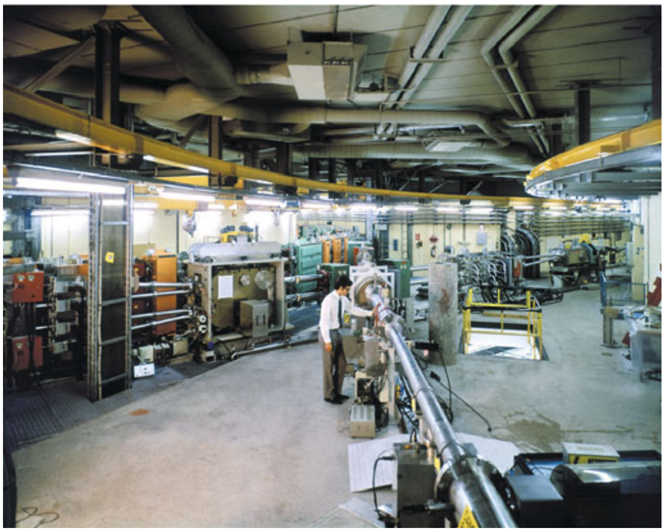

Fig. 10. PS Booster seen from the injection/ejection region

4 ISR, first proton–proton collider

In June 1957, in the middle of the construction of the PS, J.B. Adams set up a small Group in the PS Division to study new ideas for accelerators, which became later (1960) the Accelerator Research Division. The work concentrated on two possible lines: a proton–proton collider fed by the PS (later the ISR) and a proton synchrotron of about ten times the PS energy (later the SPS).

An electron analogue of a storage ring of only 2 MeV was built (CESAR, standing for CERN Electron and Accumulation Ring), to test ultra-high vacuum and particle accumulation and storage.

In December 1965, V. Weisskopf in his last Council Session as Director-General obtained approval for the ISR, the PS Improvement Programme with the Booster and the Bubble Chamber BEBC. The total investment was close to one billion CHF, but Vicky avoided making the addition of the items, which were approved in succession one by one.

The ISR [2] was the first proton–proton collider and reached eventually a centre-of-mass energy of 63 GeV[1]. It was planned for high luminosity and indeed it succeeded in colliding almost incredible beam currents (> 50 A) and scored a world record luminosity of more than 10^{32} cm^{-2} s^{-1}. The very high currents were obtained by stacking in momentum space, typically one thousand times the space occupied by a single pulse from the PS. Figure 11 is a photograph of a typical interaction region.

A considerable enhancement of accelerator technology was due to the ISR, in particular concerning reliability and stability of all components, ultra-high vacuum (1000 times lower pressure than in the PS), very intense beams inducing space-charge

[1] 2 times 31.4 GeV = 62.8 GeV $\sim$ 63 GeV

Table 1. Summary of the ISR performance (taken from the presentation by K. Johnsen at the ISR closure ceremony in 1984)

Current in normal operation	30–40 A
Maximum current	57 A
Maximum luminosity	1.4×10^{32} cm^{-2}s^{-1}
Typical current loss rate	1 ppm/m
Duration of physics runs	50–60 h
Maximum duration of antiproton beam	345 h

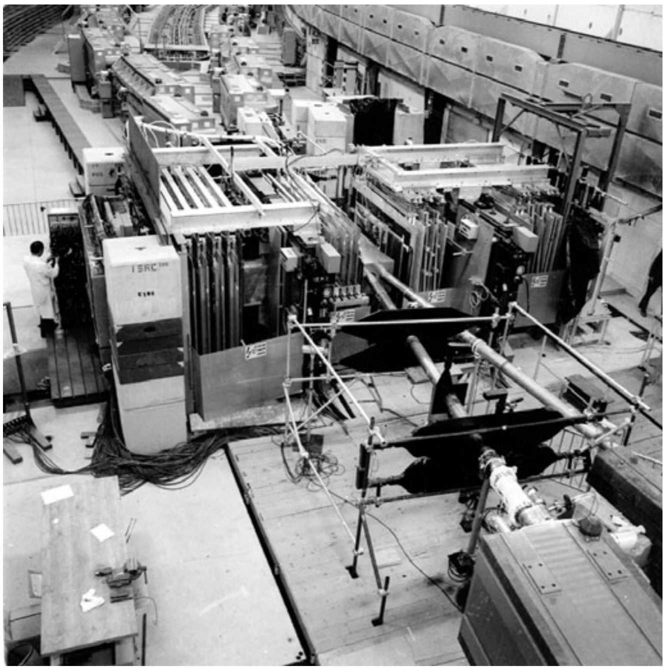

Fig. 11. Photograph of one ISR interaction region

effects and non-linear resonances. The know-how and expertise gained with the ISR were essential prerequisites for the success of the antiproton programme. The ISR performance is summarized in Table 1, as was once presented by the Project Leader, K. Johnsen.

In summary, several technical innovations were either discovered or applied with the ISR. They include:

- beam stacking;
- on-line space charge compensation;
- stochastic cooling;
- industrially built superconducting quadrupoles for low beta insertion.

5 SPS collider

The original report on Stochastic Cooling by S. van der Meer [3] was published in 1972 and the first successful tests were conducted in the ISR in 1974 by W. Schnell, L. Thorndahl and collaborators [4]. In the same period, ideas were put forward for the accumulation of antiprotons in storage rings by D. Möhl, P. Strolin and L. Thorndahl [5], and independently by P. McIntyre.

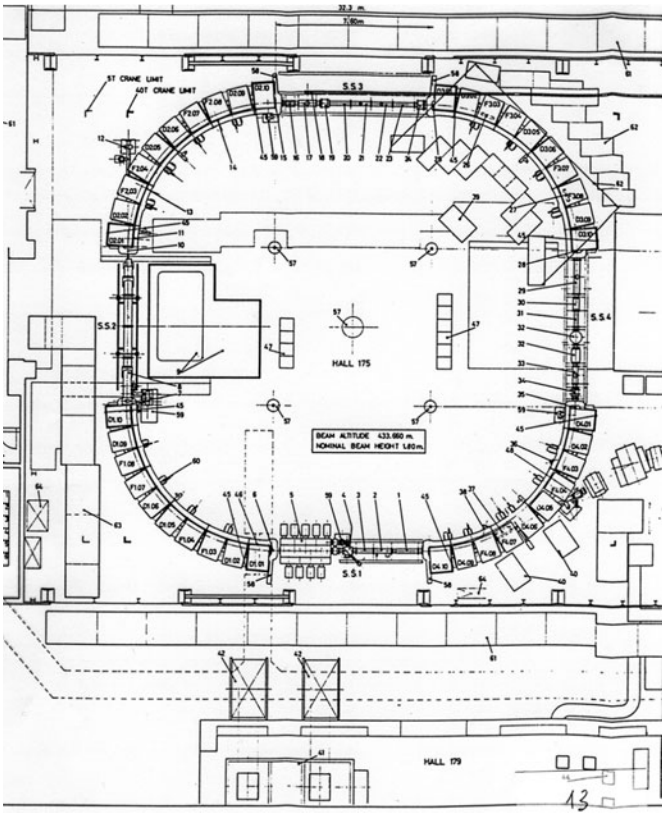

Fig. 12. Layout of ICE (Initial Cooling Experiment)

The decisive event occurred in 1976. Carlo Rubbia, at CERN, put forward the brilliant idea to convert the SPS to an antiproton–proton collider [6], which would make use of a single magnet ring (as for e^+e^- colliders). A similar proposal was made at Fermilab again by C. Rubbia, D. Cline, P. McIntyre and F. Mills [7].

The difficulty consisted in obtaining an antiproton beam of comparable intensity to the proton beam. The only way was to produce antiprotons using the 26 GeV protons of the PS (production rate of one antiproton for one million protons) and then store the antiprotons in an Accumulator Ring prior to their injection into the SPS.

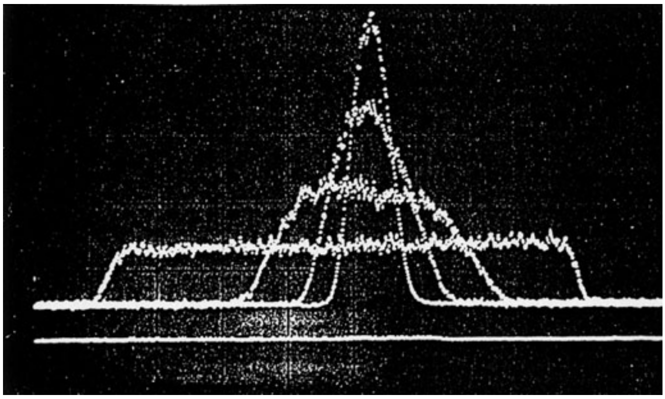

Fig. 13. Momentum cooling in ICE of 5×10^7 particles. Longitudinal Schottky signals after 0, 1, 2 and 4 minutes. The momentum spread was reduced from 3.5×10^{-3} to $5.\times10^{-4}$

The main obstacle to this operation is the large dispersion in angles and momenta of the antiprotons emerging from the target, while the Accumulator Ring has limited acceptances in the three dimensions. The only solution is to condense the beam either by electron or stochastic cooling. Since the latter was applied, let me concentrate on a simplified description of this method.

Macroscopically, the ensemble of the beam particles are contained in an area in phase-space, which, according to Liouville's Theorem, cannot be changed. In reality, the beam is not a continuum, but is made of individual particles with empty phase-space areas between them. The method consists in detecting the deviation of the barycentre of a small group of particles from the required value in a given location of the ring and then sending a correcting signal via a low-loss cable to an appropriate location on the other side of the ring in such a way that, when the particle packet passes through it, it is corrected and pushed toward the centre of the distribution. But what about Liouville's Theorem? If we now look at the beam on a microscopic scale, a way of explaining the Stochastic Cooling is that the empty phase-space areas between the particles are pushed to the outside of the beam and the particles crowded at the centre of the distribution. The operation is repeated many many times, so that, at the end, the phase-space density is increased enormously. The method requires special detectors associated to wide-band electronics (order 10 GHz).

The SPS needed also to be modified with the insertion of low-beta sections around the collision points, a considerable decrease of the vacuum pressure and, of course, the construction of huge (for the time) underground experimental areas for mobile experiments (UA1 on a platform, UA2 on air cushions). Indeed, it was necessary to withdraw the collider experiments from the ring to allow periods of fixed-target operation at least once a year.

The Research Director-General L. van Hove supported the project from the beginning, while the accelerator community was initially skeptical, but was soon filled by the enthusiasm of undertaking a very challenging enterprise. Prior to the final

Fig. 14. Photograph of the Antiproton Accumulator AA

Table 2. Overall performance of the SPS Collider from 1981 to 1990 (CC = AA + AC)

Year	1981	1982	1983	1984	1985	1988	1989	1990
	AA	AA	AA	AA	AA	CC	CC	CC
Energy (GeV)	273	273	273	315	315	315	315	315
Integrated luminosity per year (nb^{-1})	0.2	28	153	395	655	3372	4759	7241
Initial luminosity (10^{29} cm^{-2} s^{-1})	0.0	0.5	1.7	5.3	3.9	25	30	61
Hours realized	140	748	889	1065	1358	1316	2020	1803

design of the Antiproton Source, a test synchrotron called ICE (Initial Cooling Experiment) [8] was quickly assembled by G. Petrucci with the refurbished magnets of the g-2 experiment in order to test both electron and stochastic cooling (see Fig. 12). The stochastic cooling method obtained a brilliant confirmation, as it is shown in Fig. 13, and turned out to be much superior to electron cooling for the application to the CERN antiproton programme. A Committee chaired by F. Bonaudi finalized the accelerator project [9].

The scheme consisted of using the PS at maximum beam intensity concentrated over one quarter of the circumference, in order to match the circumference of the Antiproton Accumulator (AA) [10]. This was obtained by extracting the beam from the Booster in ten bunches, instead of the usual twenty, by recombining vertically the bunches of pairs of Booster rings, and by further reducing the ten bunches to five in the PS by an ingenious type of RF programming. The beam was then extracted from

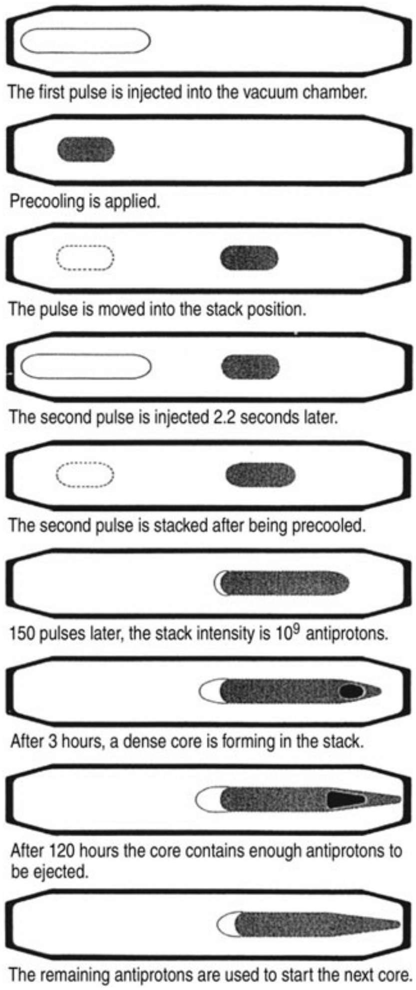

Fig. 15. Stacking and cooling of antiprotons in AA

the PS at 26 GeV and directed to the target at the entrance of the AA. The antiprotons were collected at 3.5 GeV by the magnetic horn shown in Fig. 4.

The design and construction of the AA (Antiproton Accumulator) was entrusted to R. Billinge and S. van der Meer. Despite the great sophistication and the number of elements, the ring was constructed and tested successfully in less than three years (Fig. 14). The process of stacking and cooling of the antiprotons in the AA is shown in Fig. 15 (from H. Koziol). The formation of a full antiproton stack took two to three days or one hundred thousand PS pulses. A question much debated at the time was what to do with the antiproton stack: direct injection into SPS at 3.5 GeV or post-acceleration in PS to 26 GeV in order to inject into the SPS above the transition energy. Since there was no agreement in Bonaudi's Committee about this point, J.B. Adams, Executive Director-General at that time and convinced supporter of

Fig. 16. Antiproton Collector (AC) around the AA

the project after the initial hesitation, took it upon himself to study thoroughly the question and decided in favour of post-acceleration. It was a wise decision, which undoubtedly facilitated the reliable operation of the collider.

The project was approved in 1978 and the first proton–antiproton collisions occurred on 10$^{\text{th}}$ July 1981. The first real period of physics exploitation occurred in 1982, with initial luminosities in the low $10^{29}\,\text{cm}^{-2}\text{s}^{-1}$ and integrated luminosity of 28 nb^{-1} (sufficient for the discovery of W's). The year 1983 saw the collected integrated luminosity increased to 153 nb^{-1} and the discovery of the Z's.

A few years later, a substantial improvement of the Antiproton Source was obtained by separating the function of collection and accumulation/cooling of antiprotons. This implied the addition of a second ring (Antiproton Collector, AC) around the original AA (Fig. 16). Consequently, the luminosity went well above $10^{30}\,\text{cm}^{-2}\text{s}^{-1}$, the record being $6 \times 10^{30}\,\text{cm}^{-2}\text{s}^{-1}$. Table 2 and Fig. 17 illustrate the performance of the SPS Collider over the years 1981 to 1990.

Looking back over the years to the early eighties, one non-technical but very important fall-out of the proton–antiproton undertaking was the daily working together of the experimental teams and of the accelerator people. We all remember with nostalgia the animated discussions at the five o'clock meeting in the SPS Control Room to decide the course of action for the following day. But it worked well in the end!

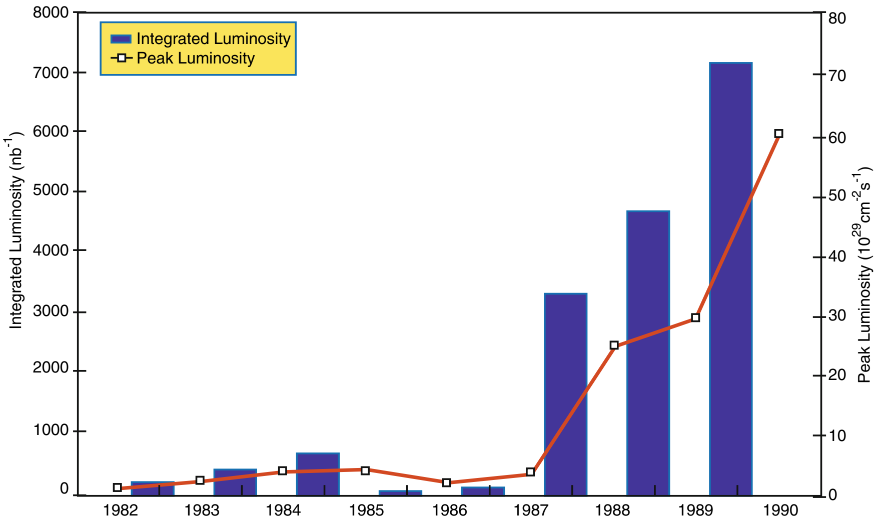

Fig. 17. Overall performance of SPS Collider from 1982 to 1990

6 LEP and LHC

After having built the PS, the ISR and the SPS, CERN took on the new challenge to construct an electron–positron collider with the purpose of studying in detail the properties of the W and Z bosons. The SPS Collider stopped operation in 1991 as LEP took over the full exploration of the Standard Model during more than a

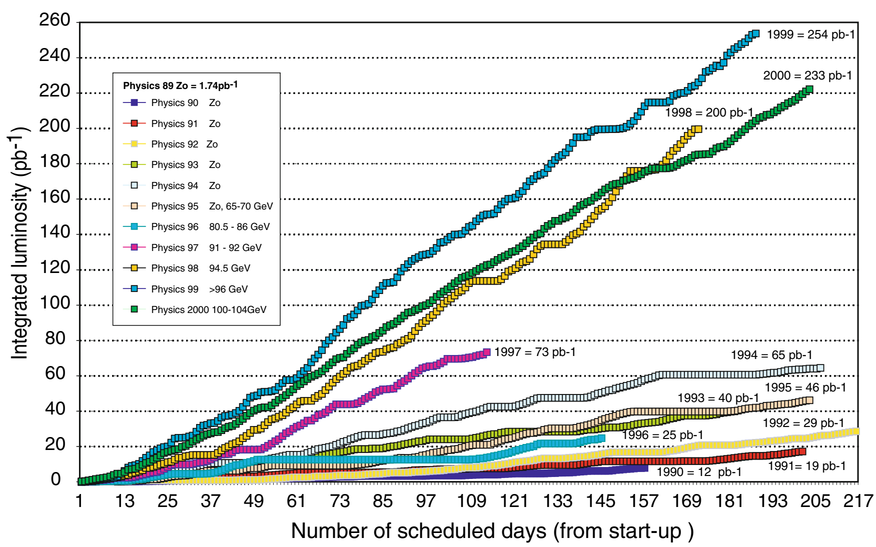

Fig. 18. Performance of the LEP Collider from 1989 to 2000

decade, by producing in particular millions of W's and Z's. Figure 18 summarizes the remarkable performance of the LEP Collider.

CERN has a tradition of developing an evolving accelerator infrastructure. Previous accelerators are used as injectors for the new accelerator. In the case of LEP, it is the tunnel which is being re-used to install a new machine. Today, the LEP tunnel starts being equipped with the elements of the next Collider, the LHC, which will continue the tradition of hadron colliders at CERN at much higher energy and luminosity. It will be the subject of a presentation in this symposium by L. Evans.

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First published in Eur. Phys. J. C 34, 15–23 (2004)
Digital Object Identifier (DOI) 10.1140/epjc/s2004-01762-0

Appendix

Volume 77B, number 3 PHYSICS LETTERS 14 August 1978

STOCHASTIC COOLING TESTS IN ICE

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Received 4 July 1978

ICE (for Initial Cooling Experiment) is a storage ring, built for testing both stochastic and electron cooling of proton beams in view of future application in a scheme for collecting antiprotons.

The ring, shown in fig. 1 is the transformed $g-2$ muon storage ring. Its main parameters are:

[1] Visitor from University of Bonn.

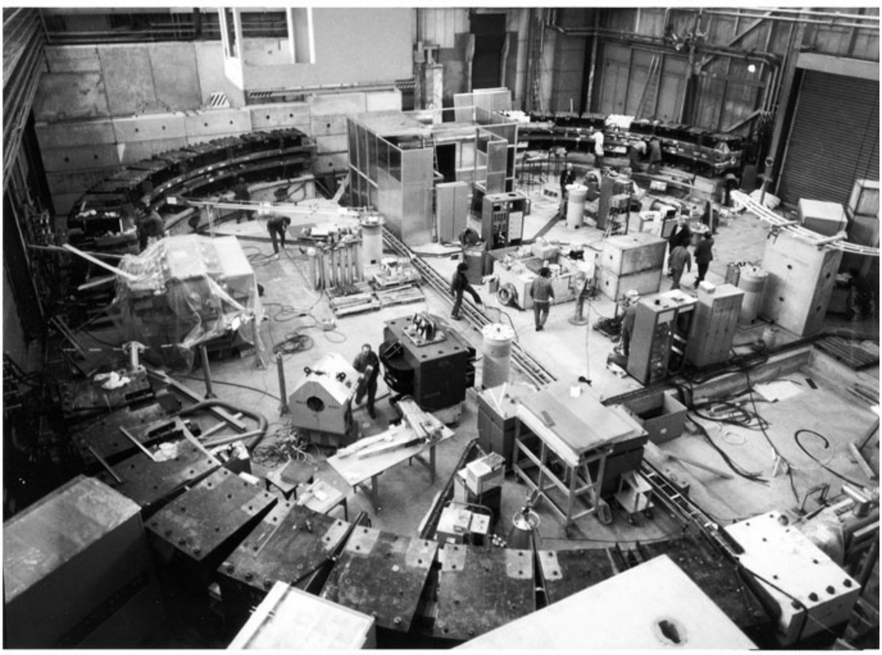

Fig. 1. General view of the ICE storage ring.

Dieter Haidt

The discovery of neutral currents

1 Prolog

It is a great honour for me to speak about the discovery of Weak Neutral Currents, the outstanding achievement, which has carried a high yield and assured CERN a place in the front row. The worldwide boost following the discovery is well known. What is perhaps less well known, are the difficulties this new effect had to overcome, before it got accepted by the community. In the 30 minutes allocated to me, I will try to elucidate some of the occurrences.

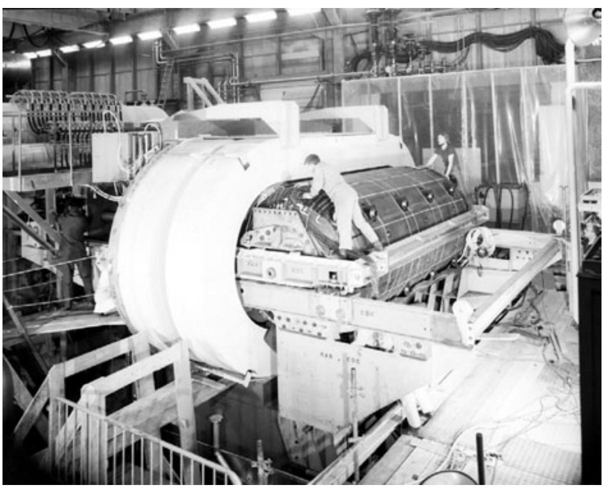

Fig. 1. The Gargamelle bubble chamber at the time of installation into the magnet coils

Shortly after the Siena Conference in 1963, Lagarrigue, Rousset and Musset worked out a proposal for a neutrino detector aiming at an increase in event rate by an order of magnitude. They had in mind a large heavy liquid bubble chamber and a large collaboration. When Leprince-Ringuet got to see the plans, he called the huge chamber *Gargamelle* (Fig. 1) invoking the mother's name of the giant *Gargantua* to pay homage to Rabelais. Lagarrigue formed gradually a strong and large collaboration built on two groups, one consisting of members from Orsay and the Ecole Polytechnique, the other consisting of members from the just finishing neutrino experiments with the NPA 1 m bubble chamber. At the end, the collaboration consisted of 7 European laboratories including guests from Japan, Russia and the United States. Figure 2 gives the list of authors[1] who signed the discovery paper [1][2].

2 The double challenge

At the end of the 50's weak interactions were well described by the $V - A$ theory. A major drawback was the bad high energy behavior and initiated various ideas to cure the problem of infinities. Guided by QED as a gauge theory, attempts were made during the 60's to construct a gauge theory of weak interactions [3]. The intermediate vector boson ($W^{\pm}$), although its existence was not yet known, was complemented with a neutral intermediate vector boson to achieve the required cancellations. The invention of the Higgs mechanism solved the problem of having a gauge theory and nevertheless massive mediators of weak interactions. The progress made by Glashow, Salam and Weinberg was completed by the work of Veltman and 't Hooft demonstrating the renormalizability of the theory. So, at the turn from 1971 to 1972 a viable theory of weak interactions claiming weak neutral currents as crucial ingredient was proposed and experiments were prompted to answer by *yes* or *no* whether weak neutral currents existed or not.

In fact, two neutrino experiments were running, the Gargamelle bubble chamber experiment at CERN and the HPWF counter experiment at NAL (now FNAL). Both were confronted with this challenge without preparation. The searches for neutral currents in the previous neutrino experiments resulted in discouraging upper limits and were interpreted in a way that the community believed in their non-existence and the experimentalists turned to the investigation of the copiously existing questions in the just opened field of accelerator neutrino physics. During the two-day meeting in November 1968 at Milan, where the Gargamelle collaboration discussed the future neutrino program, the expression *neutral current* was not even pronounced and, ironically, as seen from today, the search for neutral currents was an also-ran, low in the priority list and subsequently appearing in the neutrino proposal at place 8. The real highlight attracting the interest of all at the time was the exciting observation of the proton's substructure at SLAC provoking the question what structure would be revealed by the W in a neutrino experiment as opposed to the γ in ep-scattering.

[1] Further authors who signed only the publication of the *isolated electron* event are: H. Faissner, C. Baltay, M. Jaffré, J. Pinfold.

[2] The authors Lagarrigue, Musset, Rollier, Rousset and Schultze are deceased.

Volume 46B, number 1 PHYSICS LETTERS 3 September 1973

OBSERVATION OF NEUTRINO-LIKE INTERACTIONS WITHOUT MUON OR ELECTRON IN THE GARGAMELLE NEUTRINO EXPERIMENT

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Received 25 July 1973

Events induced by neutral particles and producing hadrons, but no muon or electron, have been observed in the CERN neutrino experiment. These events behave as expected if they arise from neutral current induced processes. The rates relative to the corresponding charged current processes are evaluated.

We have searched for the neutral current (NC) and charged current (CC) reactions:

$$\text{NC} \quad \nu_\mu/\bar{\nu}_\mu + \text{N} \to \nu_\mu/\bar{\nu}_\mu + \text{hadrons}, \tag{1}$$

$$\text{CC} \quad \nu_\mu/\bar{\nu}_\mu + \text{N} \to \mu^-/\mu^+ + \text{hadrons} \tag{2}$$

which are distinguished respectively by the absence of any possible muon, or the presence of one, and only one, possible muon. A small contamination of $\nu_e/\bar{\nu}_e$ exists in the $\nu_\mu/\bar{\nu}_\mu$ beams giving some CC events which are easily recognised by the e^-/e^+ signature. The analysis is based on 83 000 ν pictures and 207 000 $\bar{\nu}$ pictures taken at CERN in the Gargamelle bubble chamber filled with freon of density 1.5×10^3 kg/m^3 [*]. The dimensions of this chamber are such that most

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[*6] Also at University of Oxford.
[*7] Now at Rutherford High Energy Laboratory.
[*8] On leave of absence from University and INFN-Pavia.
[*9] Supported by Science Research Council grant.

[*] A more detailed account of the analysis of this experiment appears in a paper to be submitted to Nuclear Physics.

Fig. 2. Title page of the discovery paper [1]

At the beginning of 1971 everything was ready: the CERN PS [4], the neutrino beam line with horn and reflector followed by the decay channel and the neutrino shielding and, of course, the chamber itself. Also a well defined procedure for scanning and measuring was established. In order to have a reliable prediction of the neutrino flux a special run with the Allaby spectrometer was carried out. For several nuclear targets the secondary charged pion and kaon spectra were measured [5]. Furthermore, the neutrino shielding was interspersed with muon counters at various depths to monitor the muon flux [6] and so getting a constraint on the neutrino flux.

Even though the question of neutral currents had been ignored, Gargamelle could meet the challenge once it became a burning issue at the beginning of 1972. Benefitting from the experience of the previous neutrino experiment in the NPA bubble chamber a careful classification of event types has been set up for the scanning of the Gargamelle films. As a matter of fact, there was no muon identification, and there was no necessity for it, since neutrino interactions were supposed to always produce a final state muon. Consequently, charged hadrons do simulate a muon, as long as they leave the visible volume of the chamber without visible interaction. Events with a muon candidate were collected in the so called category A, while events consisting of secondaries identified as hadrons were collected in the so called category B. Moreover, there were three other categories, which however are not relevant for the present consideration. The category B events were thought to arise from undetected upstream neutrino interactions emitting a neutron and interacting in the chamber, and for that reason were called neutron stars (n^*'s). It was then easy to use these events to calculate the fraction which would not interact, thus simulating a muon, and to subtract them from the observed number of events in category A.

If indeed weak neutral currents existed, then they would induce events consisting of hadrons only, i.e. would be indistinguishable from those already in category B. This means that such events were just waiting among the already scanned events of category B and their investigation could be undertaken without any loss of time. The notorious problem of distinguishing neutrino-induced from neutron-induced events became then urgent. However, optimism was prevailing, since the much longer visible volume of Gargamelle compared to the NPA chamber increased the detection efficiency of charged particles as hadrons.

3 Euphoria in March 1973

The measurements of the inclusive neutral current candidates were carried out in the seven laboratories mainly between September 1972 and March 1973. In December 1972 an isolated electron was found at Aachen.

A little anecdote as passed down by Don Perkins [10] may illustrate the excitement. At the end of December 1972, Faissner together with Von Krogh left for Oxford. Still at the London airport Faissner was waving the event in his hand towards Perkins, who was waiting in the lobby. *"Is it in the neutrino or the antineutrino film?"*, was his only question. With "*antineutrino*" as an answer, they went happily to celebrate the event. In fact, the background level to isolated electrons in the antineutrino film was almost negligible and the interpretation of the event as elastic weak neutral current interaction on an electron [2] was most natural.

Inspired by this unique event the efforts to check carefully the far more complicated hadronic NC candidates went on vigorously. Figure 3 shows a neutral current candidate. A control sample of events with a muon candidate was prepared in parallel. In order to ensure a meaningful comparison the same criteria were applied to the hadron final state of both the charged current and neutral current candidates, which got dubbed CC and NC. A stringent cut in the total deposited hadron energy, $E_{had} >$ 1 GeV, was applied to keep the otherwise abundant number of n^*'s small. The sur-

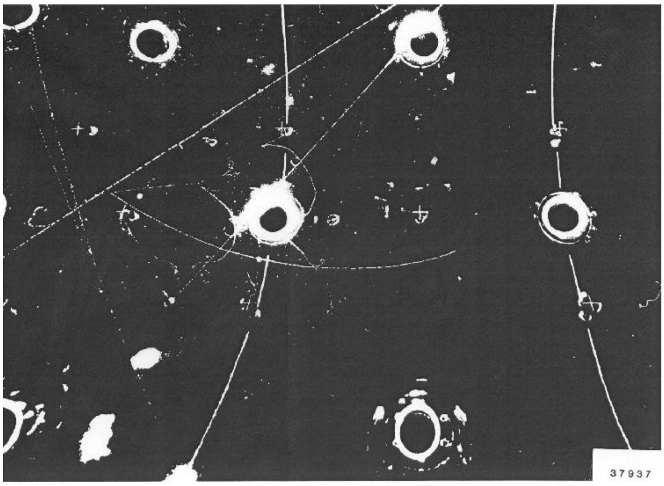

Fig. 3. Neutral Current candidate observed in Gargamelle: the neutrino beam enters from the left. Interpretation of the hadron final state: stopping proton and charged pion with charge exchange

prising result was the large number of NC candidates in comparison to the number of CC candidates, as seen in Table 1. Their spatial distributions are shown in Fig. 4. Both the event numbers and the spatial distributions were extensively discussed in the meeting mid March at CERN. There was no doubt that the only serious background to neutral currents consisted in neutron induced stars. Since their interaction length λ in the chamber liquid CF_3Br is about 70 cm, which is small compared to the longitudinal extension of the chamber, it seemed straightforward to check their presence by looking for an exponential fall-off in the vertex X-distribution. No such behavior was visible (Fig. 4). On the contrary, the X-distribution of NC candidates was rather flat and looked neutrino-like, as the CC candidates did. This was put in evidence by forming the NC/CC ratios of the spatial distributions, which in the years to come played such an important role. Evidently, it was well compatible with being flat both for the data in the neutrino and antineutrino films. Both arguments were corroborated by a Monte Carlo simulation of the ORSAY group based on the simplifying assumption that upstream neutrino-induced neutrons enter directly the

Table 1. The NC and CC event samples in the neutrino and antineutrino films

	Neutrino exposure	Antineutrino exposure
Number of NC candidates	102	64
Number of CC candidates	428	148

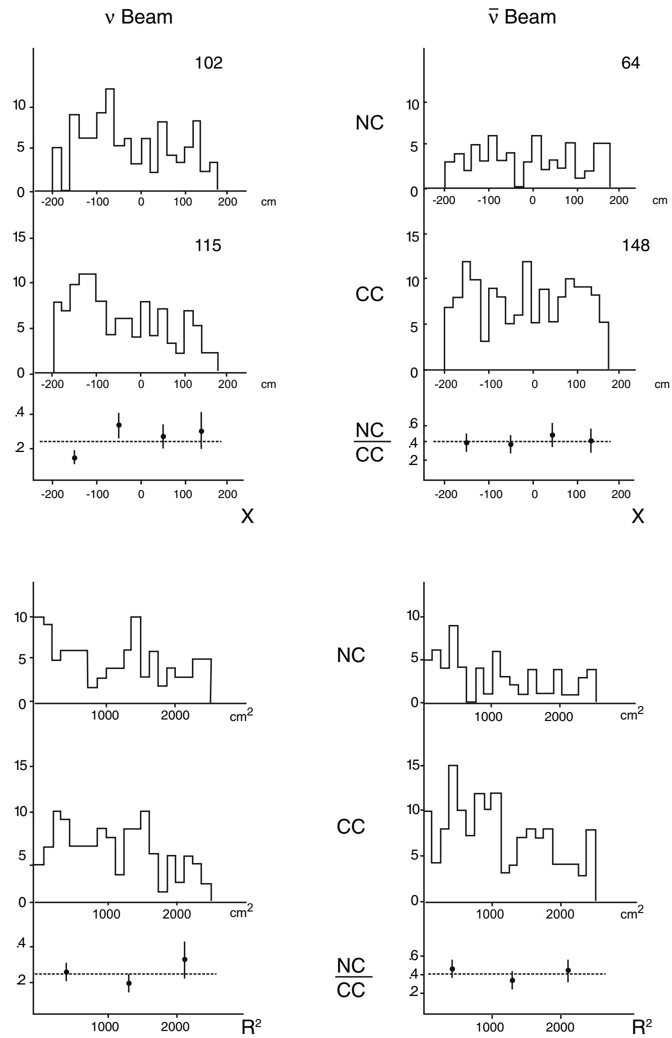

Fig. 4. Spatial distributions of the neutral and charged current candidates. X is the longitudinal vertex position of the events, R the radial position. Note: the numbers of CC candidates refer to the analysis of about a quarter of the available material

chamber along the neutrino direction. The excitement was therefore quite high and a discovery seemed at hand.

Yet, Fry and Haidt argued that the reasoning was not compelling. They brought up two strong arguments, which damped the euphoria.

Their first argument concerned the radial neutrino-flux distribution: it extends well beyond the chamber body and induces in the magnet coils a huge number of neutrino interactions, which in turn emit neutrons, thus generating a uniform flux entering sideways the fiducial volume. The net result is a flat X distribution also of n^*'s indistinguishable from neutrino-induced neutral current events.

The second, more dangerous argument concerned the fact that high-energy neutrons produce a *cascade*. Accordingly, neutrons may have had several cascade steps before entering the chamber. This meant that the relevant measure of the number of background neutrons was therefore not governed by the interaction length λ_i, but rather by the longer and energy dependent cascade length λ_C. The net result is a considerably larger n^* background than anticipated.

In this situation there was only one way out, namely to produce evidence that the number of neutron-induced events is small compared to the observed number of NC candidates despite the two new arguments.

4 The proof

The following months were characterized by feverish activity. An ambitious and detailed program was set up and carried through [9, 11]. The ingredients, which had to be taken into account, were:

- geometry and matter distribution of the whole setup,
- neutrino flux as function of energy and radius,
- dynamics of the hadron final state.

It was straightforward to describe accurately the experimental setup with the chamber, its corpus and the interior consisting of fiducial, visible, non-visible volumes, the surrounding coils and the shielding in front of the chamber. The neutrino flux $\Phi(E,R)$ was well understood, since it relied on the direct measurement of the parent distributions and the measured muon flux [6] at various depths and radial positions in the shielding [5]. On the contrary, the description of the complex final state of a neutrino interaction appeared as an insurmountable task given the short time available. It would have implied to predict for each neutrino-induced topology the tracking of all final state particles including in addition all the possible branchings. The breakthrough to a solution came from the consideration that π- or K-induced interactions never give rise to secondary neutrons, which would still be energetic enough to fake a NC candidate. The problem was then reduced to controlling the behavior of final state nucleons, i.e. protons or neutrons. Since the neutrino energy spectrum extended up to about 10 GeV, the generated nucleons can be fast and indeed propagate over several steps. However, the kinematics of nucleon–nucleon [*NN*] interactions is such that at each step there is at best *one* secondary nucleon able to continue the cascade and still have enough energy that at the end it is a neutron, which enters the chamber and deposits more than 1 GeV. With this considerable simplification the problem boiled down to establishing the nucleon elasticity distribution at each cascade step. There were plenty of NN data to derive the required distribution.

A neutrino event emitting a neutron can appear in two topologies, called *AS* and *B* events. Figure 5 shows on top an *associated* event (*AS*), where both the neutrino interaction and the downstream neutron star are visible in the chamber. On the other hand, *background* events (*B*) sketched below, are produced when the neutrino

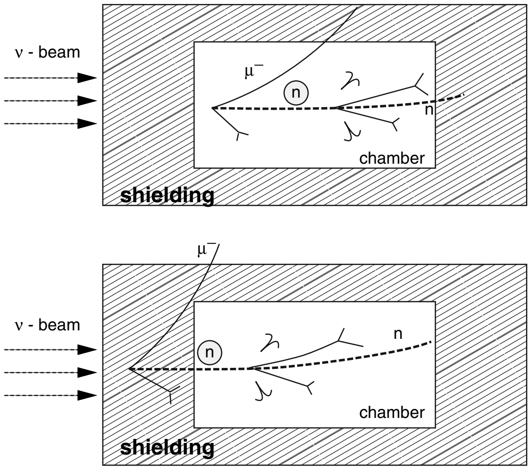

Fig. 5. Two topologies of a neutron cascade. *Above*: associated event (AS), *below*: background event (B)

interaction occurs in the invisible upstream shielding and the emitted fast nucleon cascade eventually ends up in a neutron entering the chamber and depositing enough energy to fake a NC candidate. It is important to note, that the two topologies probe different parts of the nucleon cascade: in AS events the *beginning* of the neutron cascade is directly observed, while in *B* events the observed n^* represents the *end* of the nucleon cascade and therefore depends on the kinematics of the whole upstream cascade, which cannot be inspected.

The strategy consisted then in combining the relation between the two topologies and the observed number of *AS* events (n_{AS}):

$$n_{\text{B}} = \frac{B}{AS} \times n_{\text{AS}} \ .$$

The number of background events (n_{B}) is obtained from the observed number of *AS* events and the ratio B/AS calculated with the cascade program. Since B/AS is a ratio, several systematic effects cancel out or are at least reduced. The really critical aspect in calculating B/AS concerned the treatment of the cascade. Also this aspect was under control, since it was based on data from *pp* and *pA* experiments carried out in the few-GeV region.

At the beginning of July 1973 the neutron background program was complete. It had no free parameters, was flexible and very fast. All sensitive parameters could be easily accessed and varied. All imaginable questions and worries raised from within the collaboration could be investigated and answered quantitatively and unambiguosly.

The most elegant argument consisted in testing the hypothesis that *all NC candidates are background events*. According to this worst-case hypothesis one has: $n_{\text{B}} = n_{NC}$. Consequently, the ratio B/AS would be equal to the ratio of the observed numbers of NC and AS events, i.e. 102/15 in the neutrino film and 63/12 in the antineutrino film (see Table 1). The angular and energy distributions are readily derived from the NC samples, which are neutron stars by hypothesis, and have the form

$$\frac{\mathrm{d}N}{\mathrm{d}E} \sim E^{-n}; \quad \frac{\mathrm{d}N}{\mathrm{d}\cos\theta} \sim \mathrm{e}^{-\frac{\theta^2}{2\theta_0^2}}$$

For $n = 1.1 \pm 0.1$ and $\theta_0 = 0.35 \pm 0.05$ agreement with the event sample was obtained. With this as input to the cascade program the calculated ratio B/AS resulted in 1.0 ± 0.3 in blatant contradiction to the hypothesis 102/15 and 63/12. Thus the hypothesis must be rejected and the neutron background does not dominate the NC candidates. This argument found immediate approval.

Putting in the experimental best values the prediction for the ratio B/AS was 0.7 $\pm$ 0.3. With this value the predicted neutron background was indeed small compared to the observed number of NC candidates, thus a new effect could be safely claimed and published in *Physics Letters* at the end of July. Thus ended the hot months, but a dramatic after-play was to come.

There was also another approach. Pullia [12] applied the Bartlett method to the spatial distributions. For each event, it was assumed that the interaction was induced along the direction of the total 3-momentum of the observed hadron system. Then for each event two quantities can be measured: the actual flight path l and the potential flight path L providing the probability

$$\frac{1 - \mathrm{e}^{-\frac{l}{\lambda}}}{1 - \mathrm{e}^{-\frac{L}{\lambda}}}\,.$$

A maximum likelihood analysis yielded the apparent interaction length λ. Figure 6 [7] shows, at 90% confidence level, that the result for the NC sample was $\lambda^{NC} = 2.2\,\mathrm{m}$ to be compared with the slightly larger value $\lambda^{CC} = 2.7\,\mathrm{m}$ in the CC sample. This was also evidence for the NC sample not to be dominated by neutron stars.

Furthermore, handy formulae for estimating the neutron background were obtained by Perkins [10] based on the attenuation length of neutrons and by Rousset [13] based on an equilibrium argument. They were useful, though qualitative, since the experimental conditions were considerably simplified.

5 Attack and final victory

The new results were reported at the Electron–Photon Conference one month later at Bonn together with the results of the HPWF experiment. C.N. Yang announced at the end of the conference the existence of weak neutral currents as the highlight of the conference.

There was no time for celebrating the great achievement. On the contrary, a painful time of defense against unjustified attacks started. Shortly after the Bonn Conference, the HPWF Collaboration modified their apparatus with the net result that the previously observed signal of neutral currents disappeared. These news quickly reached CERN. They caused dismay and were reason for distrust in the Gargamelle result. The opponents focused their criticism on the neutron background calculation

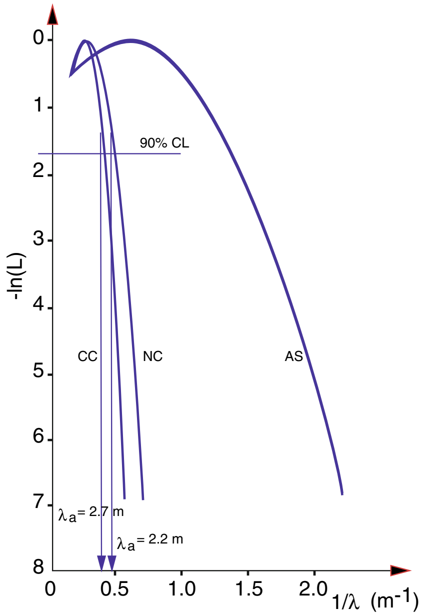

Fig. 6. Bartlett analysis of NC and CC events

and in particular on the treatment of the neutron cascade λ_C. Although the members of the Gargamelle Collaboration withstood all critical questions, the willingness to accept the validity of the Gargamelle observation had to wait until the end of the year. In a special run Gargamelle (filled with the same liquid CF_3Br) was exposed to shots of protons with fixed momentum of 4, 7, 12 and 19 GeV. In order to exclude any escape, the background program was applied to predict in advance the proton induced neutron cascade length versus initial momentum. Figure 7 shows a prominent example of a multi-step cascade. The four exposures were quickly evaluated by Rousset, Pomello, Pattison and Haidt. The final results were reported at the APS Conference in April 1974 [14] in Washington. The overlay of the predicted and measured cascade length (Fig. 8) resolved all doubts.

One year after the discovery, at the time of the June 1974 London Conference, overwhelming confirmation for the existence of weak neutral currents came from Gargamelle itself [7] with twice the original statistics. In the meantime the HPWF Collaboration had elucidated the reason why they lost the signal and now also affirmed weak neutral currents. Further confirmation came from the new counter experiment of the CITF Collaboration and from the observation neutrino-induced single pion events without muon in the 12 ft ANL bubble chamber.

6 Epilog

In retrospect the significance of the observation of weak neutral currents is highly visible. It is the key element in giving substance to the similarity in structure of

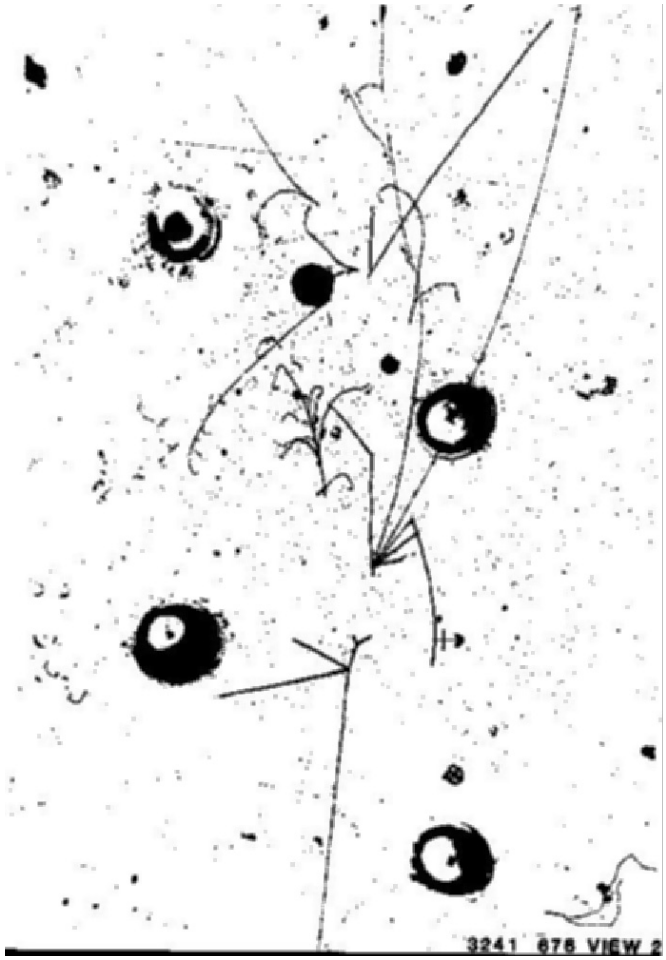

Fig. 7. A proton of 7 GeV enters Gargamelle from below and induces a three-step neutron cascade

weak and electromagnetic interactions. Rightly the new term *electroweak* came into circulation.

The discovery of weak neutral currents crowned the long range neutrino program initiated by CERN at the beginning of the 60's and brought CERN a leading role in the field. The new effect marked the experimental beginning of *the Standard Model of electroweak interactions* and triggered a huge activity at CERN and all over the world, both experimentally and theoretically.

The most immediate success was the prediction of the mass value of the elusive intermediate vector boson W on the basis of the Glashow–Salam–Weinberg model

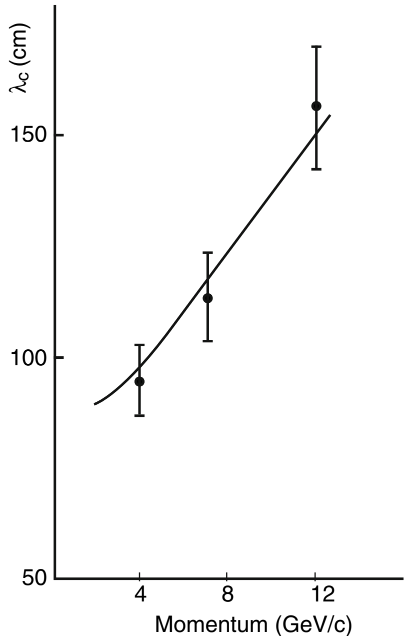

Fig. 8. The measured and predicted cascade lengths

combined with the first measurements of the weak mixing angle θ_W, namely

$$M_W = \sqrt{\frac{\pi\alpha}{\sqrt{2}G_F}}\frac{1}{\sin\theta_W} = \frac{37\ \text{GeV}}{\sin\theta_W} \approx 70\ \text{GeV}\ .$$

The large value made it evident that neutrino experiments had no chance to observe the W propagator effect. This led to the idea to produce W's in high-energy $\bar{p}p$ collisions. The transformation of the CERN SPS into the S$\bar{p}p$S collider succeeded in the observation of the mediators of the weak force, the W and Z [8].

The neutrino experiments at the CERN SPS increased in accuracy to the extent that the first test of electroweak radiative corrections was made possible by comparing the directly observed W mass with the one obtained by GSW putting in the precisely measured weak angle θ_W. In the limited time available in this talk only a summary [15] of low energy experiments is presented in Fig. 9. All low energy neutral current experiments can be displayed in a plane spanned by two effective charge couplings [15] $\bar{s}^2$ and $\bar{g}_Z^2$, which are related to $\sin^2\theta_W$ and the overall neutral current strength. The ellipse marked νq combines the results from 41 neutrino experiments. Also included in the figure are the results from the elegant *ed* experiment at SLAC, the clean νe data and results from atomic parity violating experiments. All low energy data agree well, as is evident from the thick ellipse representing the result of the combined fit.

The continuously improved knowledge on weak interactions justified building the e^+e^- collider LEP for an in-depth study of the Z decay parameters and later WW production allowing stringent tests of the electroweak theory at the quantum

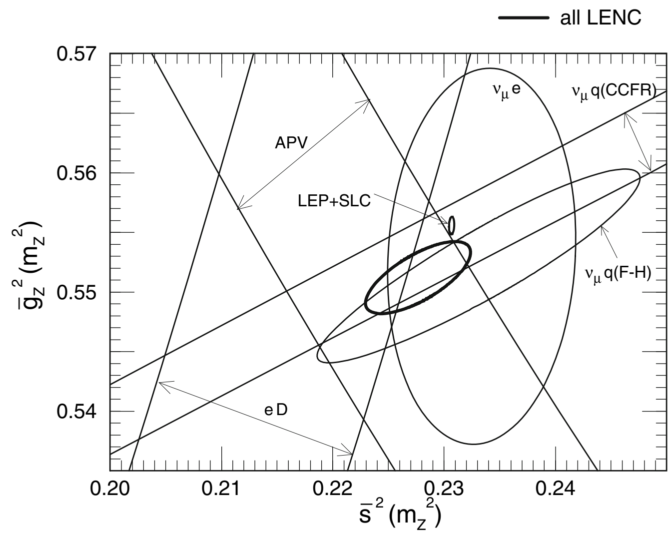

Fig. 9. Summary of four classes of low energy neutral current experiments. The effective charge parameters are determined from the data for $Q^2 \sim 0$ and then propagated to the Z-mass scale for comparison with the LEP and SLC data

level [16]. All results combined make the search for the Higgs, the last element of the electroweak Standard Model, a central issue for the Large Hadron Collider, which is presently under construction.

I would like to end this talk on a personal note. I had the privilege to be a member of the excellent Gargamelle Collaboration, to contribute to the discovery and to feel the responsibility – it was an experience for life.

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First published in Eur. Phys. J. C 34, 25–31 (2004)
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Pierre Darriulat

The discovery of the W & Z, a personal recollection

1 Preamble

It is such a pleasure to be back at CERN after four years of absence, on such a happy occasion, and to be able to meet again so many dear friends. I am very grateful to Luciano Maiani and to those who helped him with the organization of the event for having given me such an opportunity.

At the occasion of the twentieth anniversary of the W/Z discovery several articles have appeared in the press. Some are excellent, as that of Daniel Denegri, a former member of the UA1 Collaboration who reminds us in the CERN Courier of the spirit of discovery in those times. Unfortunately some others are mediocre, as the piece of gossip taken from Gary Taubes' *Nobel Dreams*, published and endorsed by Physics World in January. Such an article does no service to the history of science, it only retains a collection of anecdotes selected for their ability to seduce the general public, but this is not what history is made of. As a result it gives a completely distorted and misleading account of what had been going on. Worse, it makes no service to science by mistaking research for a horse race and scientists for bookmakers. The author, who had spent a few months with UA1, reminds me of the kid who was taken to the theatre to see a Shakespeare's play and who only remembered the shining uniform of the fireman on duty at the emergency exit without having grasped a single word of what was going on stage.

Each of us remembers only part of the story and our memories are always biased, whatever effort we devote to giving them documented support. We saw what was then the present through our own eyes and such are the images that we try later on to recall from our memories in order to reconstruct the past. What looked important to us was largely dependent on what we knew and on what we were unaware of at the time when it occurred. It is the work of the historian to put these various recollections together and to try to make a sensible story out of that material. I hope that this personal recollection can be used by him as a useful testimony of those times.

I have selected some topics among those that have been most grossly distorted by accounts such as that published in Physics World.

2 An announced discovery

The decade between 1967 and 1976 witnessed an impressive sequence of experimental and theoretical discoveries that have changed the vision we had of the world. To list just a few of the main milestones I may quote the prediction of electroweak unification in the lepton sector (Weinberg and Salam 67–68), the discovery of deep inelastic electron scattering at SLAC (69) immediately followed by the parton ideas and models (Feynman, Bjorken), the prediction of charm (Glashow–Illiopoulos–Maiani 70), the proof of the renormalizability of spontaneously broken gauge theories (‘t Hooft 71), electroweak unification in the hadron sector (Weinberg 72), the discovery of neutral currents (Gargamelle 73), asymptotic freedom and QCD (Gross–Wilczek–Politzer and Gell-Mann–Fritsch–Leutwyler 73), the measurement of R at SLAC in electron–positron annihilations and the J/ψ discovery (74) followed in 76 by the discovery of naked charm (again at SLAC).

In 1976 the Standard Model was already there, ready to confront experiments, and it was clear that a new accelerator was required to explore the electroweak unification sector where the weak gauge bosons, W and Z, were expected with approximate masses of 65 and 80 GeV/c^2 respectively: the arguments for LEP were present and strong (Fig. 1). I remember having been asked by John Adams to convene the LEP study group in April 1976 and to edit the report. In practice it meant listening and learning from John Ellis and Mary K. Gaillard all the beautiful new physics that was waiting for us, putting together some documents on the feasibility of the machine

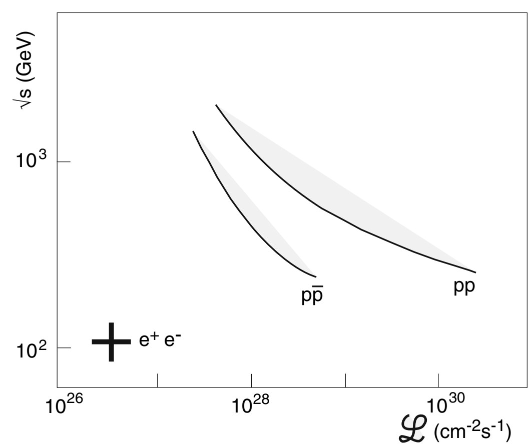

Fig. 1. Energy-luminosity requirements for the production and detection of 10 Z^0 (e^+e^-, $\mu^+\mu^-$) per year (from [4], page 758)

that were available following Burt Richter's seminal paper, and wrap it all up as fast as possible together with some bread and butter experimental comments. It took only seven months to get it all done, to the satisfaction of John Adams who wanted to push the LEP project in the wake of the success of the SPS that was just due to start operation at that time.

Moreover, it is worth recalling that supersymmetry blossomed between 1971 and 1974 and that, in principle at least, the arguments for LHC, a machine to explore the Higgs and low mass SUSY sectors, were also there. In practice, however, it took another few years before they could be expressed with some clarity.

3 The proton–antiproton choice

The 1976 situation sets the context in which the proton–antiproton decision was made.

The pressure to discover the W and Z was so strong that the long design, development and construction time of the LEP project left most of us, even the most patient among us, unsatisfied. A quick (and hopefully not dirty) look at the new bosons would have been highly welcome. But when pp colliders such as MISR or SCISR were proposed in this spirit, they got killed in the egg by the management with the argument that they would at least delay, or even worse, endanger the LEP project. This was accepted as a serious argument even by the proponents of such colliders. I remember having preached for SCISR, together with other ISR colleagues and with Maurice Jacob as our spokesman, and having been sent packing dryly by John Adams and Leon van Hove. They found it improper and somewhat irresponsible to make any noise that might divert CERN from the LEP party line and I must confess that I thought that, after all, they were right.

The same argument did not apply to the proton–antiproton collider that was not requiring the construction of a new collider ring and could be proposed as an experiment. One might object that this sounds like a bad joke because it implied the construction of an antiproton source that turned out later to include a collector/accumulator complex (AA/AC), but it remains true that the existence of the SPS, that was soon shown to perform extremely well, has obviously been an essential element of the success of the proton–antiproton project, not enough acknowledged in my opinion, and for which John Adams has to be credited. It is also true that John Adams found it difficult to swallow that his newborn baby should be pottered about with at such a young age and turned into a collider that had only little chance to work. This was indeed the feeling of the vast majority of machine experts at the time and much of the merit of Carlo Rubbia is to have pushed his ideas with such an untiring determination and in such an adverse context. Not only with determination but also with a clear vision of what they turned out to lead to and with a deep understanding of the machine physics issues at stake.

But another argument made it possible for the proton–antiproton project to break the LEP taboo. Most likely, if CERN hadn't bought Carlo's idea, he would have sold it to Fermilab. This threat was clear and had a very strong weight in the taking of the decision. In spite of the fact that the Fermilab machine was not performing well enough at the time to be used as a proton–antiproton collider, it very effectively accelerated the well known sequence of events that followed the publication of the 1976 paper by Rubbia, McIntyre and Cline. In 1977, after the proposal had been made to CERN and Fermilab to produce W/Z with existing machines, a feasibility study was undertaken by Bonaudi, van der Meer and Pope that led to the AA design, a detector study was initiated under Carlo that led to the UA1 design and the Initial Cooling Experiment (ICE) was proposed to the SPSC. Its success was demonstrated in June 1978 and the UA1 approval followed immediately. Only six months later was UA2 also approved.

It is very difficult to rewrite history, all events are so intricately linked to each other, but I strongly believe that, if it had not been for Carlo, there would have been no proton–antiproton collider physics in the world for a long time, maybe ever. Whether the weak bosons would have been discovered at LEP or at SLC or at some kind of a CBA is another matter, but it would have taken another six years at least. One might argue that six years is not that much after all, but the top quark would not have been discovered either (other than indirectly from radiative corrections at LEP) nor would

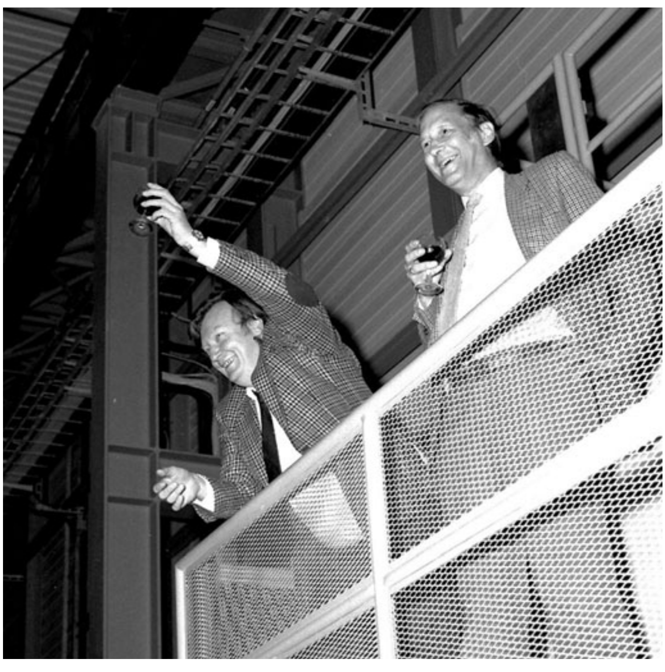

Fig. 2. Photograph of Carlo and Simon celebrating their Nobel Prize (Reference 523-10.84 from the CERN collection). As soon as it became known that the 1984 Nobel Prize was awarded to Carlo Rubbia and Simon van der Meer a celebration was organized in a CERN experimental hall, at LSS5. The happiness that they radiate was shared by the crowd of participants to the proton–antiproton project who attended the event and drank a glass in their honour. Undoubtedly, this has been one of the happiest days in the CERN history, maybe the happiest

we have learnt from the vast and rich amount of strong and electroweak physics data that have been collected at the SPS and Tevatron colliders. Not to mention the low energy LEAR physics, antihydrogen, glueballs, CP violation, antiprotonic helium atoms, etc. If the Nobel Committee were to rewrite today the caption of the 1984 award to Rubbia and van der Meer (Fig. 2), they would undoubtedly say something like “for their decisive contributions to the large projects which led to the discovery of the field particles W and Z, communicators of the weak interaction, to the discovery of a sixth quark, the heaviest of all particles known to us today, to the exploration of the strong and electroweak interactions up to masses approaching the electroweak symmetry breaking mass scale, to the identification of new mesons such as glueballs and hybrids and to remarkable advances in atomic physics.” I am fully aware that there is some irony to credit Carlo for contributions to the discovery of the top quark when one remembers some well known UA1 hiccups on that chapter, but I do mean what I just said.

4 Physics in the limelight and physics in the shade

Gossip only knows about what was going on in the limelight but history should also learn about what happened in the shade. Lacking such knowledge leads to oversimplifications and to distortions of the truth.

Such an oversimplification is the statement that before the W/Z discovery “CERN had been losing out on big discoveries to less conservative labs”. It took a quarter of a century for Europe to reconstruct fundamental research after World War II. It has been a long and painful process that required tremendous efforts of many outstanding people. Learning about that history is both fascinating and extremely instructive. Those who take today too lightly actions that are detrimental to research and to science should learn how harmful they may be from the lessons of the history of this revival. Sentences such as the one I just quoted make so little of that history that they give a completely false account of the reality.

I do not mean to recall here the discovery of neutral currents in Gargamelle, this has just been done brilliantly by Dieter Haidt, but to say a word about the CERN Intersecting Storage Rings and the seminal role that they have been playing in the success of the proton–antiproton project. The ISR was the first hadron collider ever built in the world, the machine on which the young generation of machine physicists who designed, built and operated the antiproton source and the proton–antiproton collider (and later on, may be to a lesser extent, LEP) had got their hands in, had learned their experience and gained their expertise. It worked superbly, exceeding its design goals in both energy and luminosity. It is the machine on which van der Meer’s ideas on stochastic cooling were tried for the first time, where they have been studied and understood. It is also the machine where a generation of physicists learned how to design experiments on hadron colliders. When the first ISR experiments were being designed the strong interaction was still a complete mystery, when the machine was finally shut down Quantum Chromo Dynamics was there. I do not mean to say that it is ISR physics that has taught us about QCD, but it has contributed to the development of several of its ideas and it has helped us greatly in drawing a clear picture of hadron collisions without which we would not have been able to design so effectively the UA, CDF and D0 experiments. A picture in which the soft $\log s$ physics and the hard parton interactions were separately described in simple terms. We, in UA2, were particularly indebted to the ISR where many of us had been previously working and for whom this experience had been an essential asset in designing a good detector.

It is not always clear what makes the spots of the limelight point to this physics rather than to that other. There is no doubt that they did point to the W/Z discovery that rightly appeared to be as emblematic of the progress of the new physics as had the J/Ψ discovery eight years earlier. In principle, there is no less beauty in QCD than in SU(2)$\times$U(1) but one cannot name such an emblematic experiment in the strong interaction sector. Yet, from the deep inelastic electron scattering experiments at SLAC in 1969 to the studies performed at LEP of quark–antiquark and quark–antiquark–gluon(s) final states, there has been a quarter of a century during which the strong interaction theory and experiments have progressed hand in hand to a state of near perfection. Incidentally, I take this opportunity to express my admiration of an experiment that had been running in the shade of the proton–antiproton project just before the UA2 detector had been rolled into the ring, the streamer chamber experiment UA5 that, despite the very short data taking time that was made available to it, succeeded in giving experimental $\log s$ physics much of its most important results.

I should not like to close this chapter without recalling the extraordinary concentration of outstanding talents that the proton–antiproton project succeeded to attract. One reason was of course that between the SPS and LEP projects, one completed and the other still in the egg, its timing was in some sense ideal. But the other reason, possibly more important, was the challenging nature of the project that was proper to attract to it extremely bright engineers and physicists, both machine physicists and particle physicists. The challenge of designing, constructing and assembling the antiproton source and the detectors, and of getting them to work in such a short time, was enormous. As was that of digging and equipping the large experimental halls that were required for housing the new detectors that had to be alternately rolled in and out between collider and fixed target periods. As was that of making the transformations implied by the operation of the SPS as a collider. The amount of ingenuity that went into all these achievements was truly outstanding. My best memory of those times may indeed be the good fortune it was for me to work with so many talents, and, in the particular case of UA2, to enjoy collaborating with such bright colleagues, senior physicists, postdocs, students or physicists of the same generation as mine. CERN as an institution, and more generally the whole European particle physics community, were rightly proud of the success of the proton–antiproton project: it had indeed been the result of a very coherent and efficient collective effort.

5 The UA1/UA2 competition

In presenting the W/Z discovery as a race between UA1 and UA2 Taubes has shown that he did not understand well what had really been going on. There had been a race indeed, but it was at a higher level, between Europe, with CERN, and the United

States, with Fermilab and SLAC. No doubt, the competition between UA1 and UA2 was real and lively, but it was relatively unimportant in comparison, it was anecdotic rather than historic, it was more a kind of a game, and we had a lot of fun in playing it.

There was no doubt that Carlo was the king of the proton–antiproton kingdom and was recognised as such by all of us. Undoubtedly, he would have had to take the blame if the proton–antiproton project had been a failure, but as it turned out to be a success he deserved to take the fame. Personally, I had been working in Carlo's group for six years or so, mostly on K physics, I had joined him as a postdoc in the mid sixties, coming from nuclear physics, and I had learned from him the bases of experimental particle physics. I had always been impressed by his brightness, by the readiness of his mind and by his far-reaching vision and I respected him then, as I do today, as someone of a clearly outstanding stature. To respect him as the king did not mean to belong to his courtship and we in UA2 were particularly keen at detecting occasions on which we could proclaim that the king was naked. Such occasions were very rare, the king was usually dressed splendidly, so they were the more enjoyable.

UA2 had been approved in order to create a competition to UA1 that was meant to provide a constructive and coherent emulation, and it served that purpose very well. We usually enjoyed a very friendly, helpful and even sometimes protective attitude of the management during the design and construction period, in particular from the research and accelerator directors, Paul Falk Vairant, Sergio Fubini, Erwin Gabathuler and Franco Bonaudi. Most of the time the management had the elegance to treat UA1 and UA2 on an equal footing, or at least to pretend to do so, and we were thankful to them for playing that game. There have been instances when the management did not have this elegance, I remember in particular having been called to the office of van Hove, together with Luigi Di Lella and Jean-Marc Gaillard, to pass a kind of examination before UA2 was approved (and therefore Sam Ting's proposal rejected). Van Hove wanted to check that we were not clowns. I also remember, the day when Carlo gave his W seminar at CERN in January 1983, namely the day before Luigi gave the UA2 seminar, to have found a routing slip on my desk from "the other" director of research ("our" director was out of CERN) stating that "if UA2 had anything to say that would contradict the statements made by Carlo, you should come and tell me beforehand". Clearly he did not care a damn about what we had to say, what mattered to him was only that we should not mess around and spoil the beauty of the UA1 results. Such inelegances were rare but were cruel to the collective self-respect of the members of UA2. Much more cruel than the tricks that UA1 may have been playing on us and that we were accepting as being part of the game. Indeed Rubbia himself has always considered UA2 with much respect, starting from the time when the experiment was being proposed. And the relations between the members of the two collaborations have always been excellent. The members of each collaboration were usually having several old (or less old) friends in the other and the senior members of both collaborations paid much attention to maintain this friendly atmosphere. We all were very indebted to Alan Astbury for having played a particularly constructive role in this respect.

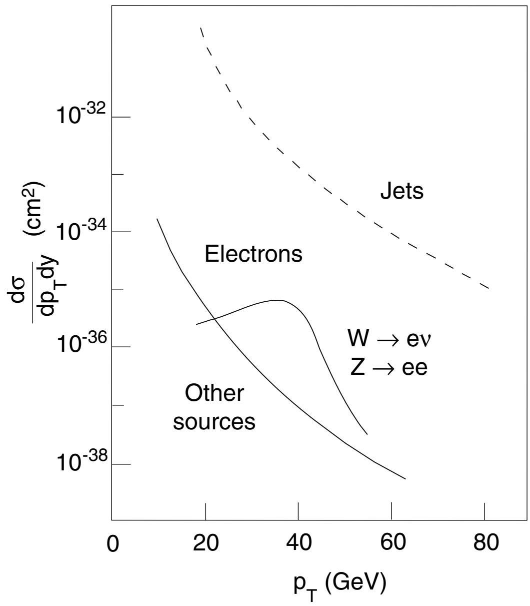

Fig. 3. The cross-section for the production of electrons from W and Z decays and from other sources is compared to the jet cross-section. A clear W/Z signal could be expected as long as the misidentification of hadronic jet faking a lepton could be kept below the 10^{-4} level. The bulk of the total cross-section, ~ 60 mb, 7 orders of magnitude above the W/Z cross-section, was easily eliminated at trigger level on the basis of the transverse energy deposition. In both UA1 and UA2 electron identification relied on the observation of a track having a good match to a calorimeter energy cluster, both track and cluster exhibiting features characteristic of an electron (from [4], page 760)

The design of the UA2 detector had been a success and its construction and running-in went extremely smoothly. We were rightly proud of it. For a cost that was only one third of the UA1 cost (a condition to our approval was that the cost should be significantly lower than the UA1 cost) we managed to build a detector that was ready on time, that saw the W and Z as soon as the collider luminosity made it possible (and at the same time as UA1 did), that measured the W and Z masses more accurately than UA1 did and that was better than UA1 at detecting and measuring hadron jets. It was easier to design UA2 than UA1 because UA2 did not have to be a multi-purpose detector and could afford to simply ignore some of the physics, in particular to be blind to muons. The main asset of the UA1 detector was its central detector, that of UA2 was its calorimetry (Figs. 3 to 6).

A difficulty in making the right design had been to have a good judgement of how well the machine would perform, how long it would be to take off, how noisy and hostile an experimental environment had to be expected. Sam Ting's detector could have run in almost any background conditions but could only see muons, the UA1 central detector was requiring very clean conditions, UA2 was somewhere in

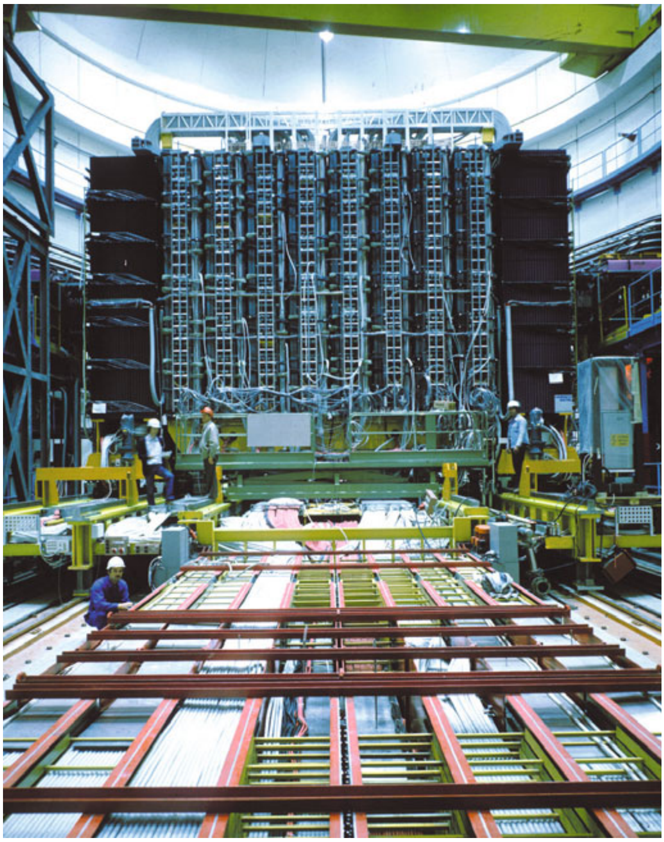

Fig. 4. Photograph of the UA1 detector (CERN reference X.595.04.81). The UA1 detector, shown here in its garage position, was a multi-purpose detector. It covered as large as possible a solid angle and was able to detect hadron jets, electrons and muons. This universality had been obtained at the price of compromises on the performance of its individual components: The 0.7 T dipole magnetic field was generated by a shoe-box magnet segmented for hadron calorimetry, electromagnetic calorimetry was made in semi-circular lead-scintillator sandwiches (the "gondolas") surrounding the central detector

between. The collider turned out to be an exceedingly clean machine and we had grossly underestimated how fast its luminosity would increase. In particular we had left an open wedge in our calorimeter, instrumented with a magnetic spectrometer, to do quietly, so we thought, some exploratory measurements while the machine would be being tuned and run in. The wedge did not stay open very long, the performance of the machine was progressing at high speed, and we were able to tackle the first high luminosity run with full calorimetric coverage.

It is sometimes said that UA1 was better than UA2 at detecting neutrinos. I do not think that this is true. What is certainly true is that UA1 did put much emphasis (and rightly so) on the virtue of using momentum imbalance as a W signature. But both

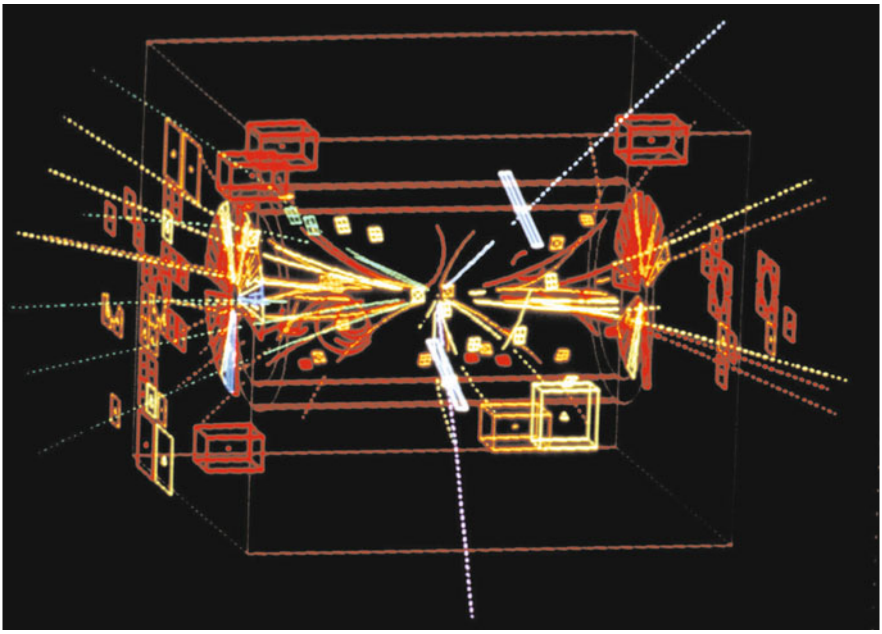

Fig. 5. Photograph of a UA1 Z^0 event in the electron channel (Reference X542.11.83 from the CERN collection). The main asset of the UA1 detector was a large volume, high-resolution central tracking detector, of an original and high performance design. It made it possible for UA1 to detect muons and tau mesons, to make precise checks of lepton universality and of the $V - A$ nature of the W-coupling, to detect muons in the vicinity of hadron jets, giving early evidence for BBbar mixing

UA1 and UA2 were well aware of the importance of measuring the lack of transverse energy balance in order to reveal the presence of neutrinos, this is beyond any doubt. It was indeed the main issue at stake in the SPSC discussion of the UA2 proposal that followed a DESY note written by Branson and Newman where they were ignoring what we were calling "background rejection by p_T balance". Moreover, ideas about neutrino detection from lack of p_T balance had been in the air for a long time and the 1976 report of the LEP study group (that I mentioned earlier) was already giving them due consideration (of course in the easier environment of an e^+e^- collider, but the idea was the same). What was not known was exactly how much the underlying soft secondaries would smear out the measurement accuracy of the p_T balance (very little it turned out to be). Moreover it took us some time to digest QCD and to realise that the W and Z (and for that matter any high mass structure in the final state) could be produced with large transverse momenta: the UA2 proposal had been written with the assumption of a Gaussian distribution, 1.5 GeV/c on average, therefore strongly damping the power law tail predicted by QCD. But being aware of the importance of a good neutrino detection was not sufficient. In fact both UA1 and UA2 were mediocre in terms of hermeticity. UA2 was suffering from a lack of coverage at small angles and

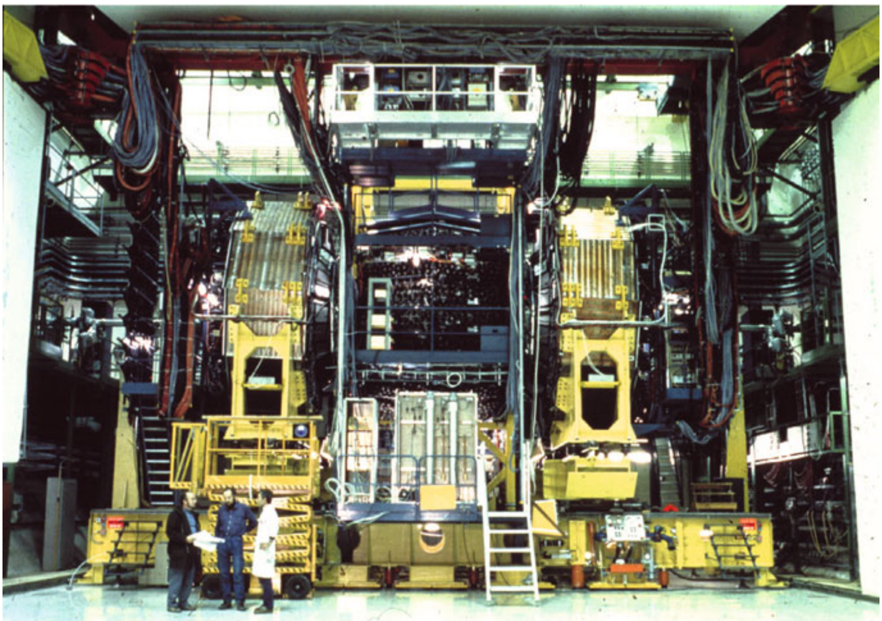

Fig. 6. Photograph of the UA2 detector (Reference X.559.3.83 from the CERN collection). The UA2 detector had a more limited scope than the UA1 detector: it could detect electrons but not muons, it focussed on the central rapidity region, it could not measure particle charges except for limited regions where the W decay asymmetry was maximal. But what it could do, it did better than UA1. It provided the most accurate measurements of the W and Z masses and its excellent jet detection capability, as illustrated by the identification of W/Z decays into two jets, gave important contributions to jet physics and to the study of the strong interaction sector. Its main asset was the fine granularity and projective geometry of its calorimeter design, with segmentation perfectly matched to the job. Tracking in the central region was done efficiently in a very limited space around the beam pipe

UA1 from imperfections of the central calorimeters (gaps, insufficient segmentation and non projective geometry). In practice however, both experiments were hermetic enough for detecting in excellent background conditions weak bosons produced with not too high a transverse momentum and both UA1 and UA2 did it very well, each making optimal use of the background rejection power of the p_T imbalance signature of W production. But it became insufficient in studies of "monojet" events as UA1 called them, or when searching for the top quark, and the main purpose of the upgrades (Fig. 7) that both UA2 and UA1 proposed after two years or so of operation was to improve hermeticity (the UA1 upgrade never got implemented).

I do not wish to repeat here the often told stories about the first seminars and the first publications reporting the UA1 and UA2 discoveries of the weak bosons. But I wish to comment on how we perceived these events. As I already said, we were all expecting to see the weak bosons, we had no competition to fear from other laboratories and there was no question of UA2 "scooping" UA1 in the sense of

Fig. 7. The upgrade of the antiproton source into a double ring, accumulator and collector, was an opportunity to also upgrade the detectors. How to do it best was difficult to decide and the UA2 Collaboration met in Assisi in order to reach an agreement on the final design. The above drawing was sketched on that occasion in the hope of getting inspiration from such a mythic meeting place. The cute little devil representing UA1 is a good illustration of the omnipresence of the UA1/UA2 competition in our minds and, at the same time, of its ludic rather than dramatic nature

stealing a Nobel prize or whatever as Taubes has been suggesting. I repeat that there was no question in our minds that Carlo (and of course Simon, but this is not what I am talking about) deserved the whole credit for the success; that what had been a real outstanding achievement was the production of the weak bosons, not their detection; that without Carlo and Simon there would have been no proton–antiproton collider but that without UA1 and UA2 there would have been other experiments that would undoubtedly have done as good a job; that the success of UA2 was largely due to the quality of many physicists who had been working together very efficiently and with an excellent team spirit and that it was impossible to single out a few of them as deserving a larger part of the credit. Of course there was competition, of course we enjoyed being faster or more clever than UA1 whenever we could afford to be, as when we were first at reporting to the 1982 Paris Conference the observation of very clear hadron jets, a breakthrough in the history of strong interaction physics. But this was not the dish, it was just the spices. The dish was serious business. It was reporting to the physics community what we had been finding. It was writing papers that would stay forever as important documents in the history of science. For years we had learned that this implied intellectual rigour and honesty, that it should resist biasing influences such as theoretical preconceptions, to make it short that it had to obey the ethic of scientific research. We surely were not to forget that in such an outstanding occasion. In retrospect I am proud that we resisted the pressure that was exerted on us to publish faster than we thought we had to. It would have been stupid and childish to give in and would not have shown much respect for science. In fact this pressure made us almost overreact and, in the case of the Z, it caused a delay of nearly two months between the UA1 and UA2 publications because we preferred to wait for the imminently coming new run and collect more statistics before publishing. There was virtually no dissenting opinion in UA2 that we should have behaved differently, we were all feeling quite strongly about it, in particular the wiser and more experienced members of the Collaboration (I mean the generation before mine) were giving their full support to this line. It is obvious today that there would have been no point in making a fuss about an event detected in 1982 that was most likely a Z but had one of its decay electrons not identified because it was hitting a coil of our forward spectrometer magnets. It is obvious today that we had been wise to wait for more statistics before publishing the Z results. The issue at stake was not to bet on the truth (as I explained already there would have been no pride in making the right bet) but to behave as if we had been the only experiment. There was no hurry from a purely scientific point of view, and there was no glory in taking any risk. Of course we had no reason to doubt that the events we were seeing were W's and Z's, what else could they have been? But this was not an argument to be taken into consideration, in our opinion at least. As in UA1, several of our W and Z candidates had some peculiar features, usually instrumental, sometimes real, like a $Z \to e^+e^-\gamma$ event that had been collected very early. Understanding all that was asking for some statistics and I do not regret that we decided to wait for the coming run. I am not at all trying to criticize UA1 for having published too early, this is not for me to judge. I am just trying to explain that this was not a very important issue, it was only the kind of media pressure and excitement that was prevailing in the community at that time that made it appear important. Anyone who followed these events knows well that both experiments had very similar data and that there was no scientific argument for one to publish before the other. At least this is how we felt in UA2. We thought that time would damp the noise and help in having a more serene look at the history of those happy days. This is why I find it so disappointing that a journal like Physics World, that has some pretension at being scientific, does exactly the opposite.

Scientists of my generation are very fortunate to have witnessed such amazing progress in our understanding of nature, in phase with our own scientific life. It is remarkable that this has not only been the case in particle physics but also, and may be to an even greater extent, in astronomy – in particular astrophysics and cosmology

– and in life sciences – in particular genetics, molecular biology and neurosciences. While many questions remain unanswered in each of the three fields, none can be left aside any longer as being a mystery inaccessible to science. Our vision of the world has changed drastically. Having had a chance to contribute to this progress, however modest our contribution may have been, is a very happy fortune. May science be smiling at the next generation as kindly as it did to us with the new physics that LHC is soon going to reveal.

References

Here is not the place to give an exhaustive list of sources. I only mention a very few from which an extensive list of references can easily be extracted.

The four publications from UA1 and UA2 announcing the observations of the *W* and *Z* bosons are the following (the front pages of these publications have been reproduced in the Appendix below):

– G. Arnison et al., Phys. Lett. **122B**, No. 1, (1983) p. 103
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2. J. Krige, article on the proton–antiproton project in History of CERN, Volume III, chapter 6 (Elsevier, North Holland, Amsterdam, 1996)
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4. P. Darriulat, *The W and Z bosons: chronicle of an announced discovery*, in History of Original Ideas and Basic Discoveries in Particle Physics, H.B. Newman, T. Ypsilantis, editors, NATO ASI Series B: Physics Vol. **352**, Plenum Press, New York and London, page 757. I take this opportunity to add three references to the list given in this latter article
5. D. Möhl et al., *Possibilities for Antiproton Beams at CERN Using Cooling by Electrons*, CERN/EP Internal Report **76-03**, February 20, 1976
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First published in Eur. Phys. J. C 34, 33–40 (2004)
Digital Object Identifier (DOI) 10.1140/epjc/s2004-01764-x

Appendix

Volume 122B, number 1 PHYSICS LETTERS 24 February 1983

EXPERIMENTAL OBSERVATION OF ISOLATED LARGE TRANSVERSE ENERGY ELECTRONS WITH ASSOCIATED MISSING ENERGY AT $\sqrt{s}$ = 540 GeV

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Received 23 January 1983

We report the results of two searches made on data recorded at the CERN SPS Proton–Antiproton Collider: one for isolated large-E_T electrons, the other for large-E_T neutrinos using the technique of missing transverse energy. Both searches converge to the same events, which have the signature of a two-body decay of a particle of mass ~80 GeV/c^2. The topology as well as the number of events fits well the hypothesis that they are produced by the process $\bar{p} + p \to W^{\pm} + X$, with $W^{\pm} \to e^{\pm} + \nu$; where $W^{\pm}$ is the Intermediate Vector Boson postulated by the unified theory of weak and electromagnetic interactions.

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Volume 122B, number 5,6 PHYSICS LETTERS 17 March 1983

OBSERVATION OF SINGLE ISOLATED ELECTRONS OF HIGH TRANSVERSE MOMENTUM IN EVENTS WITH MISSING TRANSVERSE ENERGY AT THE CERN $\bar{p}p$ COLLIDER

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Received 15 February 1983

We report the results of a search for single isolated electrons of high transverse momentum at the CERN $\bar{p}p$ collider. Above 15 GeV/c, four events are found having large missing transverse energy along a direction opposite in azimuth to that of the high-p_T electron. Both the configuration of the events and their number are consistent with the expectations from the process $\bar{p} + p \to W^{\pm}$ + anything, with $W \to e + \nu$, where $W^{\pm}$ is the charged Intermediate Vector Boson postulated by the unified electroweak theory.

1. Introduction. The very successful operation of the CERN $\bar{p}p$ Collider at the end of 1982, with peak luminosities of $\sim 5 \times 10^{28}$ cm^{-2} s^{-1}, has allowed the UA2 experiment to collect data corresponding to a total integrated luminosity of ~ 20 nb^{-1}. According to current expectations [1], these data should contain approximately four events of the type

$$\bar{p} + p \to W^{\pm} + \text{anything}, \quad W^{\pm} \hookrightarrow e^{\pm} + \nu(\bar{\nu}), \tag{1}$$

where $W^{\pm}$ is the charged Intermediate Vector Boson (IVB) which mediates the weak interaction between charged currents [2]. In fact it was the search for such particles, and for the neutral IVB, the Z^0, that motivated the transformation of the CERN Super Proton Synchrotron (SPS) into a $\bar{p}p$ collider operating at a

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0 031-9163/83/0000–0000/\$ 03.00

Volume 126B, number 5 PHYSICS LETTERS 7 July 1983

EXPERIMENTAL OBSERVATION OF LEPTON PAIRS OF INVARIANT MASS AROUND 95 GeV/c^2 AT THE CERN SPS COLLIDER

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Received 6 June 1983

We report the observation of four electron–positron pairs and one muon pair which have the signature of a two-body decay of a particle of mass ~95 GeV/c^2. These events fit well the hypothesis that they are produced by the process $\bar{p} + p \to Z^0 + X$ (with $Z^0 \to \ell^+ + \ell^-$), where Z^0 is the Intermediate Vector Boson postulated by the electroweak theories as the mediator of weak neutral currents.

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 0 031-9163/83/0000–0000/$ 03.00

Volume 129, number 1,2 PHYSICS LETTERS 15 September 1983

EVIDENCE FOR $Z^0 \to e^+e^-$ AT THE CERN $\bar{p}p$ COLLIDER

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Received 11 August 1983

From a search for electron pairs produced in $\bar{p}p$ collisions at $\sqrt{s} = 550$ GeV we report the observation of eight events which we interpret as resulting from the process $\bar{p} + p \to Z^0$ + anything, followed by the decay $Z^0 \to e^+ + e^-$ or $Z^0 \to e^+ + e^- + \gamma$, where Z^0 is the neutral Intermediate Vector Boson postulated by the unified electroweak theory. We use four of these events to measure the Z^0 mass

$M_Z = 91.9 \pm 1.3 \pm 1.4$ (systematic) GeV/c^2.

1. Introduction. The primary goal of the experimental program at the CERN $\bar{p}p$ Collider has been to search for the massive Intermediate Vector Bosons (IVB), which are postulated to mediate the electroweak interaction [1].

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The recent observation of single isolated electrons with high transverse momentum in events with missing transverse energy [2,3] is consistent with the process $\bar{p} + p \to W^\pm$ + anything, followed by the decay $W^\pm \to e^\pm + \nu(\bar{\nu})$, where W is the charged IVB.

We report here the observation in the UA2 detector of eight events which we interpret in terms of the reaction

$$\bar{p} + p \to Z^0 + \text{anything}$$
$$\hookrightarrow e^+ + e^- \text{ or } e^+ + e^- + \gamma, \qquad (1)$$

Peter Zerwas

W & Z physics at LEP

1 Introduction

The fundamental laws of nature which govern the microscopic world have been systematically explored by particle physics since the middle of the last century. Particle physics has succeeded not only in revealing the structure of matter, but also in explaining its interactions. The present state of our knowledge is contained in the Standard Model, formulated at the quantum level as required for microscopic physics. The model incorporates three components: the matter particles are grouped in three lepton and quark families; the forces are generated by the electromagnetic, weak and strong interactions; and the Higgs mechanism, still hypothetical, is introduced to generate the masses of the fundamental particles[1]. Gravity is attached *ad hoc* as a classical phenomenon but not deeply incorporated into the system.

The electromagnetic and weak interactions are unified to electroweak interactions within the Standard Model – one of the greatest achievements of physics in the 20th century. They are formulated in the Glashow–Salam–Weinberg model [1, 2] as an $SU(2) \times U(1)$ gauge field theory, including the Higgs mechanism for generating the masses [3].

The first two crucial steps in establishing the electroweak part of the Standard Model experimentally were the discovery of Neutral Currents in neutrino scattering by the Gargamelle Collaboration [4, 5] and, only a decade later, the discovery of the

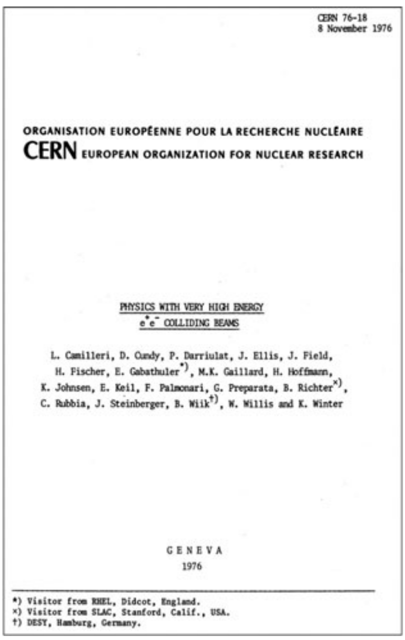
[1] The observation of non-zero neutrino masses leads to an extension of the Standard Model as conceived originally. While the lepton and quark sectors are symmetrized beautifully by introducing right-handed degrees of freedom for neutrinos, the R-neutrino fields may carry along a new mass parameter generated at high energy scales close to the grand unification scale of the three gauge interactions.

gauge bosons $W^{\pm}$ and Z in $\bar{p}p$ collisions at the $S\bar{p}pS$ collider by the UA1 and UA2 Collaborations [6, 7].

Establishing the theory at the quantum level was the next logical experimental step. This step followed the pioneering theoretical work by G. 't Hooft and M. Veltman [8] by which the renormalizibility of the Standard Model, as a non-Abelian/Abelian massive gauge field theory incorporating the Higgs mechanism, was proven, *i.e.* the firm mathematical foundation and basis for precise calculations of physical quantities. The theory could be extended from leptons to hadrons after the charm quark was introduced by the Glashow–Iliopoulos–Maiani mechanism [9].

The experimental proof that the theory correctly describes phenomena at the quantum level is a necessary requirement for any theory operating in the microscopic world. At the same time, performing experimental analyses with high precision opens windows to new physics phenomena at high energy scales that can only be accessed indirectly through virtual effects. These goals have been achieved by LEP.

For the fourth step in this process, establishing the Higgs mechanism for generating the masses of the fundamental particles, indirect evidence has been accumulated by LEP but the picture could not be completed. The final decision, most likely, has to await experimentation in the near future at LHC [10].


CERN 76-18
8 November 1976

ORGANISATION EUROPÉENNE POUR LA RECHERCHE NUCLÉAIRE
CERN EUROPEAN ORGANIZATION FOR NUCLEAR RESEARCH

PHYSICS WITH VERY HIGH ENERGY
e^+e^- COLLIDING BEAMS

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GENEVA
1976

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Fig. 1. *Left*: cover page of the seminal CERN Report 76-18 [12] on the physics potential of a 200 GeV e^+e^- collider; *right*: LEP at CERN, including the four universal detectors, ALEPH, DELPHI, L3 and OPAL

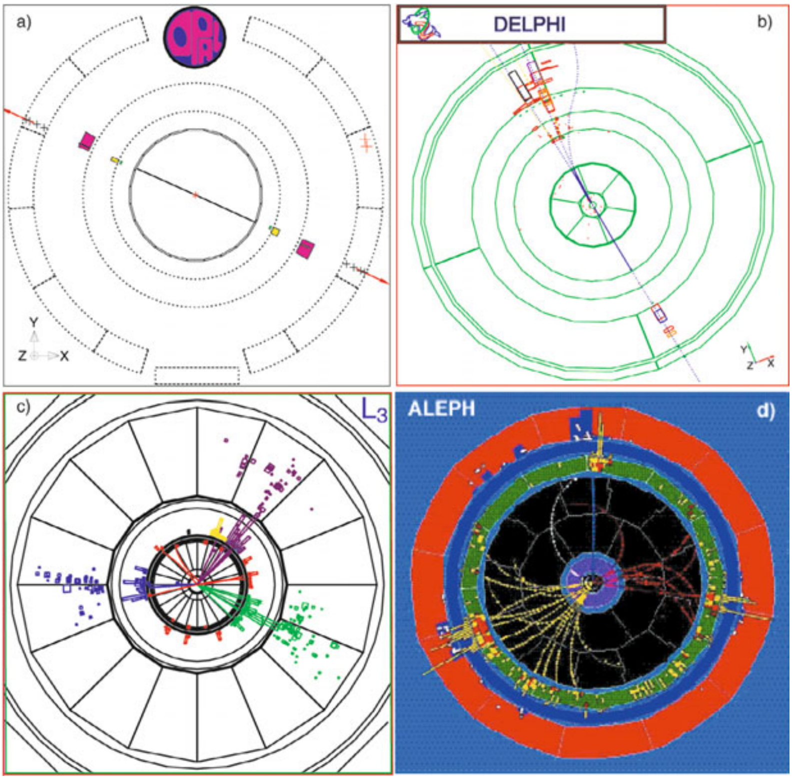

Fig. 2. Typical events, as recorded by the four LEP experiments, **a** $\mu^+\mu^-$ pair in OPAL; **b** $\tau^+\tau^-$ pair in DELPHI; **c** 3-jet event in L3; **d** W^+W^- event close to threshold, with W decays to τ–ν_τ and a pair of jets in ALEPH

Before LEP operations started in 1989, the state of the electroweak sector could be described in condensed form by a small set of characteristic parameters, see [11]: the masses of the $W^\pm$ and Z bosons had been measured to an accuracy of a few hundred MeV, and the electroweak mixing angle, $\sin^2\theta_W$, had been determined at the percent level. The accuracy with which these observables could be measured, led to a prediction of the top-quark mass at 130 ± 50 GeV, but no bound could be derived on the Higgs mass.

Soon after the highly successful operation of e^+e^- colliders in the early 1970's, the idea of building such a facility in the energy region up to 200 GeV was advanced by a group of experimentalists and theorists in a seminal CERN report, CERN 76-18 [1976], in which the physics potential was outlined quite comprehensively [12] (Fig. 1, *left*).

LEP, the Large Electron–Positron Collider (Fig. 1, *right*), finally started operation in 1989, equipped with four universal detectors, ALEPH, DELPHI, L3 and OPAL (Fig. 2). The machine operated in two phases. In the first phase, between 1989 and

1995, 18 million Z bosons were accumulated, while in the second phase, from 1996 to 2000, some 80 thousand W bosons were generated at energies gradually climbing from the W^+W^--pair threshold to the maximum of 209 GeV – with excellent machine performance at all energy steps.

2 Z-Boson physics

2.1 The electroweak basis

The Z boson in the Glashow–Salam–Weinberg model is a mixture of the neutral SU(2) isospin W^3 and the U(1) hypercharge B gauge fields, with the mixing parameterized by the angle θ_W:

$$A = B\cos\theta_W + W^3 \sin\theta_W$$
$$Z = -B\sin\theta_W + W^3 \cos\theta_W$$

The Z-boson interacts with vector and axial-vector currents of matter proportional to the Z-charges of the leptons and quarks which are determined by the isospin and the electric charges of the particles:

$$g_V^f = I_{3L}^f - 2Q' \sin^2\theta_W$$
$$g_A^f = I_{3L}^f$$

The Z–matter couplings are affected by electroweak radiative loop corrections. The overall couplings are modified by the ρ parameter while the mixing angle is generically parameterized by the effective value for the lepton currents. High-precision analyses of the couplings therefore allow tests of the theory at the quantum level.

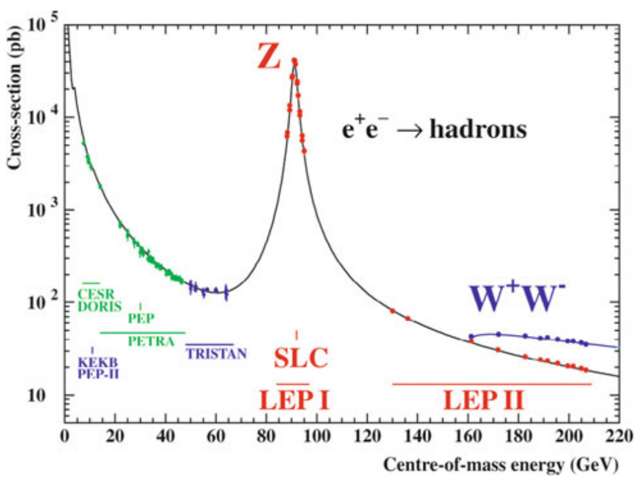

Fig. 3. The e^+e^- annihilation cross section to hadrons, from initial low energies in early colliders to the maximum energy at LEP [13]

The properties of the Z-boson and of the underlying electroweak theory could be studied at LEP by measuring a threefold ensemble of observables: the overall formation cross section, *i.e.* the line-shape, that is parameterized in the Breit–Wigner form by the Z-boson mass and its width; the forward–backward asymmetries of the leptons and quarks; and the polarization of τ leptons, both measuring the vector- and axial-vector Z-boson charges of the fermions involved. Outstandingly clear events could be observed in each of the four detectors (Fig. 2). As a result, the experimental analysis of the Z line-shape (Fig. 3), of the decay branching ratios and the asymmetries could be performed with precision unprecedented in high-energy experiments [13]:

$$M_Z = 91\ 187.5 \pm 2.1 \text{ MeV}$$
$$\Gamma_Z = 2495.2 \pm 2.3 \text{ MeV}$$
$$\sin^2 \theta_{\text{eff}}^{\text{lept.}} = 0.23138 \pm 0.00014$$

(including SLC results). Thus, the electroweak sector of the Standard Model has passed examination successfully at the per-mille level. This is highlighted by the global analyses of the electroweak mixing parameter $\sin^2 \theta_{\text{eff}}^{\text{lept.}}$ – truly in the realm where quantum theory is the proper basis for formulating the laws of nature. The collection of observables and parameters in Fig. 4 evidently conforms to the theory, with deviations from the average line at the 2 standard deviation level only in the

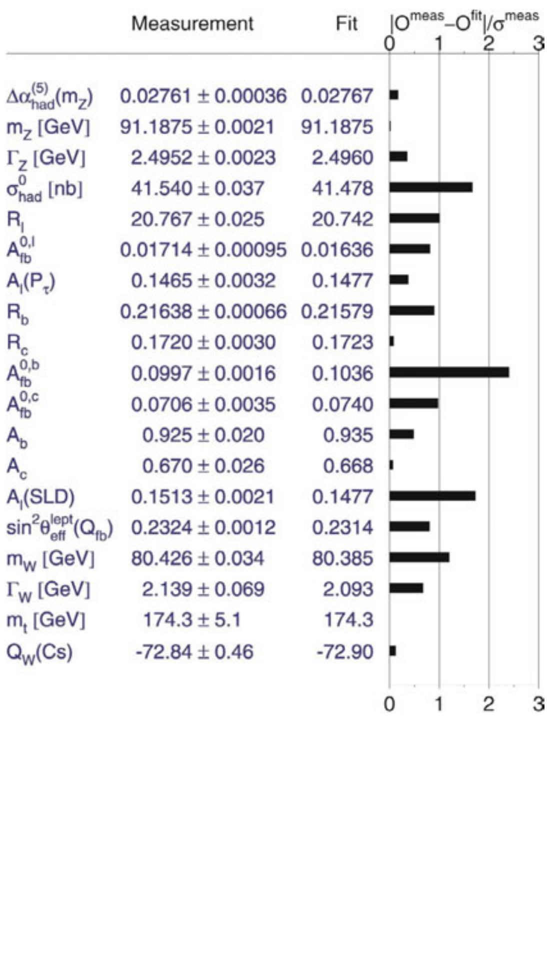

Fig. 4. Precision observables in the electroweak part of the Standard Model, as measured at LEP and elsewhere (excerpt from [13])

forward–backward asymmetry of b-quark jets and the left–right electron polarization asymmetry measured at the Stanford Linear Collider SLC facility.

Beyond the most stringent test of the electroweak theory itself, important conclusions could be drawn on other aspects of the Standard Model and potential physics beyond by studying the e^+e^- collisions on the tip of the Z-boson resonance and in its Breit–Wigner wings.

2.2 Top-quark prediction

The physics of the top quark has truly been a success story at LEP, even though the particle is too heavy to be produced at a 200 GeV collider. Not only could the existence of this heaviest of all quarks in the Standard Model be predicted, but also its mass could be pre-determined from the analysis of quantum corrections with amazing accuracy – a textbook example of the great potential of fruitful cooperation between theory and experiment in high-precision analyses.

By analyzing the partial decay width and the forward–backward asymmetry of Z decays to b-quark jets at LEP and complementing this set by the production rate of b quarks at the lower-energy collider PETRA, which is sensitive to the interference between s-channel γ and Z exchanges, the isospin of the b-quark could be uniquely determined [14] (Fig. 5). From the measured quantum number $I_3^L = -1/2$, the existence of an isospin +1/2 partner to the bottom quark could be derived conclusively – the top quark.

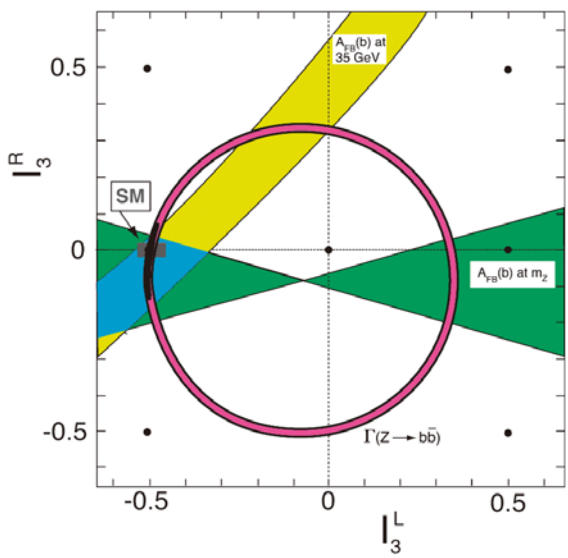

Fig. 5. Determining the weak isospin of the bottom quark [14]; circle: partial Z-decay width to $b\bar{b}$ at LEP; wedges: forward–backward b asymmetry at LEP; strip: $b\bar{b}$ cross section at PETRA. All measurements cross the point $[I_3^L, I_3^R] = [-1/2,\ 0]$ so that an isospin partner to the b quark with $[I_3^L, I_3^R] = [+1/2, 0]$ should exist – the top quark

More than that: virtual top quarks in $t\bar{b}$ and $t\bar{t}$ loops affect the propagation of the electroweak gauge bosons. This effect modifies, in particular, the relation between the Fermi coupling G_{F} of β decay, the Z-boson mass M_Z, and the electroweak mixing angle $\sin^2\theta_{\mathrm{eff}}^{\mathrm{lept.}}$. The correction is parameterized in the ρ parameter and increases quadratically in the top-quark mass [15]:

$$\rho = 1 + \Delta\rho_t + \Delta\rho_H$$
$$\Delta\rho_t \sim +G_{\mathrm{F}} m_t^2$$

leading to the prediction [16]:

$$m_t = 173^{+12+18}_{-13-20}\ \mathrm{GeV}$$

for the top-quark mass before top quarks were established at the Tevatron and the mass confirmed by direct observation.

Truly – a triumph of high-precision experimentation at LEP joined with theoretical high-precision calculations at the quantum level of the Standard Model.

2.3 Quantum chromodynamics QCD

Many of the key elements in QCD, the strong-interaction component [17] of the complete SU(3) × SU(2) × U(1) Standard Model, were established experimentally at e^+e^- colliders. The clean signals make these machines precision instruments for studying QCD, and the observations have contributed significantly towards putting this field theory of the strong interaction on a firm experimental basis.

That quarks come in three colors was indicated quite early on by the ratio of the hadronic e^+e^- annihilation cross section to the μ-pair cross section at ADONE – R being close to the value 3 × 2/3 = 2 instead of $2/3$ as naively expected in the colorless quark model. While the existence of quark jets was demonstrated a little later at SPEAR, the development was crowned by the observation of the PETRA jets – a direct and clear experimental signal for gluons, the carriers of the microscopic force of the strong interaction. This line continued straight through the LEP experiments.

2.3.1 QCD coupling

With the measurement of the QCD coupling at the scale M_Z,

$$\alpha_{\mathrm{s}} = 0.1183 \pm 0.0027,$$

and the observation of the running of α_{s} from low PETRA to high LEP energies as observed in jet analyses [18], the validity of *asymptotic freedom*, a key prediction in QCD [19], was demonstrated in a wonderful way (Fig. 6).

2.3.2 Non-Abelian gauge symmetry

With the observation of angular correlations in 4-jet final states of Z-boson decays [20], the 3-gluon self-coupling was clearly established, the characteristic of QCD

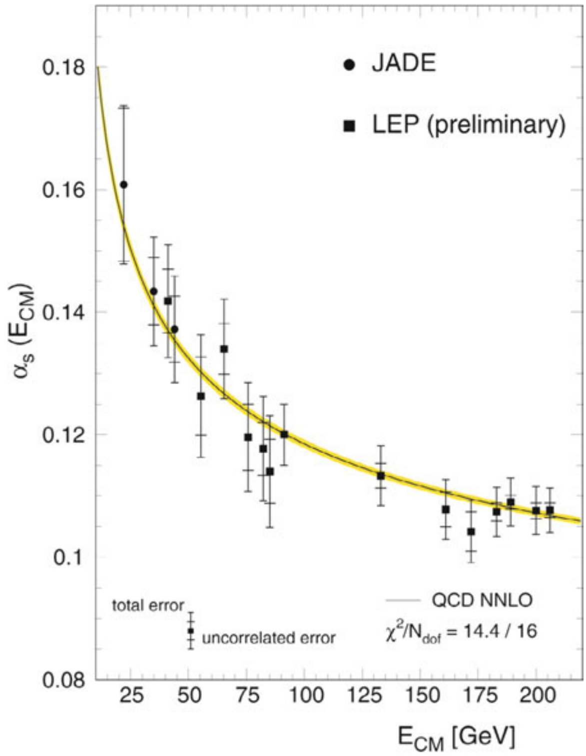

Fig. 6. The running of the QCD coupling from low PETRA to high LEP energies compared with the prediction of asymptotic freedom [18]

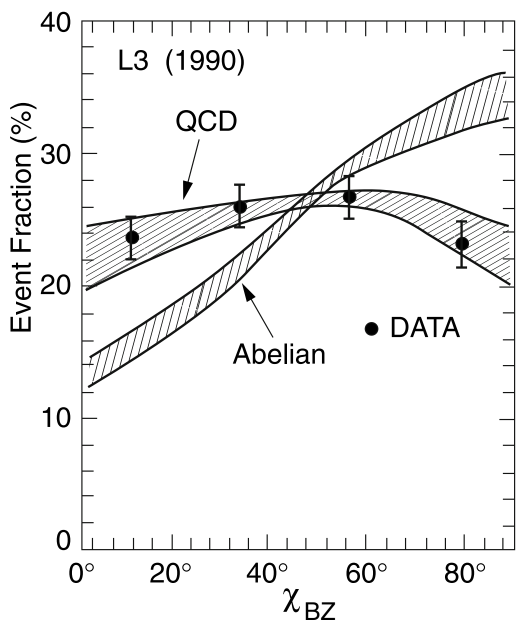

Fig. 7. The distribution of the azimuthal angle between the planes spanned by the high-energy jets and the low-energy jets in 4-jet events of Z decays [21]. The experimental distribution [20] is compatible with QCD, involving the self-coupling of the gluons, but it cannot be reproduced by an Abelian "QED-type" field theory of the strong interaction without gauge-boson self-coupling

being a *non-Abelian gauge theory* [21] (Fig. 7). With the measured value of the Casimir invariant [22] C_A,

$$C_A = 3.02 \pm 0.55,$$

the strength of the 3-gluon coupling agrees with the predicted value $C_A = 3$ for non-Abelian SU(3), but being far away from the value zero in any Abelian "QED-type" field theory without self-coupling of the gauge bosons.

2.3.3 Running quark masses

In the same way as couplings run, *quark masses* change when measured at different scales. The change of the mass value is a consequence of the retarded motion of the gluon cloud surrounding the quark when its momentum is altered by absorbing momentum from a hard photon, for instance. This effect could be observed in a unique way by measuring the b-quark mass at the Z scale and comparing this value with the value at a low scale [23] (Fig. 8). The measurement of the running b mass agrees well with the prediction of QCD.

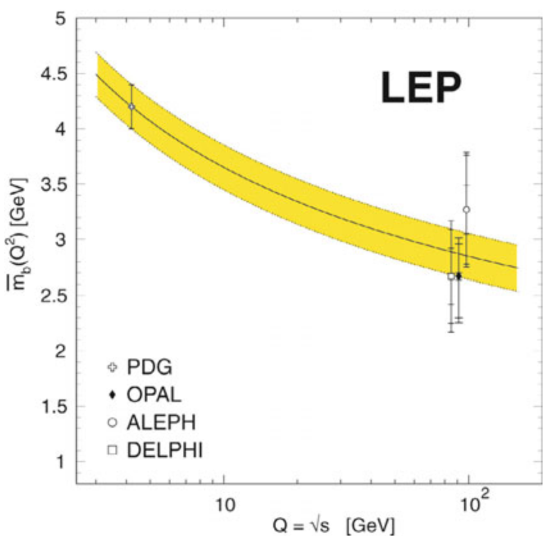

Fig. 8. The change of the bottom-quark mass when weighed at low and at high energies [23]

2.4 Three families in the Standard Model

The number of light neutrinos was determined at LEP by comparing the Z width as measured in the Breit–Wigner line-shape, with the visible lepton and quark-decay channels [13]. The ensuing difference determines the number of light neutrino species to be three:

$$N_\nu = 2.985 \pm 0.008.$$

Thus, LEP closed the canonical Standard Model with three families of matter particles.

2.5 Gauge coupling unification

When charges are measured in scattering experiments at different values of momentum transfer, they are altered as a consequence of screening and anti-screening effects in gauge field theories. These effects are generated by the vacuum polarization induced by virtual gauge-boson and fermion pairs in the fields surrounding charges. Fermions screen the charges; gauge bosons have the opposite effect, so that couplings in Abelian theories like QED increase when probed for larger momentum transfer, while non-Abelian theories are asymptotically free so long as the number of fermion degrees of freedom is small enough.

Extrapolating the three couplings [24] associated with the gauge symmetries SU(3) × SU(2) × U(1) in the Standard model to increasingly higher scales, they approach each other but do not really meet at the same point. This is different if the particle spectrum of the Standard Model is extended by supersymmetric partners [25] which modify, as virtual particles, the vacuum polarization. Independently of the mass values, so long as they are in the TeV region, the new degrees of freedom make the couplings converge to an accuracy close to 2% [26] (Fig. 9). This observation opens the exciting perspective that the three forces of the Standard Model may be unified at an energy scale close to 2×10^{16} GeV.

At the same time, strong support is given, though indirectly, for supersymmetry – a symmetry intimately related to gravity, the fourth of the fundamental interactions. This may thus lead us closer to the ultimate unification of all the four forces in nature.

Experimental high-precision results from LEP therefore have far-reaching, deep consequences for potential physics scenarios at scales far above the energies directly accessible at accelerators – whatever their energy range may be in even the distant future.

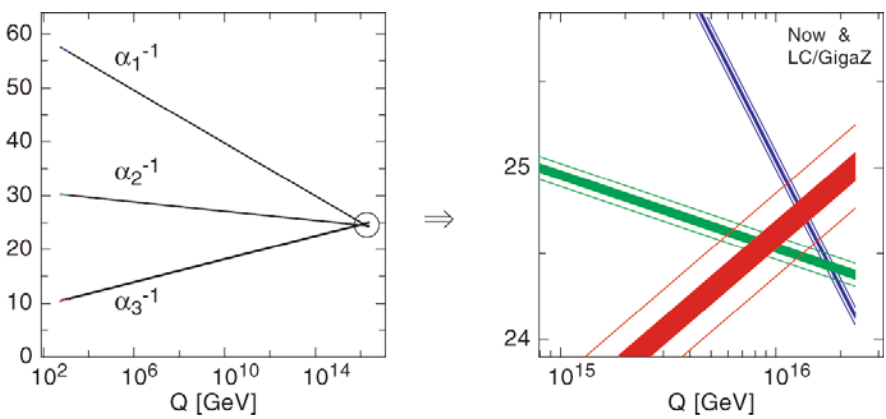

Fig. 9. Extrapolation of the SU(3), SU(2) and U(1) gauge couplings to high energies in the Minimal Supersymmetric extension of the Standard Model. They approach each other near 2×10^{16} GeV at a level of 2%, indicative of the Grand Unification of the three gauge interactions [26]

3 W-Boson physics

Gauge field theories appear to be the theoretical framework within which the three fundamental particle forces can be understood. Introduced by Weyl [27] as the basic symmetry principle of electrodynamics, the scheme was generalized later by Yang and Mills [28] to non-Abelian gauge symmetries before being applied to the electroweak and strong interactions.

One of the central tasks of the LEP experiments at energies beyond the W^+W^--pair threshold was the analysis of the 3-gauge boson couplings, predicted in form and magnitude by the gauge symmetry. A first glimpse could also be caught of the corresponding 4-boson couplings.

Charged W^+W^- pairs are produced in e^+e^- collisions (see Fig. 2) by three different mechanisms – neutrino exchange, and photon- and Z-boson exchanges [29].

From the steep increase of the excitation curve near threshold, and from the reconstruction of the W bosons in both leptonic and hadronic decay modes, the mass M_W and the width Γ_W can be reconstructed with high precision [30]:

$$M_W = 80.412 \pm 0.042 \text{ GeV}$$
$$\Gamma_W = 2.150 \pm 0.091 \text{ GeV}$$

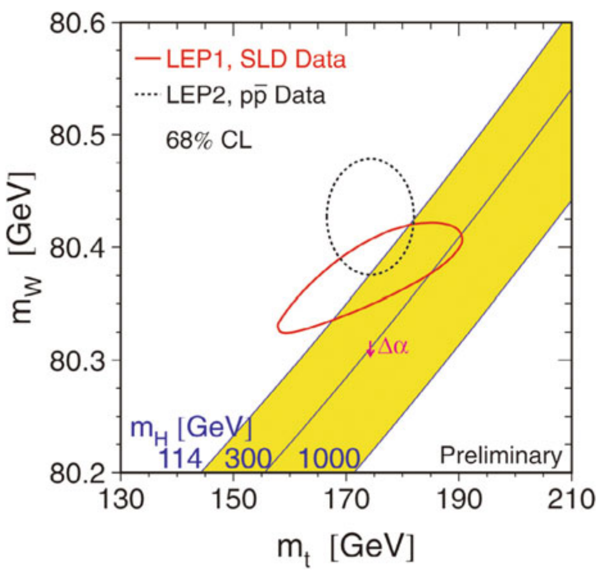

Fig. 10. Comparison of the W-boson and t-quark masses, as extracted from radiative corrections, with the directly measured mass values [13]

This value of the directly measured W mass is in excellent agreement with the mass value extracted indirectly from precision observables, as evident from Fig. 10.

Any of the three W^+W^--production mechanisms, if isolated from the others, leads to a cross section that rises indefinitely with energy. However, the amplitudes interfere destructively as predicted by the gauge symmetry between fermion and gauge boson couplings. As a result of these gauge cancellations, the final cross section is damped by a factor $1/E^2$ for large energies. The prediction is clearly borne out by the LEP data [13] (Fig. 11), thus confirming the crucial impact of gauge symmetries on the dynamics of the electroweak sector in the Standard Model in a most impressive way.

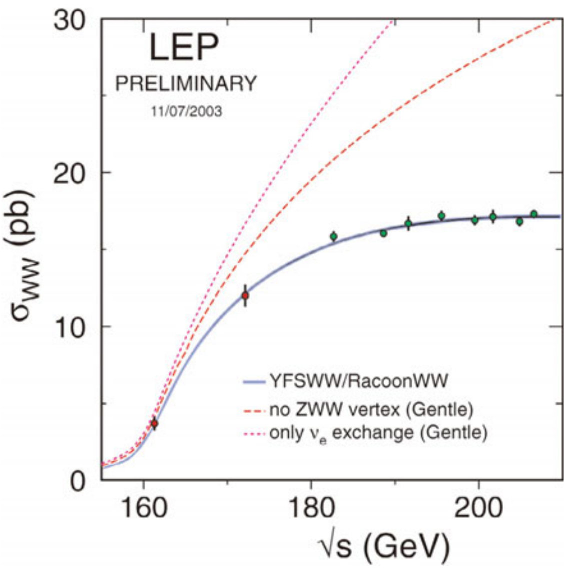

Fig. 11. The total cross section for W-pair production $e^+e^- \to W^+W^-$ at LEP in the Standard Model. The measurements are also confronted with ad-hoc scenarios in which three-boson self-couplings are switched off. The gauge symmetries are evidently crucial for the understanding of the measurements [13]

The impact of the gauge symmetries on the trilinear couplings can be quantified by measuring the static electroweak parameters of the charged W bosons, *i.e.* the monopole charges, the magnetic dipole moments and the electric quadrupole moments of the W bosons coupled to the γ and to the Z boson; for the photon coupling,

$$
\begin{aligned}
g_1 &= e \\
\mu_W &= 2 \times \tfrac{e}{2M_W} \\
q_W &= -\tfrac{e}{M_W^2}
\end{aligned}
$$

and for the Z coupling analogously. These predictions were confirmed experimentally within a margin of a few percent.

Studying the quartic couplings requires 3-boson final states. Some first analyses of $W^+W^-\gamma$ final states have bounded any anomalies to less than a few percent.

4 Higgs mechanism

The fourth step in establishing the electroweak sector of the Standard Model experimentally, the search for the Higgs particle, could not be completed by LEP. Nevertheless, two important results were reported by the experiments.

4.1 Virtual Higgs mass estimate

By emitting and reabsorbing a virtual Higgs boson from a propagating electroweak boson, the mass of the boson is slightly shifted. In parallel with the top quark, this effect can be included in the ρ parameter. With the contribution [31]

$$\Delta\rho_H \sim -G_\mathrm{F} M_\mathrm{W}^2 \log M_H^2/M_\mathrm{W}^2$$

the Higgs boson is screened, as expected for any field-theoretic regulator, and the effect is only logarithmic in the Higgs mass so that the sensitivity is reduced considerably.

Nevertheless, from the "Blue-Band Plot", cf. Fig. 12, in which the set of all the established precision measurements is summarized, a most probable value of about 100 GeV is indicated, with large error though, for the Higgs mass in the Standard Model and related theories, such as supersymmetric theories. An upper bound close to 200 GeV has been found in the analysis [13]:

$$\begin{aligned} M_H &= 91^{+58}_{-37}\ \mathrm{GeV} \\ M_H &< 202\ \mathrm{GeV} \end{aligned}$$

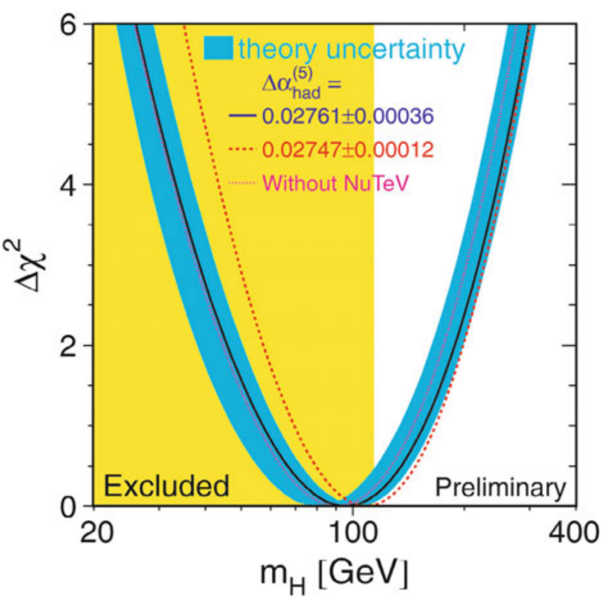

Fig. 12. "Blue-Band Plot": Probability distribution of the Higgs mass in the Standard Model [and related theories], derived from precision data from LEP and elsewhere [13]

Thus, in the framework of the Standard Model and in a large class of potential extensions, LEP data point to a moderately small Higgs mass in the intermediate mass range of the particle. This is corroborated by analyses of all the individual observables except the forward–backward asymmetry of b jets. This indirect evidence for a light Higgs sector is complemented by indirect counter-evidence against a large class of models constructed for generating mechanisms of electroweak symmetry breaking by new strong interactions.

4.2 Real Higgs mass bound

The direct search for the Higgs particle in the Higgs-strahlung process $e^+e^- \to ZH$ has set a stringent lower limit on the mass of the particle in the Standard Model [32]:

$$M_H > 114.4 \text{ GeV} \quad [95\% \text{ C.L.}]$$

However, we have been left with a 1σ effect for Higgs masses in excess of 115 GeV, fueled by the 4-jet channel in one experiment. “This deviation, although of low significance, is compatible with a Standard Model Higgs boson in this mass range while being also in agreement with the background hypothesis” [32].

5 Summary

Based on the high-precision measurements by the four experiments, ALEPH, DELPHI, L3 and OPAL, and in coherent action with a complex *corpus* of theoretical analyses, LEP led to an impressive set of fundamental results, the traces of which will be imprinted in the history of physics:

- Essential elements of the Standard Model of particle physics are firmly established at the quantum level:
  - the SU(3) × SU(2) × U(1) multiplet structure of the fundamental constituents of matter and their interactions with the strong and electroweak gauge bosons;
  - the gauge symmetry character of the self-interactions among the electroweak bosons $W^\pm$, Z and γ, and among the gluons.
- Indirect evidence has been obtained for the existence of a light Higgs boson in Standard Model type scenarios.
- The extrapolation of the three gauge couplings points to the Grand Unification of the individual gauge interactions at a high energy scale – compatible with the supersymmetric extension of the Standard Model in the TeV range.

In the precision analyses performed at LEP, many physics scenarios beyond the Standard Model were probed, constraining their scale parameters to ranges between the upper LEP energy and the TeV and multi-TeV scales. These studies led to a large number of bounds on masses of supersymmetric particles, masses and mixings of novel heavy gauge bosons, scales of extra spacetime dimensions, radii of leptons and quarks, and many other examples.

In conclusion:
LEP has made significant contributions to the process of establishing the Standard Model for matter and forces.

In addition, experiments at LEP have built a platform for physics scenarios beyond the Standard Model in the TeV range which can shortly be explored at the hadron collider LHC under construction and prospective electron–positron linear colliders.

Acknowledgements. Special thanks for help in preparing this report go to S. Bethke, G. A. Blair, A. Brandenburg, K. Desch, M. Grünewald, W. Porod, and R. Seuster.

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First published in Eur. Phys. J. C 34, 41–49 (2004)
Digital Object Identifier (DOI) 10.1140/epjc/s2004-01765-9

John Ellis

Physics at the LHC

1 Introduction

The LHC will be the first accelerator to explore directly the TeV scale. Any new energy range takes us deeper into the structure of matter, but there are good reasons to expect the TeV range to be particularly interesting, since there are several indications that it might reveal new physics. One is that we expect it to reveal the *origin of particle masses*, which are presumably due to the Higgs mechanism [1] but possibly with the aid of additional particles beyond the single Higgs boson of the minimal Standard Model, such as *supersymmetry* [2]. These seem to be required, for example, to stabilize the energy scale of the weak interactions below 1 TeV [3]. Another indication of new physics at the TeV scale may be provided by attempts to unify the fundamental gauge interactions, which fail if only Standard Model particles are included in the calculations, but work well if supersymmetric particles appear at the TeV scale [4]. Another hint of new physics at the TeV scale is provided by the astrophysical evidence for *dark matter*, which is naturally explained by new weakly-interacting particles weighing less than a TeV [5]. Finally, the muon anomalous magnetic moment [6] provides evanescent suggestions of new physics at the TeV scale.

As seen in Fig. 1, the LHC is designed to provide high collision rates that should be ample to produce the Higgs boson and supersymmetric particles if they exist in the TeV energy range. In addition, the LHC will yield plenty of bread-and-butter Standard Model physics. For example, its large sample of W bosons will enable the W mass to be measured with an accuracy of about 15 MeV, and its large sample of top quarks will enable the top mass to be measured with an accuracy of about 1 GeV [7, 8]. In addition to these bread-and-butter topics, the LHC will be able to

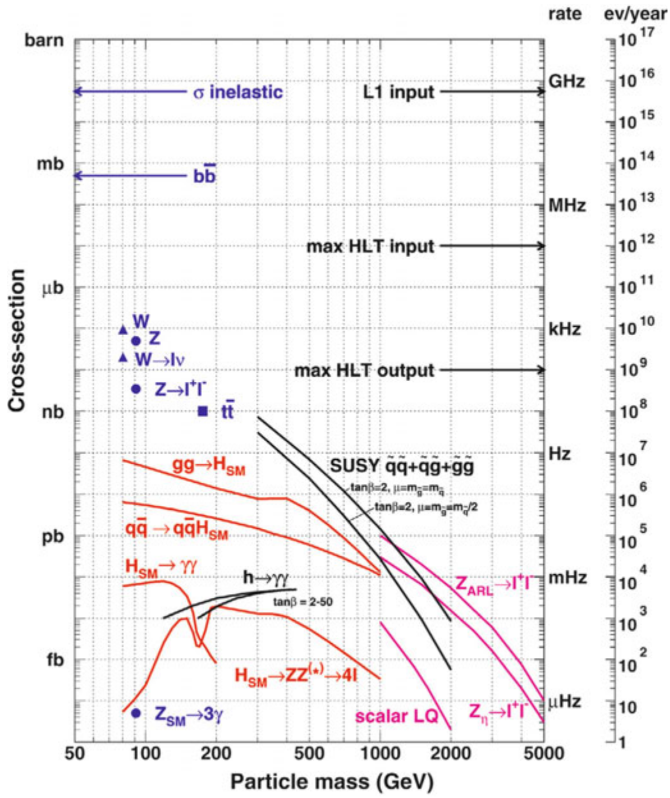

Fig. 1. Typical cross sections and event rates at the LHC, at $\sqrt{s} = 14$ TeV, assuming a luminosity of 10^{34} cm^{-2} s^{-1}

explore dense hadronic matter in relativistic heavy-ion collisions, where the quark–gluon plasma may be created. The LHC will also provide a good opportunity to study matter–antimatter asymmetry via *CP* violation in *B* system. Each of these LHC opportunities is reviewed in the following.

Many of the most interesting aspects of LHC physics touch on the interface between particle physics and cosmology: the Higgs boson may be a prototype for the inflaton, supersymmetry may provide the dark matter in the Universe, heavy-ion collisions may reproduce conditions in the first microseconds in the life of the Universe, and *CP* studies may help understand the origin of the matter in the Universe.

2 The quest for the Higgs boson

Generating the masses of the electroweak vector bosons requires breaking gauge symmetry spontaneously, i.e., there must be a field X with non-zero isospin I that

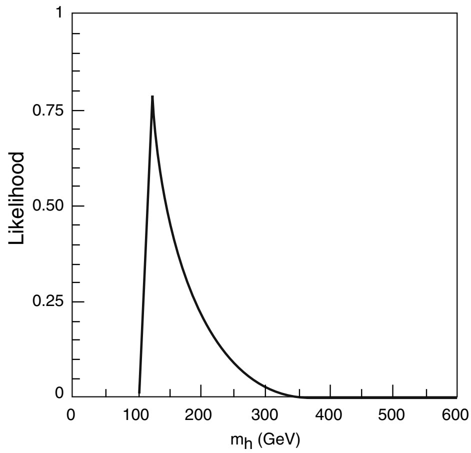

Fig. 2. The likelihood function for the mass of the Higgs boson obtained by combining information from direct searches at LEP and precision electroweak measurements

has a non-zero vacuum expectation value:

$$m_{W,Z} \neq 0 \Leftrightarrow \langle 0 | X_I | 0 \rangle \neq 0$$

In addition, the relation:

$$m_W^2 = m_Z^2 \times \cos^2 \theta_{\mathrm{W}}$$

implies that I = 1/2 is preferred. Moreover, the value I = 1/2 is also needed to give masses to the fermions of the Standard Model.

The next question concerns the nature of the field X: is it elementary or is it composite? The option used in the original formulation of the Standard Model was an elementary Higgs field: $\langle 0|H|0\rangle \neq 0$ [1]. However, this option is subject to large quantum (loop) corrections:

$$\delta m_{H,W}^2 = O\left(\frac{\alpha}{\pi}\right) \Lambda^2$$

where Λ is a cut-off representing the energy scale at which new physics beyond the Standard Model appears. One of the favoured origins for this cut-off is supersymmetry [2]. If the loop corrections to the Higgs and W masses are to be naturally small, the cut-off Λ should be less than about 1 TeV. In particular, sparticles should appear below this scale, if they are to stabilize the electroweak scale [3].

An alternative to an elementary Higgs field H is a condensate of fermion pairs, as happens in the BCS theory of superconductivity – where electron pairs condense – and in QCD – where quark–antiquark pairs condense in the vacuum. One of the theories studied was that top quark–antiquark pairs might condense and replace the elementary Higgs field [9], but the simplest examples of this type would have required the top quark to have weighed above 200 GeV, so these models are excluded. An alternative theory postulated a new strong technicolour force binding together new technifermions [10]. However, simple examples of this type are inconsistent with precision electroweak data [11]. In the absence of a viable alternative for the moment, in the following we concentrate on the elementary Higgs option.

Precision electroweak measurements at LEP, SLC, etc., predicted successfully that the top quark would be found with mass in the range 160 to 180 GeV, and it was indeed found with a mass ~ 175 GeV [12]. The precision electroweak experiments are also sensitive to the mass of the Higgs boson and, when combined with the measurement of the top mass, suggest that $m_H < 200$ GeV [13]. Direct searches for the Higgs boson at LEP using the reaction $e^+e^- \to Z + H$ saw a hint in late 2000, whose significance is now estimated to be $< 2\sigma$. Finally, they only provide the lower limit $m_H > 114.4$ GeV [14]. The likelihood function obtained by combining the direct and indirect information on the Higgs boson is shown in Fig. 2: it is peaked sharply around 120 GeV, suggesting that the Higgs boson may not be far away.

The most important Higgs decays vary rapidly as the Higgs mass increases from 120 to 200 GeV, so the LHC experiments must be prepared for a range of different signatures. These include $H \to$ bottom–antibottom pairs in association with top or bottom quarks, $H \to \gamma\gamma$, $H \to ZZ \to 4$ leptons, $H \to WW$ and $H \to \tau\tau$ [7, 8]. Combining these channels, it seems certain that a Standard Model Higgs boson can be found at the LHC, whatever its mass, and potentially quite quickly if the Higgs mass is about 150 GeV or more, as seen in Fig. 3. Most difficult to find would be a Higgs boson weighing about 115 GeV. The Higgs mass could be measured with a precision of the order of 1‰ if it weighs less than about 400 GeV, and a number of ratios of its couplings could be measured at the ~ 10 to 20% level [7, 8].

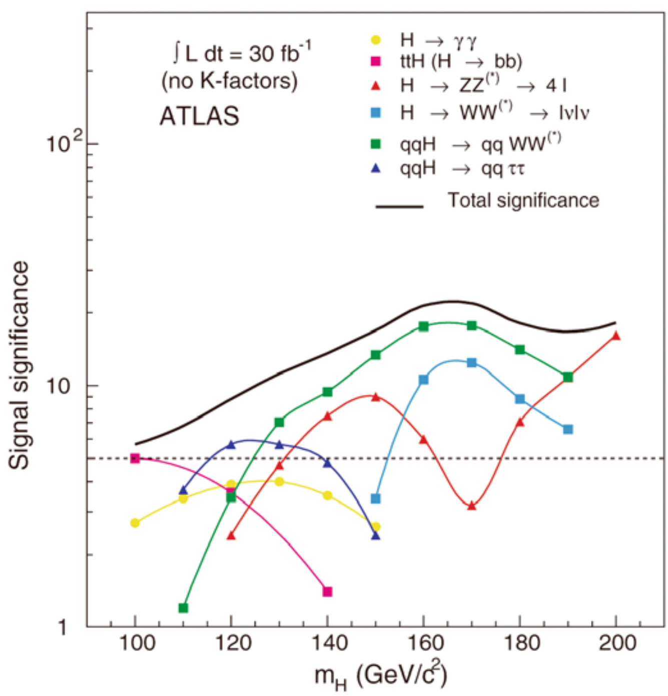

Fig. 3. Signal-to-background ratios for Higgs detection in various channels at the LHC

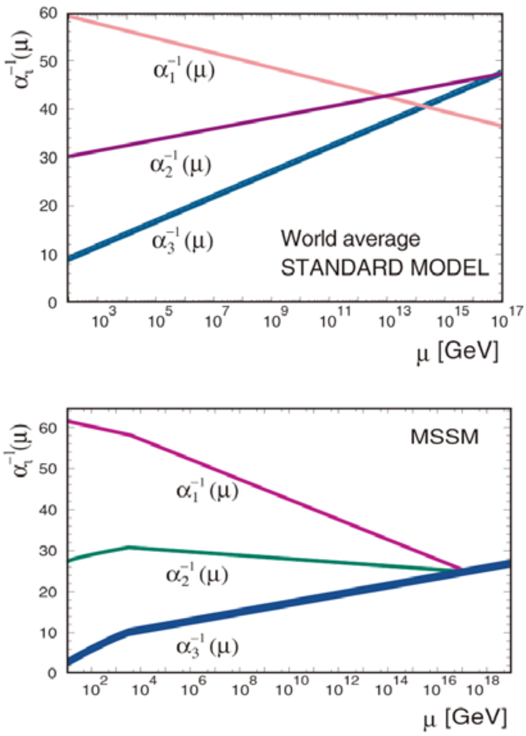

Fig. 4. Unification of the strong and electroweak interactions is not possible without supersymmetric particles (*top graph*) but is possible with supersymmetric particles (*bottom graph*)

3 The quest for supersymmetry

As already mentioned, the primary motivation for supersymmetry in the TeV range is the hierarchy problem [3]: why is $m_W \ll m_P$? where m_P is the Planck mass of about 10^{19} GeV, the energy where gravitational forces become as strong as the other interactions, and the only known candidate for a fundamental energy scale in physics. Alternatively, why is $G_N = 1/m_P^2 \ll G_F = 1/m_W^2$? Or why is the Newton potential inside an atom so much smaller than the Coulomb potential: $G_N m^2/r \ll e^2/r$? Supersymmetry does not by itself explain the origin of this hierarchy, but it can stabilize the hierarchy if supersymmetric particles appear with masses below about 1 TeV. Other reasons for liking accessible supersymmetry include the help it provides to enable the gauge couplings to be unified as shown in Fig. 4 [4], its prediction of a relatively light Higgs boson [15], and the fact that it stabilizes the effective Higgs potential for small Higgs masses [16].

There are important constraints on supersymmetry from the non-observation of supersymmetric particles at LEP and the Tevatron, the absence of the Higgs boson at LEP, the agreement of $b \to s\gamma$ measurements with the Standard Model and measurements of the anomalous magnetic moment of the muon [6]. Also very important is the relic density $\Omega_\chi h^2$ of the lightest supersymmetric particle χ [5],

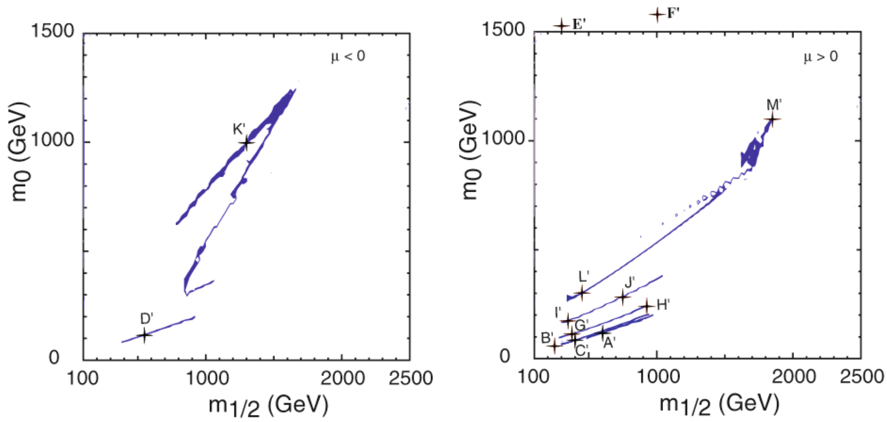

Fig. 5. The strips of supersymmetric parameter space allowed by WMAP for different values of tanβ. The crosses indicate specific benchmark scenarios that have been studied in more detail [18, 19]

which has recently been constrained more strongly by the WMAP satellite [17]: $0.094 < \Omega\chi h^2 < 0.124$, assuming that it constitutes most of the dark matter in the Universe.

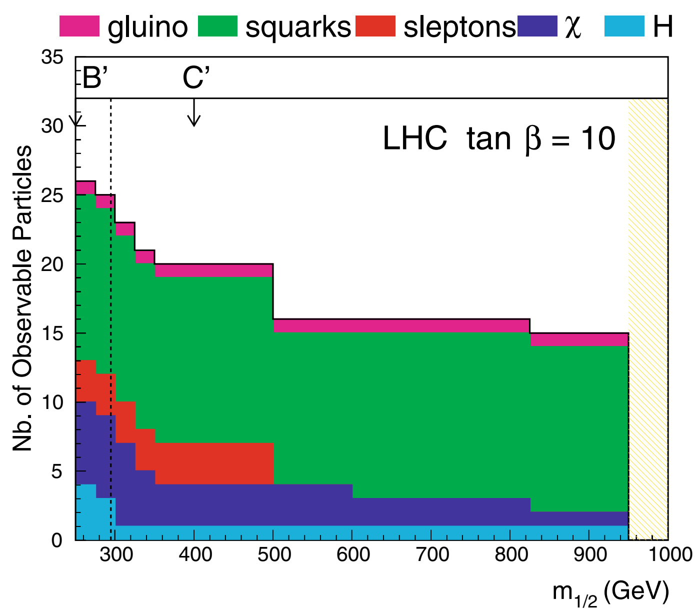

Fig. 6. The numbers of sparticles detectable along a WMAP line, as a function of an input supersymmetric fermion mass, $m_{1/2}$, which is about 2.4 times larger than the mass of the lightest supersymmetric particle

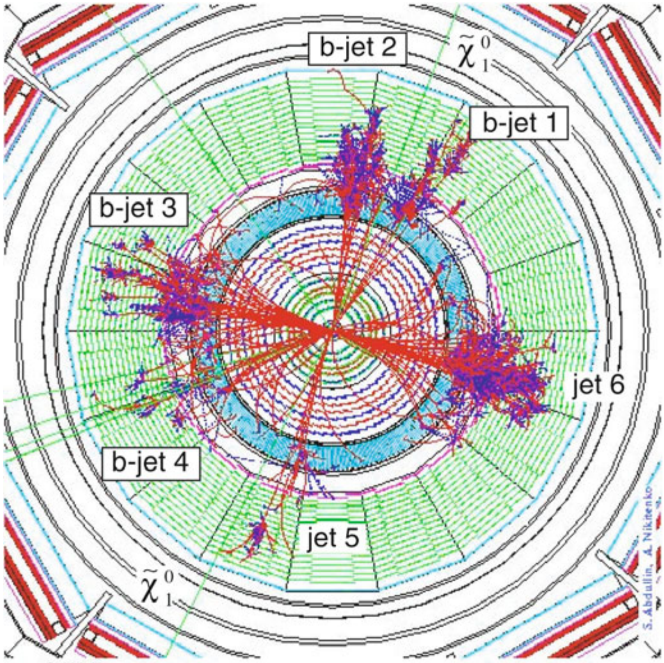

Fig. 7. Simulation of a "typical" supersymmetric event in the CMS detector

As seen in Fig. 5, narrow lines in the supersymmetric parameter space are allowed [18] by the accelerator constraints and the WMAP data, and the detectability of sparticles along one of these WMAP lines is shown in Fig. 6 [19]. In typical supersymmetric scenarios, the LHC discovers many sparticles and one or more Higgs bosons, via cascade decays of heavy sparticles [20] such as that simulated in Fig. 7. In suitable cases, the decay chain can be reconstructed and several of the sparticle masses measured. The quality of LHC measurements at specific benchmark [21] points located along these WMAP lines has been explored in more detail, and it seems they would provide inputs sufficient to calculate the relic density with an error comparable to the WMAP estimate, at least in some cases. The LHC is almost "guaranteed" to discover supersymmetry if it is relevant to the hierarchy and dark matter problems.

4 The quest for extra dimensions

These were suggested originally by Kaluza and Klein in attempts to unify gravity and electromagnetism. More recently, it has been realized that extra dimensions are required for the consistency of string theory, and could help unify the strong, weak and electromagnetic forces with gravity if they are much larger than the Planck length [22]. In other scenarios, extra dimensions could originate the breaking of supersymmetry [23], or enable a reformulation of the hierarchy problem [24].

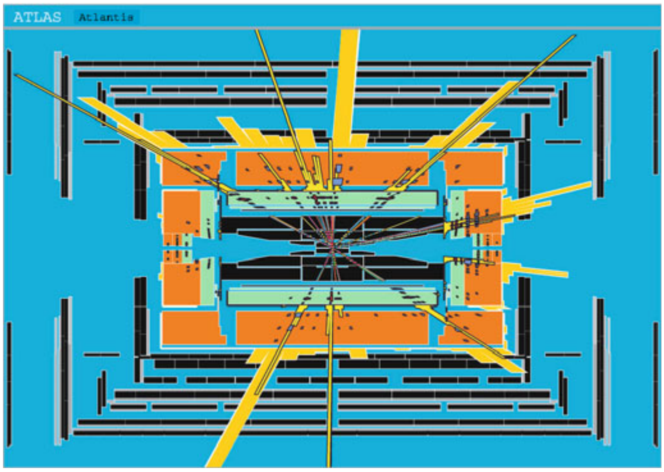

Fig. 8. An ATLAS simulation of a black hole production event at LHC

Possible signatures of extra dimensions could include a diphoton graviton resonance, if gravity "feels" the extra dimensions, or a dilepton Z boson resonance, if the electroweak gauge interactions feel them. In some scenarios with extra dimensions, gravity becomes strong at the TeV scale and black hole formation may form and then decay via Hawking radiation, emitting many jets and leptons, as seen in Fig. 8.

The LHC also has great capabilities for finding the new strongly-interacting particles predicted by some composite "technicolour" models of electroweak symmetry breaking, or of detecting composite structure inside quarks. All in all, the LHC has unparalleled reach for finding new physics at the TeV scale, as shown in Fig. 9.

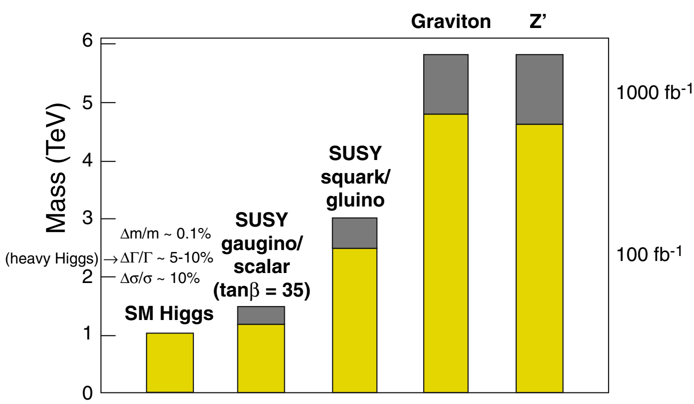

Fig. 9. CMS estimates of the reach for new particles at the LHC. The LHC luminosity upgrade extends the mass reach by about 20%. For the SM Higgs there is complete coverage of the mass range up to 1 TeV

5 The quest for the quark–gluon plasma

Relativistic heavy-ion collisions at the LHC are expected to create effective temperatures of the order of 600 MeV, which are far above the critical temperature of about 170 MeV for the quark–hadron phase transition that has been found in lattice calculations.

Previous experiments at the CERN SPS and RHIC have already found evidence that hadronic matter changes its nature around 170 MeV, and the LHC should be able to tell us what lies beyond the quark–hadron phase transition, recreating conditions in the first microsecond of the Universe with "Little Bangs".

As seen in Fig. 10, among the signatures that the dedicated experiment ALICE [25] plans to explore are $\pi\pi$ interferometry – that can determine the size and expansion rate of the little fireball, the abundances of strange particles – that are expected to increase near the transition temperature [26], J/ψ production – that is sensitive to Debye screening in a plasma [27], and jet quenching – that could be due to parton energy dissipation during propagation through a plasma. All these signatures are to be explored in a hostile environment where thousands of particles are produced in each collision.

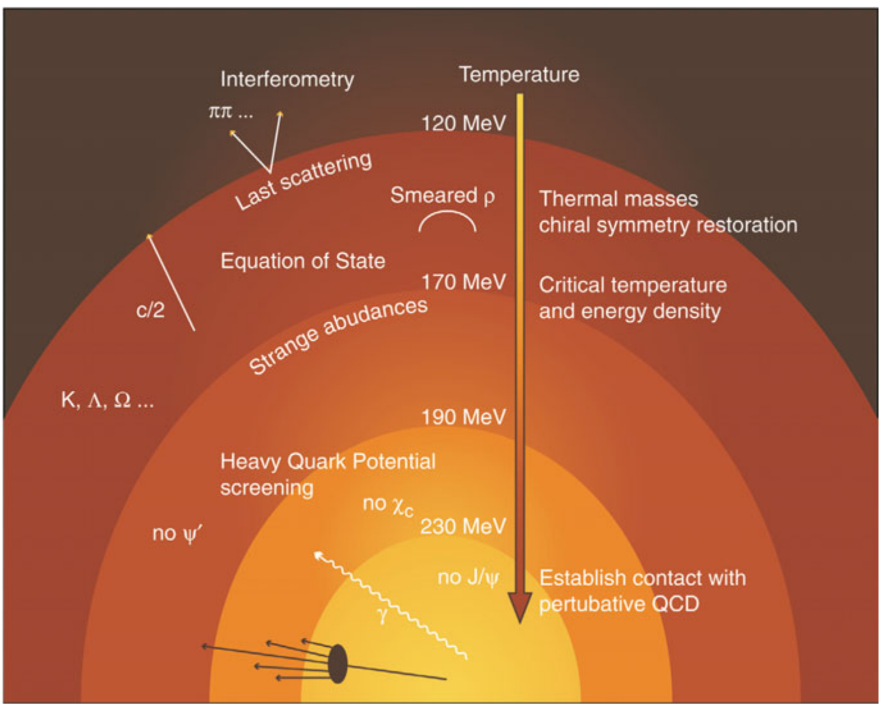

Fig. 10. Possible signatures of the quark–gluon plasma in relativistic heavy ion collisions

ALICE plans to measure J/ψ and Υ production in both the central region (using e^+e^- decays) and towards the forward direction (using $\mu^+\mu^-$ decays), and to compare the J/ψ production with open charm production, to see whether there is any significant suppression. ATLAS and CMS may also contribute to the studies of heavy-ion collisions: for example, CMS can study Z bosons produced with large transverse momenta, and look whether there is a jet on the opposite side, or whether it has been quenched [8].

6 The qest for CP violation beyond the Standard Model

So far, measurements of quark mixing angles and CP violation in the decays of K and B mesons agree well with the Standard Model and its Kobayashi–Maskawa mechanism, though there are some puzzles, notably in B $\to$ ΦK and $\pi\pi$ decays. In 2007, when the LHC comes into operation, not all the angles of the CP-violating unitarity triangle will have been measured accurately. It will fall to the LHC to carry further these tests of the Standard Model, and perhaps provide a glimpse beyond it. There have been many suggestions how new physics, such as supersymmetry, might show up in studies of CP violation in mesons containing b quarks [28].

These possibilities will be explored at the LHC by a dedicated experiment, LHCb [29], as well as by ATLAS and CMS. There are some channels where the LHC will provide a significant increase in the available statistics, such as $B \to J/\psi K$ and $\pi^+\pi^-$ decays, as seen in Fig. 11. There are other channels where LHCb may be able to make the first measurements, such as $B_s \to D_s K$ decays, enabling the unitarity triangle to be overconstrained. The stakes are high: the CP violation present in

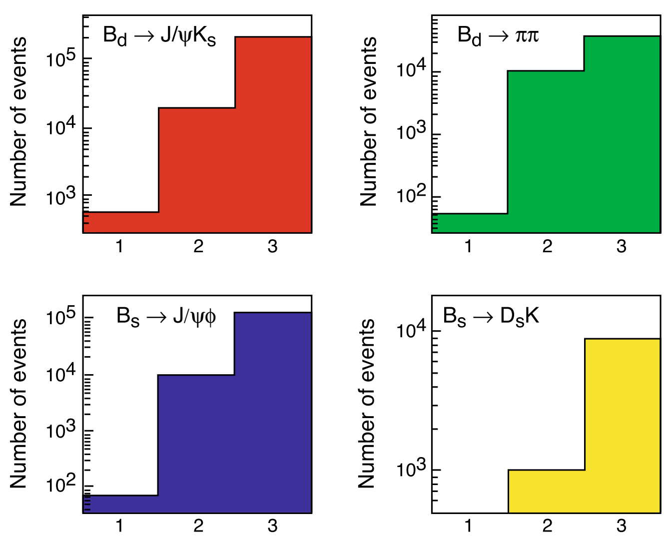

Fig. 11. Present, pre- and post-LHC statistics for interesting CP-violating decays of B mesons, shown respectively in bins 1, 2 and 3 of the histograms

the Standard Model is apparently unable to explain the origin of the matter in the Universe. This would require some extension of the Standard Model, which might be found at the LHC.

7 The LHC will explore new dimensions of physics

The LHC will explore a new dimension in *energy*, up to the TeV scale [30]. There are good reasons to think that the origin of particle masses, a Higgs boson or its replacement, will be revealed in this energy range. The LHC will also explore new dimensions of *space*. These might be additional curled-up versions of the more familiar *bosonic* dimensions, or they might be more novel *fermionic* "quantum" dimensions, that appear in the formulation of supersymmetry in "superspace". The LHC will also explore a new dimension of *time*, recreating particles and events that occurred just 10^{-12} sec after the beginning of the Big Bang. This time travel should reveal to us the nature of the primordial "soup" that filled the Universe before nuclear particles were born. It may also reveal the nature of dark matter, and perhaps also hints about the origin of matter itself.

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First published in Eur. Phys. J. C 34, 51–56 (2004)
Digital Object Identifier (DOI) 10.1140/epjc/s2004-01766-8

Lyndon Evans

Challenges of the LHC: the accelerator challenge

The LHC is a project that faces – or has faced – challenges at each stage. Here I would like to focus on particular challenges in the three phases of approval, construction and operation.

1 The challenge of project approval

It is generally considered that the starting point for the LHC was an ECFA meeting in Lausanne in March 1984 [1], although many of us had begun work on the design of the machine in 1981. It took a very long time – 10 years – between then and project approval. During most of this time Giorgio Brianti led the LHC project study. However we should not forget the enormous debt we have to Carlo Rubbia in the second half of that decade, in holding the community together – the particle physics community and the accelerator community – behind the LHC, against all the odds.

The first project approval came in December 1994, although under such severe financial constraints that we were obliged to make a proposal for building the machine in two stages, which would have been a terrible thing to do, but at that point we had no alternative. However, after a major crisis in 1996, where CERN had a rather severe budget cut, at least the constraints on borrowing were relaxed, and a single-stage machine was approved. The first operation of the LHC is now foreseen for spring 2007. It has been a very long road indeed.

2 The challenge of project construction

It is very clear that building the LHC is a very challenging project [2]. It is based on 1232 double aperture superconducting dipole magnets – equivalent to 2464 single dipoles – which have to be capable of operating at up to 9 T. We were doing R&D on these magnets in parallel with constructing the machine and the experimental areas. This was not just a question of building a 1-m scale model with very skilled people here at CERN, but of being able to build the magnets by mass production, in an industrial environment, at an acceptable price. This is something we believe we have achieved.

The machine also incorporates more than 500 "2-in-1" superconducting quadrupole magnets operating at more than 250 T/m. Here our colleagues at Saclay have taken on a big role in designing and prototyping the quadrupoles very successfully. There are also more than 4000 superconducting corrector magnets of many types. Moreover, operating the machine will involve cooling 40,000 tonnes of material to 1.9 K, below the lambda point of helium.

An additional challenge has been to build the machine in an international collaboration. Although usual for detectors, this was a "first" for the accelerator community, and it has proved an enriching experience.

Production of the superconducting cable for the dipoles has driven the final schedule for the LHC, because we have to supply the cable to the magnet manufacturers. We could not risk starting magnet production too early, when we were not sure that we could follow it with cable production. Figure 1 shows the ramp up of cable production, which has now reached its required plateau. The final schedule for machine startup in spring 2007 was fixed once we were confident in reaching this plateau. This schedule is also well matched to the construction of the detectors.

The next step is the series production of the dipoles, with installation in the tunnel starting in January 2004 and finishing in summer/autumn 2006. The "collared

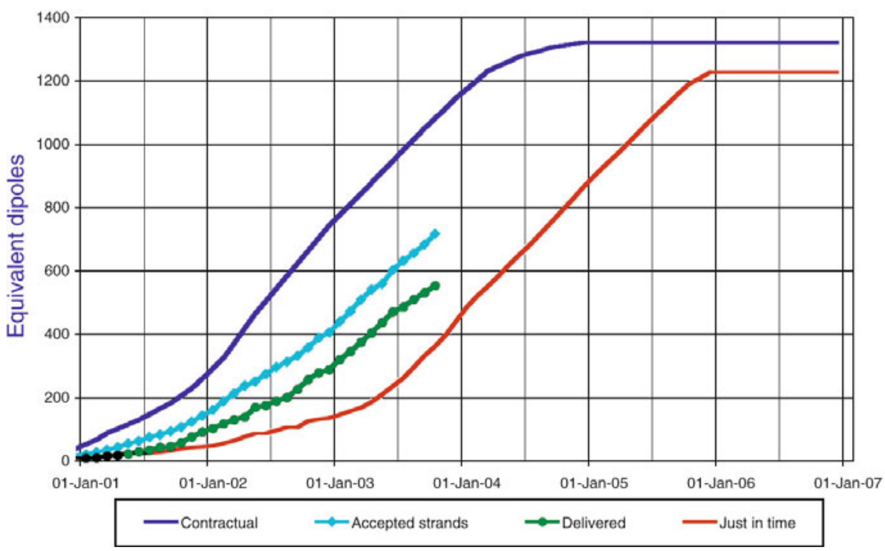


Fig. 1. The production of cable for LHC superconducting magnets

coils" – more than half the work on the dipoles – are now being made at the rate we need. These collared coils are assembled into the cold masses, which are delivered to CERN where they are installed in their cryostats, tested and stored. More than 100 dipole cold masses are now at CERN, and we are confident that we will be very close to the final date for installation.

At the same time the infrastructure of the tunnel is being prepared for the installation of the superconducting magnets. Sector 7–8, the first sector to be instrumented, now has its piping and cabling installed. The next step is installation of the cryoline, to provide the liquid helium refrigeration. This must be finished by the end of 2003 so that we can begin installing dipoles in January. We are now looking forward to as smooth a passage as possible from installation into commissioning.

3 The challenges of operation

The LHC is a very complicated machine, and there are many challenges in its operation. The most fundamental ones concern the beam–beam interaction and collimation. In designing a particle accelerator, we try to make sure that the magnets have as little non-linearity as possible, that is, they have pure dipole and quadrupole fields. We then introduce controlled non-linearities – sextupoles to control chromatic aberrations and octupoles to give beam stability (Landau damping). But we always make sure that we do not introduce any harmonics. We want smooth, distributed non-linearity, not a "lumped" linearity at one point in the ring. So we take a great deal of care, but then we are stuck with what we absolutely do not want – the beam–beam interaction itself. When the beams are brought into collision, a particle in one beam sees the Coulomb field of the other beam, which is strongly non-linear and is lumped – in every revolution the particle sees the beam–beam interaction at the same place [3]. This produces very important effects, as I shall describe.

First, however, I should mention that the conversion of the Super Proton Synchrotron (SPS) into a proton–antiproton collider was a vital step in understanding this phenomenon – indeed, it is not generally known what a step into the unknown we took with the collider. In this machine the strength of the beam–beam interaction – which we call the beam–beam "tune shift" – was very large, much larger than at the Interesting Storage Rings (ISR). The collider was to operate in a domain where only electron-positron machines had worked, and these machines have the enormous advantage of strong synchrotron radiation damping: particles that go through large amplitudes are "damped" into the core of the beam again. So we were going to operate a machine with no damping and a strong beam–beam effect. (Indeed, tests at SPEAR at lower and lower energies with reduced damping showed catastrophic effects, which when extrapolated indicated that the proton–antiproton collider could never work!)

Figures 2a and b show the effects in a simulation of the transverse phase space – the position-velocity space – of a particle in a perfect machine, apart from the beam–beam interaction. At small amplitudes there is harmonic oscillation, but because of the beam–beam non-linearity the frequency varies with amplitude, and at some amplitude

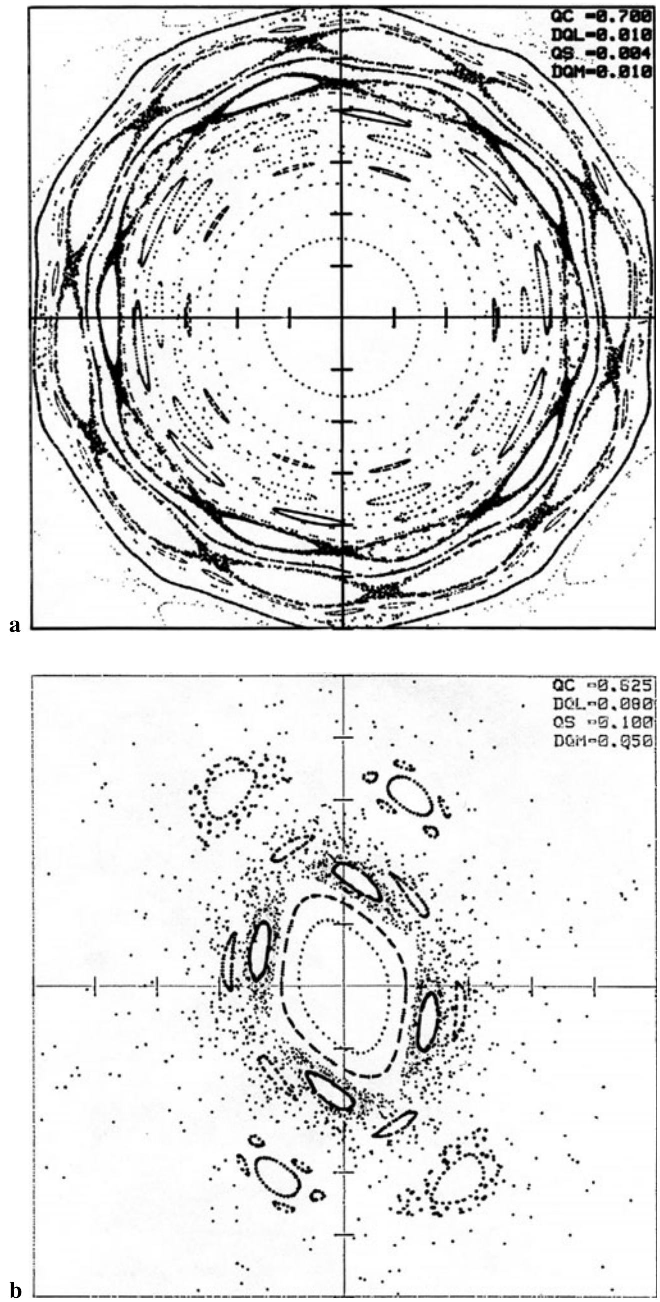
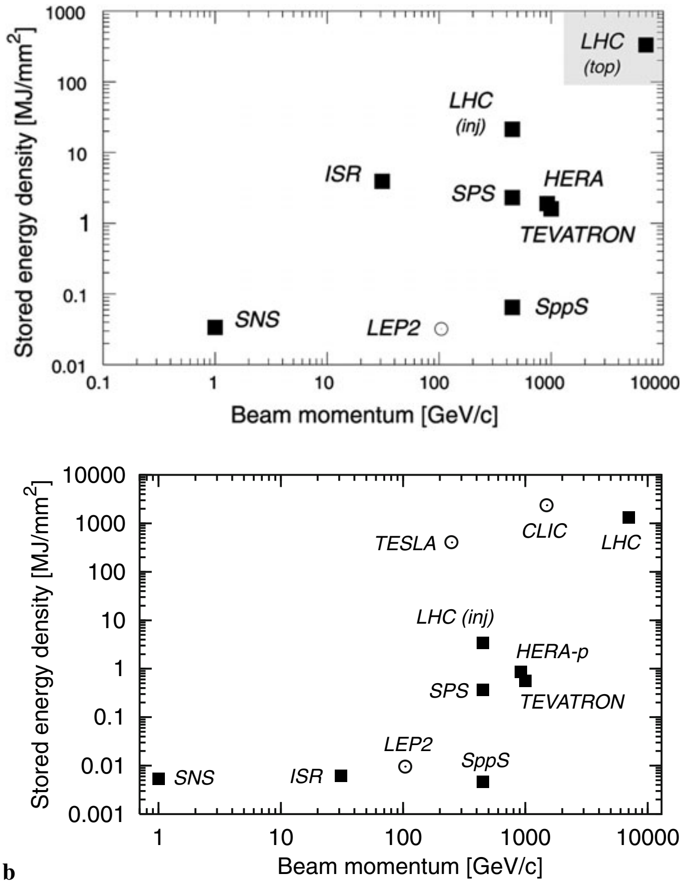


Fig. 2. **a** Simulation of position versus velocity of particle in a perfect LHC. The ten "islands" of a 10th order resonance. **b** Simulation of the chaotic motion created by beam–beam interaction at the LHC



a

b

Fig. 3. **a** Energy stored in the accelerator beam, as a function of beam momentum. At less than 1% of nominal intensity LHC enters new territory. Machine damage (e.g. collimators) and quenches must be avoided. **b** Stored energy density as a function of beam momentum. Transverse energy density is a measure of damage potential and is proportional to luminosity! Collimators must survive expected beam losses

higher order non-linear resonances appear. Figure 2a shows the ten "islands" of a 10th order resonance. The situation is further complicated by synchrotron motion. This produces synchro-betatron resonances, which in turn create a side-band island structure, with much higher order resonances, again visible in Fig. 2a. This, then, is the complicated phase space in the presence of the beam–beam interaction. As you increase the strength of the non-linearity the size of the islands expands and the logical question is what happens when they touch? Figure 2b shows the result – we get chaotic motion.

This was a real worry at the proton–antiproton collider, which proved to be an absolutely essential prototype for defining the parameters of the LHC. We have designed the LHC to beat this effect by sitting in a very small corner of "tune space" with very precise control in order to stay away from high order resonances. So we have designed the machine such that we are in a parameter space that we have already visited, although the beam–beam interaction will always be a fundamental limit. The tune shift is proportional to luminosity and there will always be a tendency to push it to the limit.

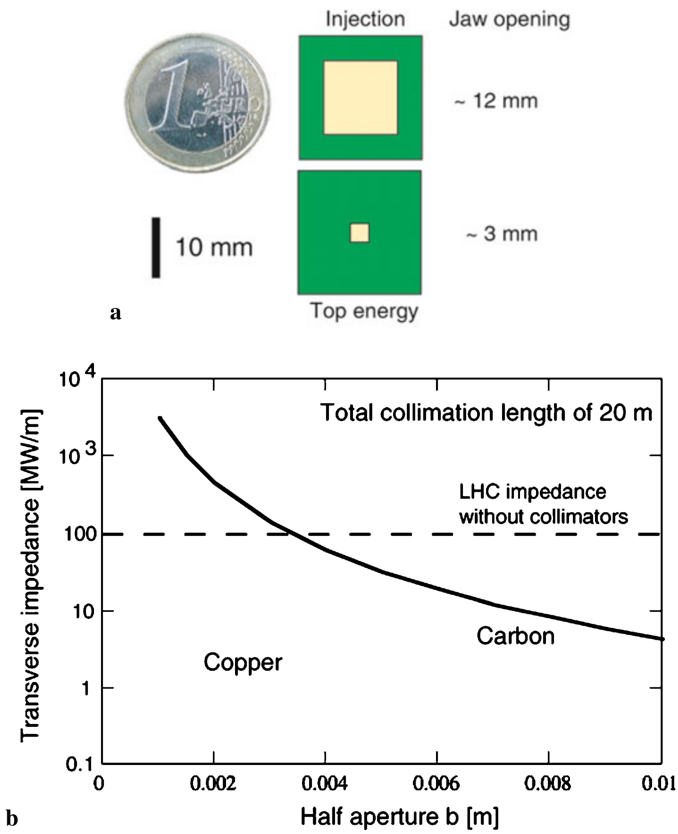


Fig. 4. Collimating with small gaps. **a** LHC beam will be physically quite close to collimator material and collimators are long (up to 1.2 m)! **b** The machine impedance increases while closing collimators (Carbon curve). LHC will operate at the impedance limit with collimators closed!

A second major challenge in operating the LHC concerns collimation [4], which is needed to remove halo particles from the beams, in order to avoid their touching the superconducting magnets, and to control the background in the detectors. We also need collimation to protect the machine in the phenomenal intensity in the LHC, and to protect against fault conditions – the stored energy in the nominal LHC beam is equivalent 60 kg of TNT! If there is a fault the beam will be kicked out, and for that there is a 3 microsecond hole in the bunch spacing to allow the field in the kicker magnets to rise. If there is a misfiring, particles will be lost as the kickers rise, and the collimators can melt, so they have to be very carefully designed.

Already, at less than 1% of its nominal intensity, the LHC will enter new territory in terms of stored energy. As Fig. 3a shows, the LHC is two orders of magnitude more in stored beam energy. But the beam energy density is three orders of magnitude higher (Fig. 3b) because as it is accelerated the beam becomes very small. To cope with this we have designed a very sophisticated collimation system. At injection the beam will be big, so we will open up the collimators to an aperture of about 12 mm, while in physics conditions the aperture of the beam will be 3 mm – the size of the Iberian Peninsula on a one euro coin. The beam will be physically close to the collimator material, and the collimators themselves are up to 1.2 m long. As Fig. 4 shows the machine impedance increases while closing the collimators, and once the collimators are closed down, the LHC will operate at the impedance limit!

4 Conclusions

We are now on the final stretch of this very long project. Although there are three and a half years to go, they will be very exciting years as we install the machine and the detectors. It is certainly going to be a big challenge both to reach the design luminosity and for the detectors to swallow it. However, we have on the project a competent and experienced team, and we have put into the machine design 30 years of accumulated knowledge from previous projects at CERN, through the ISR and proton–antiproton collider. We are now looking forward to the challenge of commissioning the LHC. It will be there in spring 2007.

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First published in Eur. Phys. J. C 34, 57–60 (2004)
Digital Object Identifier (DOI) 10.1140/epjc/s2004-01767-7

Jos Engelen

Challenges of the LHC: the detector challenge

1 Introduction

The quote I remember must date from the mid 1980's or a little bit later. It says: "we think we know how to build a high energy, high luminosity hadron collider – we do not have the technology to build a detector for it; for a high energy, high luminosity linear electron–positron collider the situation is just the opposite". Clearly the decision was taken to first choose the former of these "impossible" routes towards new discoveries and to, somehow, make the necessary progress in detector technology to allow detection and analysis of complex and rare final states resulting from proton–proton collisions at very high energy.

As is illustrated in the presentation by Lyn Evans at this symposium, the claim that the technology for building a very high energy hadron collider was already available at the time of the statement quoted was a serious simplification of reality; after years of hard work this technology now is available and the Large Hadron Collider is well underway towards first collisions of 2×7 TeV proton beams in 2007.

The LHC detectors are radically different from their predecessors at the $Sp\bar{p}S$ collider, LEP, SLC, HERA, Tevatron, etc.: they are designed for a luminosity of 10^{34} cm^{-2} s^{-1} for pp collisions at an energy of 14 TeV in the center of mass reference system, so the detectors need to be fast, radiation hard (also the electronics) and big.

ATLAS and **CMS** took up the challenge to elucidate electroweak symmetry breaking, find "the" Higgs boson and more;

LHCb took up the challenge to exploit the prolific production of b-quarks in the forward direction to study CP violation and rare decays;

ALICE took up the challenge to explore the properties of QCD matter at extreme energy densities (the quark–gluon plasma) over a large, new region of its phase diagram;

TOTEM took up the challenge to accurately measure the total cross section.

2 The ATLAS and CMS detectors

ATLAS and CMS are 4π "general purpose" detectors (Fig. 1 and Fig. 2 respectively). They will see 20 to 40 events per bunch crossing, i.e. every 25 ns, leading to 10^9 events per second and to something like 10^{11} to 10^{12} tracks per second. It is really remarkable and quite a step from what could be anticipated when first discussions on the design of these detectors started, that ATLAS and CMS will, in this environment: – reconstruct secondary vertices from B mesons and τ leptons, only mm's away from the primary vertex; – reconstruct individual photons with sufficient energy and angular resolution for detection of a light Higgs boson decaying in two photons. In addition, these detectors have many more capabilities. As stated above they are "general purpose" 4π detectors featuring tracking, magnetic momentum analysis, calorimetry, muon spectrometry in an, almost, *hermetic* setup. (Incidentally: the importance of hermeticity was emphasized by the pioneering $Sp\bar{p}S$ experiments, the achievements of which we are celebrating at this symposium.)

We will not extensively review the layouts and design choices of ATLAS and CMS here; we will discuss some of their characteristics, however.

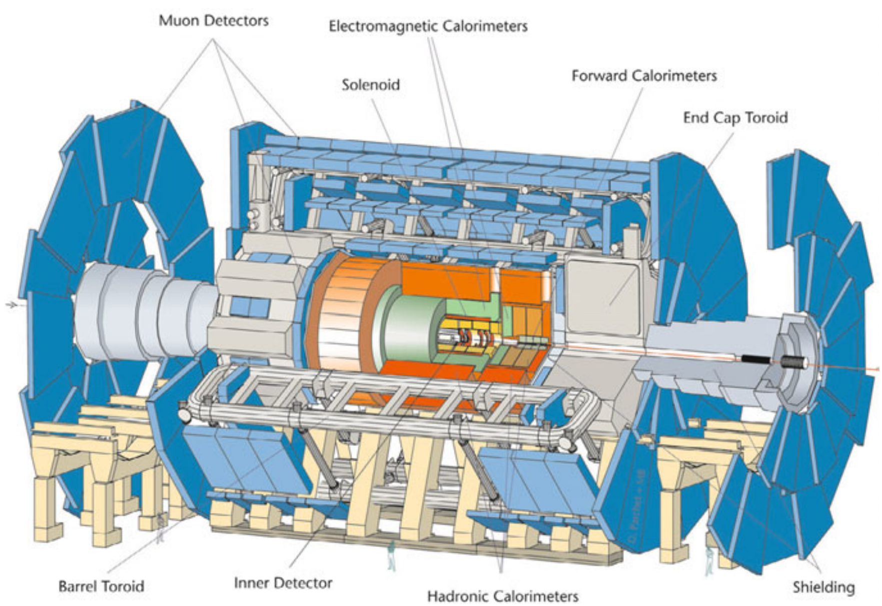

Fig. 1. A schematic view of ATLAS, a low density, general-purpose detector at LHC

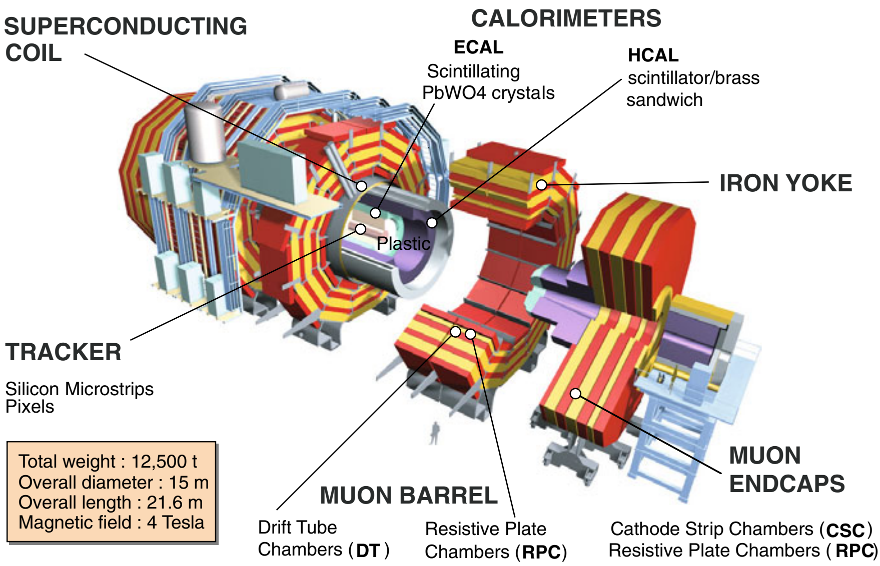

Fig. 2. A schematic view of CMS, a compact, general-purpose detector at LHC

A remarkable feature of ATLAS is its huge air core toroid muon spectrometer with stand-alone capabilities for momentum measurement. This will allow accurate reconstruction, in particular of muons with high transverse momentum, e.g. resulting from the decay of a very heavy Higgs boson. This spectrometer makes the ATLAS setup very large with a length of 46 m and a diameter of 25 m. The specific weight, however, is only 300 mg/cm^3.

CMS, the Compact Muon Solenoid, is characterized by a large, 6 m bore, central solenoid with a 4 T magnetic field, containing tracking and calorimetric devices. With a diameter of 15 m and a length of 22 m this setup is relatively "compact", with a specific weight of 3 g/cm^3.

One of the most important developments for the instrumentation of the LHC detectors is in the field of Silicon sensors and the associated electronics. For example: the innermost pixel detector layer will typically be exposed to 10^5 Gy/year due to ionizing radiation and to 1.6 10^{14} n/cm^2/year.

Radiation hardness of sensors has been achieved empirically, there are many parameters that can be varied – crystal cut orientation (<100>, <111>); geometry of implants; pixel dimensions, pitch of microstrips; temperature; improvement of production methods; etc. Increasing depletion voltage can (up to a limit) compensate for signal loss.

Radiation hardness of electronics can be achieved by using special rules and processes, but there was a very pleasant "coincidence": the 0.25 µm technology appears to be intrinsically radiation hard (and will be widely used by LHC experiments, not only for pixel detectors) – even though ATLAS also uses DMILL electronics.

CMS has taken the drastic and, in a sense, revolutionary step of opting for an "all silicon" tracker consisting of barrel and end-cap detectors, providing of the order of 10 high precision points per track. The barrel consists of 3 pixel layers, 4 inner and 6 outer microstrip layers. The availability of large wafers (6") was crucial for the decision to also produce the outer layers of silicon.

The ATLAS tracker consists of pixel and microstrip silicon detectors (3 + 4 layers) completed with a Transition Radiation Tracker (TRT). The TRT provides track coordinates with a lower precision than the semiconductor tracker, but provides a large number of measurements for each track. It is produced from small diameter (4 mm) straw tubes. The layers of straw tubes are interspersed with poly-ethylene foam or foils where electrons generate transition radiation, also detected by the straw tubes, providing electron–pion separation at high energies.

Great care had to be taken to limit the amount of material in the trackers to the absolute minimum in order to limit multiple scattering and photon conversions as much as possible. Among others, this led to the design of advanced light weight support structures of composite material. This typically resulted in an average of 50–60% of X_0 in the central active volume.

As already indicated above, the successful implementation of the pixel and other read-out chips in 0.25 μm technology was a great success, leading to the required radiation hardness and to cost effectiveness.

Both ATLAS and CMS have developed new concepts in electromagnetic calorimetry. The detection of a relatively light Higgs boson (120 GeV) decaying into two photons requires electromagnetic calorimeters of exceptional performance. ATLAS will have an electromagnetic calorimeter with very high granularity and longitudinal sampling – providing directional information for individual photons – and good energy resolution ($\Delta E/E = 9\%/E^{1/2}$) (calorimeter placed outside the central solenoid); CMS will have an electromagnetic calorimeter with very good energy resolution ($\Delta E/E = 3\%/E^{1/2}$) and good granularity (calorimeter placed inside the central solenoid).

ATLAS has developed an electromagnetic calorimeter based on liquid argon technology, with essentially no dead space. The latter is achieved by employing "accordion" electrodes and absorbers, that zigzag along the particle's flight direction. Mechanical and electrical design and construction of absorbers and electrodes was extremely challenging, but all specifications have been achieved.

CMS has invested in a long and intensive R&D program on $PbWO_4$ as a crystal for high resolution electromagnetic calorimetry. The challenge was to produce crystals with the required properties (radiation hardness, reproducibility, light yield, uniformity) at an affordable price, starting from cm^3 samples leading to a m^3's calorimeter. Also here the goals have been achieved and crystals are being produced at a steady rate.

This brief presentation is not the place to give a comprehensive overview of the achievements and the status of these very large, complex, state of the art detector systems. Both experiments are now well into the production phase and although there are still many challenges ahead we may optimistically look forward to first data taken with these detectors in 2007, at the start up of LHC.

For both ATLAS and CMS new, large underground caverns had to be excavated. The ATLAS caverns have been handed over to the collaboration recently. The CMS detector will be largely assembled on the surface, the experimental cavern will be handed over to the collaboration in 2004. As a recent example of the "surprises" one can encounter in large civil engineering projects we mention the water leaks that developed in the two CMS access shafts, as a consequence of the settling of the underground halls. It is clear that unexpected events like these (there are many more and certainly not only in civil engineering) require the utmost resourcefulness and flexibility of the collaborations in order to minimize delays.

Among the remaining challenges ahead, assembly, installation and integration are the most immediate ones. For example: CMS recently successfully tested the insertion of the 220 ton central coil (using a "dummy" of course) inside the vacuum tank shell: a "heavy duty" but very delicate operation. ATLAS is presently integrating its 25 m long barrel toroid coils, a complex operation. In one year's time, i.e. towards the end of 2004, these large and heavy devices (there are eight in total) will have to be installed in the ATLAS cavern, filling it to the roof.

3 The LHCb detector

The LHCb experiment (Fig. 3) will exploit the Large Hadron Collider as a B-factory (including B_s, B_c – not produced at the presently running e^+e^- B-factories – and also b-baryons). Its design is optimized for the detection of B mesons in the "forward" direction. Dynamics (mainly gg fusion) and kinematics (Lorentz boost) lead to a "one arm spectrometer" design, rather unusual at a collider. Due to the large b production cross section at LHC energy, the luminosity at the LHCb interaction point will be tuned at 2×10^{32} cm^{-2}s^{-1}. The main challenges for this experiment are: trigger, sensitive to multibody hadronic final states; particle identification (K/π separation) over a large momentum range and tracking (vertexing), the latter allowing a proper time resolution of the decaying B mesons of 40 fs.

4 The ALICE detector

The LHC will collide Pb beams at 2.75 TeV per nucleon: this should, in central collisions create the extreme temperature and density required for producing a plasma of quarks and gluons. In order to investigate the many facets of this unusual state of matter, in a single dedicated experiment at LHC, ALICE (Fig. 4) will have to study a diverse set of observables, needing a great variety of sub-detectors, not all of them requiring full angular coverage, but providing unique particle identification. Rather than "rate", the problem for ALICE is "occupancy" and "data volume", as one central Pb–Pb collision will produce one thousand times more particles than a typical pp collision.

The greatest instrumental challenge certainly is the central, large Time Projection Chamber (88 m^3) and its associated electronics (570,000 channels). Combined with a six layer, silicon vertex detector, it will provide an excellent momentum resolution

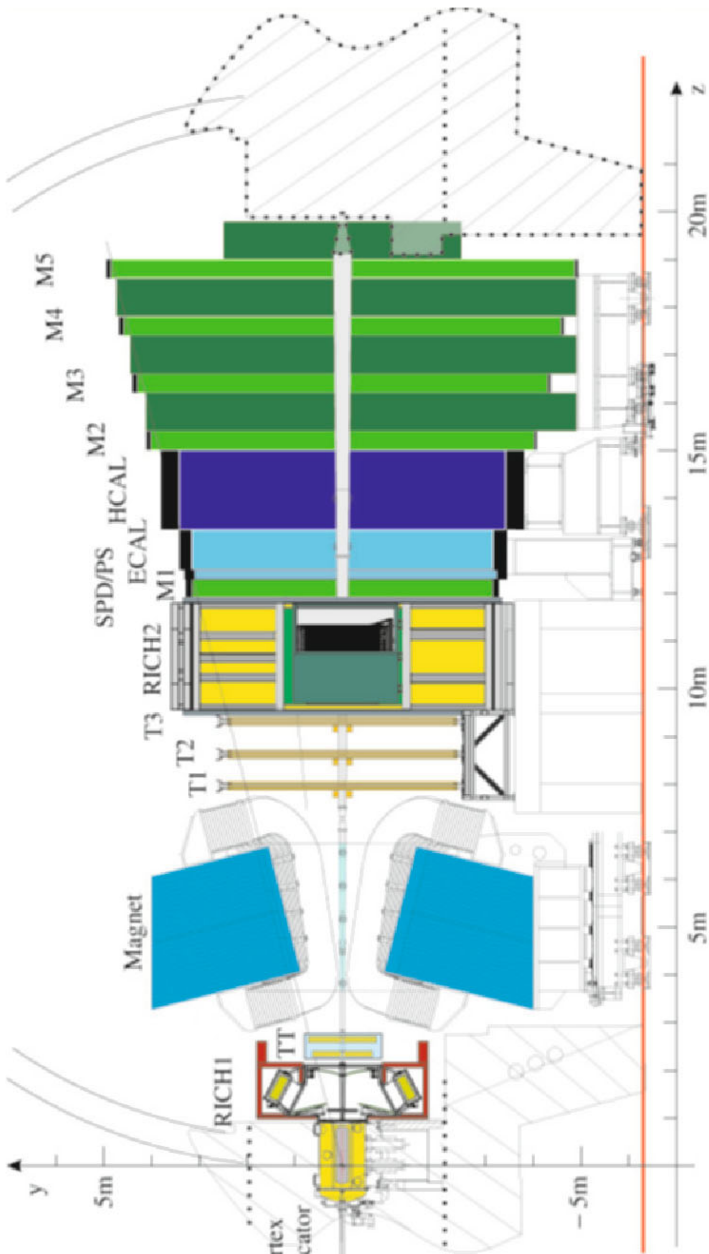

Fig. 3. A schematic view of the LHCb detector at LHC, optimised for B physics in the forward direction

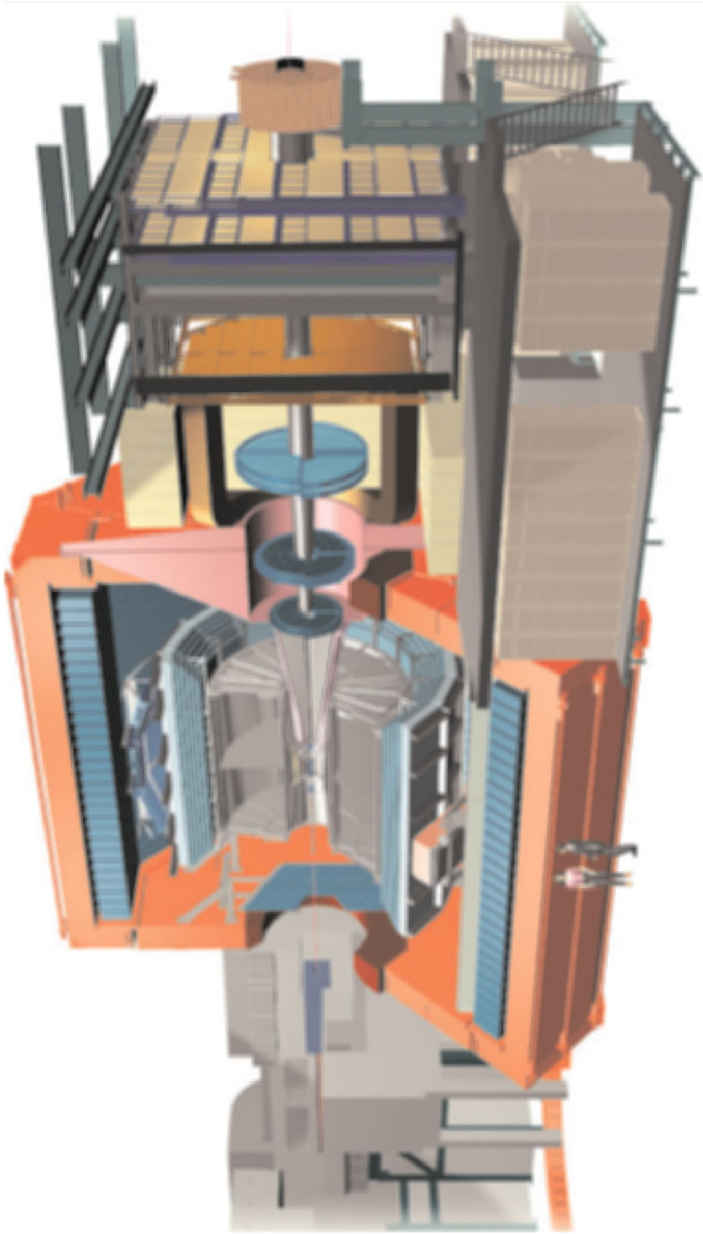

Fig. 4. A schematic view of the ALICE detector at LHC, optimised for the study of heavy ion collisions

over a broad range from 100 MeV/c to above 100 GeV/c. A further remarkable feature of the ALICE detector is the integration of a "muon arm" (for J/ψ and Υ detection) in the setup.

In addition to Pb beams, the ALICE detector will study collisions with lighter ions, proton–nucleus interactions as well as a number of topics in pp reactions where its unique coverage of soft and semi hard observables combined with particle identification are relevant.

5 Looking forward to ...

We are all eagerly awaiting the startup of the LHC and the data taking of the LHC experiments in the second trimester of 2007. Discovery of "the" Higgs boson, investigation of electroweak symmetry breaking are of course on top of the priority list. Even moderate (!) initial luminosities of 10^{33} cm^{-2}s^{-1} will open a promising window on new physics. For example, if (low-energy) super-symmetry is realized in nature, this could be one of the first discoveries at the LHC, perhaps already within the first 6 months.

Acknowledgements. I thank Jean-Pierre Revol for the perfect organization of this very interesting Symposium. I thank Jim Virdee, Peter Jenni, Michel Della Negra, Tatsuya Nakada and Jurgen Schukraft for their help in preparing this presentation.

First published in Eur. Phys. J. C 34, 61–65 (2004)
Digital Object Identifier (DOI) 10.1140/epjc/s2004-01768-6

Paul Messina

Challenges of the LHC: the computing challenge

1 LHC's computing needs

The LHC will generate unprecedented volumes of data, hence meeting the LHC computing needs will require innovative approaches that involve linking storage and computing resources that are distributed worldwide. The success of this strategy will depend on advancing the state of the art in a number of technologies, primarily in the software realm. This paper deals entirely with the LHC *off-line* computing needs, from raw data to the physics plots (calibration, reconstruction, simulation, analysis).

The current estimates are that the major LHC experiments will store data onto permanent storage at a raw recording rate of 0.1–1 GigaBytes/sec (GB/s). A single copy of the archive is estimated to grow at a rate of 5–8 PetaBytes (PB)/year and at any time 10 PetaBytes of data will reside on disk. (A Petabyte is 10^{15} bytes. In more familiar terms, it takes more than one million CDs to store one Petabyte.) Each of the four LHC experiments will store between 3 and 10 PB on tape. The total data volume will be tens of Petabytes by 2007–8 and an Exabyte (10^{18} bytes) five to seven years later.

The analysis of these data will require tens of thousands of processors (the high-end commodity processors of 2008, not today's), perhaps as many as 100,000 such processors. Thus the sheer scale of the data and the corresponding analysis poses challenges. If one believes the rule of thumb that when something increases by one order of magnitude, it changes in nature, then the LHC computing task must truly require different approaches, since it is several orders of magnitude greater than previous scientific data investigations.

2 Distributed, "grid computing" approach chosen

Given the very large requirements for LHC data analysis, it was not considered feasible to put all of the resources at CERN. MONARC (MOdels of Networked Analysis at Regional Centers for LHC experiments), a collaborative effort of all four experiments, has developed a strategy to meet the LHC needs that uses computing and storage resources at physics research centers (including laboratories and universities) worldwide to tackle the analysis [1]. The LHC community contains more than 5000 physicists, residing in about 300 institutes in about 50 countries. The MONARC approach was endorsed as the appropriate one after a comprehensive review of the LHC computing needs [2].

Under the MONARC model, while CERN will retain a copy of all the data, it will not have the computing capacity to satisfy the needs of the thousands of physicists who will undertake the analysis of the data. Copies of subsets of the data will be sent to the sites that will provide resources for LHC data analysis. Even if CERN were to have sufficient computing power, distributing the data and computing resources is desirable since it reduces the need for repeated transfer of data from a central site (CERN) to each user site.

LHC computing will be done on resources located at a large number of Regional Computing Centers in many different countries, interconnected by fast networks. In other words, the LHC computing services will be implemented as a geographically distributed Computational Data Grid. The participating sites will have varying levels of resources, organized hierarchically in Tiers. An important benefit of this approach is that it enables physicists all over the world to contribute intellectually, without requiring their physical presence at CERN.

To be more specific, a multi-tier hierarchical model similar to that developed by the MONARC project has been adopted as the key element of the LHC computing model. In this model, for each experiment, raw data storage and reconstruction will be carried out at the Tier0 centre, which will be at CERN. Analysis, data storage, some reconstruction, Monte-Carlo data generation and data distribution will mainly be the task of several Regional Tier1 centers, followed by a number of (national or infra-national) Tier2 centers, by institutional Tier3 centers or workgroup servers, and by end-user workstations (Tier4). The CERN-based Tier0 + Tier1 facility will support all LHC experiments whereas some Tier1 centers may be dedicated to a single experiment.

A rough estimate is that the sum of the resources at centers outside of CERN will be twice the resources at CERN and that the sum of the resources at all the Tier1 centers will be equal to the power of the resources at the Tier0 centre, as will be the sum of the resources at all the Tier2 centers.

It is worth noting that some Tier1 and Tier2 centers may well be part of a larger institutional computing facility that serves other user communities, in addition to physicists engaged in LHC experiments. This aspect of the distributed facilities poses some technical and managerial challenges, as will be described later in this article.

To be usable, this distributed, hierarchical set of computing and data storage resources must have software and policies of operation that provide to the user a fairly uniform interface and tools to facilitate the migration of data and analysis runs from one part of the tree to others.

The LHC Computing Grid project (LCG) led by CERN is developing and deploying the software, methodologies, and policies needed to create and operate this distributed, hierarchical computing environment [3].

3 A few words on the network infrastructure

A very capable network infrastructure will be required to support the anticipated data flows among the elements of the LHC global computing environment. The estimated bandwidth between Tier0 and the Tier1 centers is 1.5 to 3 Gbps for a single experiment. The traffic between other pairs of nodes in the distributed systems will be comparable, with lower numbers for the lower tiers.

Fortunately, such a network infrastructure is emerging and is certain to be available when LHC data analysis begins in earnest. The exponential use of the web by industry and the general population led commercial carriers to install a prodigious amount of optic fiber and related equipment, with far more capacity than the current demand. That excess capacity, coupled with advances in optical network technology (such as dense wave division multiplexing) have resulted in steeply declining network prices. Furthermore, largely due to the adoption of Grids by the global high-energy physics community, transoceanic networks for research are becoming much faster; in 2003 there was at least one transatlantic network running at 2 Gb/s, faster than most networks within continents. The European Union has created the GEANT network which provides a European “backbone” network for research and education with 10 Gb/s bandwidth presently and with firm plans for further upgrades in the near future. GEANT connects individual country high-speed research networks (such as RENATER/France, GRNET/Greece, GARR/Italy, FCCN/Portugal, REDIRIS/Spain, SuperJANET/United Kingdom and ACONET/Austria). In total, GEANT and the networks it connects reach almost four thousand institutes in 33 countries. Other world regions have or are putting in place high-speed research networks (e.g., in the United States the Teragrid network (40 Gb/s backbone, 30 Gb/s to individual sites), the Internet2 and Lightrail networks, the high-speed networks created by the CANARIE organization in Canada, CERNET in China, Academic and Research Network in Indonesia, Japan Gigabit Network and SInet in Japan, KOREN in Korea, Research Networks in Malaysia, PHNET and PREGINET in the Philippines, SingAREN in Singapore, Thailand and APAN, the Asia Pacific Advanced Network. Equally important, high-speed transoceanic links are bridging these networks so that there will soon be a global research network infrastructure fast enough and with sufficient connectivity to support LHC data transfer needs.

4 What is grid computing?

The Computing Grid (usually just called “the Grid”) is a powerful concept that provides a unifying principle for many activities in – and infrastructure plans for –

computational science and engineering. It is a premier example of applications-driven research and development that are inspired by the confluence of several technological trends: dramatic advances in network transport, storage devices, and computing power. (The Grid is also quite relevant for commercial applications and many are being pursued, but in this article we will limit ourselves to the world of science.)

5 The grid vision

The "Grid concept" is to enable *resource sharing & coordinated problem solving in dynamic, multi-institutional virtual organizations,* and to do so without requiring central control or omniscience.

A quote from a description of a particular grid project, the Teragrid [http://www.teragrid.org] presents a vision from the perspective of science:

"An exciting prospect for the TeraGrid is that, by integrating *simulation and modeling* capabilities with *collection* and *analysis* of huge scientific databases, it will create a computing environment that unifies the research methodologies of theory, experiment, and simulation."

The name "Grid" or "computing Grid" was chosen based on an analogy with the electrical power grid. Part of the concept was to be able to obtain seemingly unlimited, ubiquitous distributed computing power and access to remote data and to do so transparently, just as one gets electrical power in the office or at home without having to know what generating plant produced the electricity. Of course the computing grid is much more complex because it must provide transparent access to a variety of information technology resources, such as:

- distributed data collections and data bases,
- computers, of many different types,
- instruments with digital output, and
- telecollaboration tools.

Grid Computing has been identified as an important new technology by a remarkable spectrum of scientific and engineering fields as well as by many commercial and industrial enterprises. See for example [4–13].

The widespread adoption of the grid computing paradigm has taken place very rapidly, even faster than was the case for the web. In only a decade since the formulation of the first concepts that led to the Grid [14], there are scores of grid computing projects underway or in the planning stages in dozens of countries and there are even some production grids for both research and commercial applications. What makes grid computing such a compelling concept?

Grid Computing enables or facilitates the conduct of virtual organizations – geographically and institutionally distributed projects – and such organizations have become essential for tackling many projects in commerce and research. With grid computing one can readily bring to bear the most appropriate and effective human, information, and computing resources for tackling highly complex and multidisciplinary projects.

In commerce, grids will facilitate the integration of efforts across large enterprises as well as the contributions of contractors for projects of finite duration. For instance, new services may be provided in health care as well as medical research.

It is becoming apparent that the use of Grids will be an enabler for major advances and new ways of doing science. Grids have the potential to integrate as never before the triad of scientific methods – theory, experiment, and computation – and to do so on a global scale. This integration can be accomplished by providing a unified environment in which one can execute simulations using models based on theory, access relevant experimental data, perhaps obtain instrument data in real time under control of the simulation, and compare the computational and experimental results. Grids also provide a way to greatly increase the number of individuals who analyze observational data, to facilitate telecollaboration, and to provide broader access to unique experimental or computational facilities.

6 A brief history of grids

A brief history of grids may help explain their nature. While distributed computing began several decades ago (and the Grid can be thought of as a form of distributed computing), the essence of the technologies and methodologies that we now refer to as "the Grid" can be traced to the Gigabit Testbed project initiated by Robert Kahn in the late 1980s [15]. The five testbeds in that project (which was funded by DARPA and the US National Science Foundation) dealt with the issues of using high-speed networks to link geographically distant computers, visualization facilities, and data collections. The testbed teams developed hardware, software, and protocols for supporting the very fast networks and interfacing them to computers. In addition, much effort was focused on creating software that would facilitate the *dynamic and simultaneous use of those resources* to support applications such as interactive exploration of multi-sensor data [16], cancer radiation treatment planning, and climate simulation. So we see that from the outset, Grid technologies (often called *metacomputing* in those early days) were driven both by applications and by infrastructure technologies, the latter including fast wide-area networks, large data archives, software and hardware interfaces, and visualization technologies.

The use of grids for high-end scientific computing, while no longer the prevalent use of grid technologies, is by no means dead. A recently formed activity in Europe was formed to do just that. Distributed European Infrastructure for Supercomputing Applications (DEISA) [17] is a consortium of leading national supercomputing centers in Europe aiming to jointly build and operate a distributed terascale supercomputing facility.

By the mid 1990s a confluence of trends and research advances enabled large-scale demonstrations of Grids. The I-Way experiment of 1995 [18] showed that over a dozen systems on multiple wide-area networks could be linked through common software and that many applications could be executed on the ensemble of resources thus created. Soon after, telecollaboration [19] became an additional focus as it was recognized that the Grid would provide good support for many aspects of distributed research collaborations as such approaches become more prevalent. Instruments and sensors were also added to the scope of resources managed by Grid software, thus providing real-time or near real-time access to data from those sources.

In the same time-frame the Web became an everyday tool for many millions of people around the globe. This phenomenon had several effects on the Grid. One was that most people became familiar with accessing remote resources; typically the resources accessed are static documents but some are dynamic. Consequently, the idea of harnessing major remote computational and data resources was no longer quite so foreign. Second, most institutions installed higher speed connections to the internet as demand increased and prices fell. Third, researchers began to put more and more data collections on line and accessible to others, facilitated by the additional trend of rapidly decreasing data storage costs [20].

Projects were formed to conduct research and develop software tools to enable grid computing, notably Condor [21], Globus [22], UNICORE [23], Legion [24], and their products form the majority of the software technology in use today.

By the late 1990s, the confluence of these trends and advances led to the initiation of projects that could only be done on the Grid or ones that reap major benefits from Grid approaches. Among those are the European Data grid [26], and the Digital Sky [26], which led to Virtual Observatory projects such as [9] and the Astrophysical Virtual Laboratory [27]. Work on the software components that implement the Grid concept – by then usually called *middleware* – accelerated as the new applications became operational and revealed shortcomings or missing functionality.

As is often the case in computing trends, technology advances in several fields inspired and enabled grid computing. Dramatic improvements in the cost-performance and reliability of disks have enabled even small research groups to keep many terabytes of data on-line. Sensor technology has advanced as well and scientists are gathering more and more data. A major motivator for the use of Grids is the access they provide to the huge data collections that are being assembled, maintained, and made available electronically by many disciplines. Unlike computing power, such data archives are not so readily replicated at each user site, hence they must be accessed remotely. Furthermore, multidisciplinary investigations often require the simultaneous access of several data collections, each of which is in a different location. Finally, the analysis of the data can require powerful computer systems that are in another location and the visualization of the results of the analysis might require the use of a system at yet another site.

Computer science research projects worldwide are gradually identifying needed functionalities and ways to provide them, as well as creating a body of software and methodologies that include more and more functionality and provide better support for applications. There are also many application-oriented Grid projects, some of which are operational, including some with an international span, that focus on addressing the challenges their application domain poses for the Grid infrastructure.

An indication of the magnitude of the trend towards adopting the Grid as the computing environment for science and engineering is the existence of support for widely used middleware such as Globus [22] by a number of computer hardware and software companies and *commercial* software efforts for systems such as Legion and for supporting Grid applications. Every major computer manufacturer has internal grid projects, some already have commercial offerings, and there are nearly fifty commercial sponsors of the Global Grid Forum, a grid middleware standards body [28]. A few articles and books that provide useful introductions to grids as they are evolving currently can be found in [29–34].

7 Benefits of grid computing

In just a decade, the potential benefits of Grids have become recognized to the extent that some government agencies and commercial companies have adopted them for production use. Grids are seen as a way to greatly increase the number of scientists who will analyze observational data, to federate data bases to enable the study of complex, multidisciplinary issues, to facilitate telecollaboration, and to provide broader access to unique experimental or computational facilities. Many believe that the use of Grids is likely to be an enabler for major advances and new ways of doing science. Certainly Grids will integrate as never before the triad of scientific methods: theory, experiment, and computation.

8 Benefits of grid computing for LHC

As has already been alluded to, by using Grid Computing, as adapted in the MONARC model, should provide a number of benefits, such as:

- empowering more universities and individual scientists to do research on LHC data, and without having to be at CERN,
- sharing LHC computing resources dynamically,
- handling peak loads better,
- providing capacity “on-demand”,
- enabling opportunistic use of non-LHC computing resources,
- avoiding duplicating calculations already carried out by others, through the use of Virtual Data (see [10, 35] for a description of Virtual Data).

By using widely deployed grid software as much as possible and by connecting to facilities that serve other technical communities, additional potential benefits might accrue, such as sharing with other communities the effort of maintaining and enhancing the grid middleware, network and grid monitoring tools, and security mechanisms. One is reminded of Metcalfe’s Law:

“The usefulness, or utility, of a network equals the square of the number of users”

One wonders whether Metcalfe’s Law should be modified to apply to grids, perhaps:

“The usefulness, or utility, of Computational Grids equals the cube of the sum of the number of users, disciplines, and different resources that participate.”

The LHC Computing Grid [3] was formed in 2002 to create a new computing environment that will support the LHC computing requirements. The LCG builds upon relevant efforts of other projects, including two pioneering projects led by CERN and funded by the European Union: the European DataGrid [25] and Enabling Grids for E-science and industry in Europe (EGEE) [36].

The EDG project focused on enabling next generation scientific exploration that requires intensive computation and analysis of shared large-scale databases, millions of Gigabytes, across widely distributed scientific communities. It is a three year project that began in 2001. In many ways the EGEE is a natural, larger-scale follow-on to the EDG.

The EGEE project aims to integrate current national, regional and thematic Grid efforts to create a European Grid infrastructure for the support of the European Research Area. This infrastructure will be built on the EU Research Network GEANT and will exploit Grid expertise that has been generated by projects such as the EU DataGrid project, other EU supported Grid projects and national Grid initiatives such as UK e-Science, INFN Grid, Nordugrid and the US Trillium (a cluster of projects). The EGEE project will begin operation in the spring of 2004.

Grid environments are still in the early stages, so perforce the LCG has to adapt and deploy technologies that are still under development. Fortunately, other projects have similar needs and are engaged in developing many of the needed components. See for example [7–11]. Many of these projects involve distributed access and analysis of scientific, medical, or engineering data, and – while not at the same scale as the LHC – require rather similar functionality as the LCG.

9 Challenges

Despite the existence of many Grid projects that support real applications, there is still much to be done. Some of the existing software has adequate functionality but is not yet robust or easy to install. Fundamental issues such as security and fault tolerance require more work. In a number of cases, it is not just the middleware that needs to evolve to provide the required functionality. Operating systems, data archiving systems, and network software need to be enhanced to support co-scheduling, deadline scheduling, global name spaces, and bandwidth reservation, for example. Better interfaces to database systems are also badly needed.

While standardization of Grid middleware will accelerate the rate of progress, the pace of standardization must take into account the limited experience we have with existing approaches and software: better ideas will surely emerge but we need to facilitate the deployment of Grids in order to determine what works well and what needs to be improved. The Global Grid Forum is a community-led standardization effort that is struggling with these issues.

The current shortcomings and difficulties are not unusual in new fields. Given the great strides already taken by early grid projects, the intense interest by applications communities, and the potential benefits of Grid environments, Grid technologies and applications are exciting fields to pursue.

The previous remarks allude to challenges that all grid projects face. The challenges that LHC computing faces can be categorized into three types: technical, research, and managerial challenges.

9.1 Technical challenges

There are many difficult technical challenges due to the scale, heterogeneity, physical distribution, and dynamic variation of the resources and analysis tasks. To get a feeling for the scale, consider the data points in Table 1.

Table 1. Comparison of parameters related to the handling of one Terabyte and one Petabyte of data

	TERABYTE	PETABYTE
RAM time to move	15 minutes	2 months
1 Gb WAN move time	10 hours	14 months
Disk cost	7 disks	6800 disks
	= $ 5000	= $ 7 million
Disk power	100 Watts	100 KW
Disk weight	5.6 kg	33 tons
Disk footprint	Inside machine	60 m^2

Therefore, storing the data is certainly feasible – at a cost – but requires attention to facilities and ways to cope with frequent hardware failures. If one has to keep 10 PB on disk, nearly 70,000 units will be required. If the mean time to failure is 100,000 hours, a disk will fail every hour or two.

As was mentioned previously, obtaining adequate network speed is not expected to be a challenge. There are wide area networks already in operation at tens of Gigabits per second. Cost has become affordable, so by the time LHC is operational it is likely that all major network links will be of the order of 10 Gb/s per experiment or better.

With such large volumes of data and many millions of individual files, ways have to be developed to reduce the difficulty of:

- sending copies of subsets to many sites and keeping track of what site has which files and replica management,
- storing the data in a safe way, and especially,
- finding the desired files for a given analysis in the context of "dauntingly complex metadata."

Hence a comprehensive data management effort is needed to design and develop a consistent and complete mechanism for tools to manage storage access, data transfer, replica management, and file access from jobs.

An area that is even less mature is workflow management, to allow jobs to move across grids, run on various resources, access data, and receive status and output at a user specified location.

Another technical challenge arises from heterogeneity, which makes interoperability much more difficult. The LHC computing environment will have heterogeneity in essentially everything, including policies:

- computing resources,
- storage resources,
- applications,
- network speeds,
- management domains,
- policies, especially security mechanisms and policies.

Because the resources and the users are distributed, a number of technical issues arise, most of which do not yet have robust solutions:

- identifying the best resources available for the task at hand, in real time,
- global access and global management of massive and complex data,
- monitoring, scheduling, and optimization of job execution on a heterogeneous grid of computing facilities and networks,
- end-to-end networking performance.

Furthermore, the resource requirements will be highly dynamic. In the traditional physics data processing model, the tasks can be categorized as follows:

- event simulation,
- detector calibration,
- reconstruction,
- physics analysis.

Resource access patterns are less predictable than for the other three (jobs are initiated from almost any HEP site in the world; large variation in the patterns of data access).

These tasks involve intimate combinations of data and computation, with unpredictable (autonomous) development of both. In other words, the dynamic, sometimes chaotic nature of the computing load is inherent in the LHC computing requirements.

Thus the nature of physics computing raises the need for:

- on demand computing,
- real-time resource identification,
- fault tolerance,
- virtual data support (retrieve instead of recomputing, unless it will cost less to recompute) – Book-keeping of what has been computed and what has not, in a global environment.

The technical challenges sketched out above are challenging and in general production-quality solutions are not available. However, in most cases there are prototypical implementations, and solutions will emerge with sufficient time, level of effort, and careful development guided by experience with early applications.

9.2 Research challenges

Some topics probably require more than refinement and professional implementation. These are research challenges, some examples of which are:

- Integration of workflow and data base access, co-optimized. For example, if a job has been loaded into computer memory for execution but the devices that store the data needed for that job are busy or unavailable, user time and computer resources will be wasted;
- Performing distributed queries on a global scale. This will be necessary since the data will be distributed among various sites;
- Dealing with dynamic variability in authorization of access for a given user, what resources are operational and available to take on the work, data and schema, and performance of the computers, the networks, and the data servers;
- Dealing with chaotic resource demands due to the thousands of physicists who may submit jobs on the distributed resources;
- Generating metadata automatically for discovery, automation of tasks. Without such metadata, it may be hopelessly time consuming to find the desired files or database records;
- Data provenance tracking, so that one can determine exactly what computations were performed to derive the data products.

The distributed nature of the computing environment raises the need for:

- Flexible and extensible user interfaces that hide most of the complexity of the environment;
- Ways to identify the best resources available for the task at hand;
- Global access and global management of massive and complex data collections;
- Monitoring, simulation, scheduling and optimization of job execution on a heterogeneous grid of computing facilities and networks;
- Achieving and monitoring end-to-end networking performance, application integration;
- Technologies and services for security, privacy, accounting.

In short, informatics research advances will be required to devise mechanisms for implementing some of the functionality that the LHC computing community would like to have.

9.3 Managerial challenges

The managerial challenges are perhaps the thorniest because they involve political and cultural considerations that are sometimes in conflict with the concept of a computing facility that encompasses resources from many different institutions and that requires using software designed and produced by others.

One major challenge is how to effect the transition from research prototype software to production software. The transition requires much more money and different types of people.

Much of the software required to create the LHC computing environment is still immature. Although a good bit of it is in use at a number of grid projects worldwide, it is still far from being easy to install, well documented, professionally implemented, robust, reliable, and interoperable with other software components. To make the transition requires spending a great deal more effort/money than was needed to develop the initial version. A rule of thumb is that it takes 10 to 100 times the effort to develop production-quality software as it took to develop the initial prototype. In addition, the people who are needed to develop the production software need to have different skill sets and motivations from the people whose research created the prototype. Until now, much of the maintenance of the software has been carried out by the group that did the research on which it is based. It is admirable that they were willing to do so, but it is not a sustainable arrangement. On the other hand, there has to be an excellent relationship and lines of communication between the groups who develop new software and the groups charged with supporting an operational grid. Often newcomers to the grid world question the validity of the approach taken by a piece of software and may set out to design a new way of providing the functionality, only to run into major difficulties that had been identified by the original creators of the software. One wants to avoid such experiences, since they waste both time and money.

Having the managerial will to use the right types of professionals for each task is not always easy in research institutions that are accustomed to inventing their own solutions for much of their research. In addition, it is easy to convince oneself that one's needs are unique and therefore unique solutions have to be developed. However, the LHC computing needs turn out not to be very different from those of other scientific and engineering disciplines or even those of some commercial grids. Management will have to consider carefully when it is essential to develop LHC/HEP specific solutions versus when "community" or commercial software is used. In general, the latter choice should be taken if at all possible. By using widely deployed and used software, the cost of maintenance will be much lower (others will be working to ensure that the software runs on new versions of operating systems and new hardware) and its interoperability with other components of the environment is much more likely. The standardized software may not be as esthetically pleasing and users may need to learn new user interfaces, but the long term benefits are substantial.

Policy issues raise many impediments to creating the LHC computing environment. Many of the resources that will comprise the LHC computing facilities will be at different institutions, funded by different governments, and often serving the computing needs of other communities in addition to the LHC physicists. Therefore, mechanisms have to be established for sharing resources that are funded in part for other applications. In addition, security policies vary greatly yet LHC users will frequently need to carry out their computing on systems in several different administrative domains. Even policies on the use of disk and archival storage vary widely and those differences can cause tremendous difficulties in running jobs that use the distributed data.

Fortunately, the LHC community does not have to do it all. As has been mentioned, there are other projects and efforts that have identical or similar needs and goals and with which LHC can collaborate, share the cost, and obtain a better end product. One can readily identify projects and initiatives with which collaboration would be mutually beneficial – and in many cases is taking place to some extent – including, to name just a few:

- European Union sponsored projects: EDG, EGEE, GridLab [37], many other grid projects, and the GEANT network;
- UK e-Science: Core Programme, GridPP [38], Astrogrid [39];
- United States projects: NASA's Information Power Grid (IPG) [40], the Extensible TeraGrid Facility [41], Grid Physics Network (GriPhyN), iVDGL, National Virtual Observatory, NSF Middleware Initiative (NMI) [42], and the Cyberinfrastructure initiative;
- Japan's National Grid Research Initiative, Naregi [43].

Many other countries and regions are also implementing grids for science and engineering, the previous list represents a small fraction of the efforts worldwide.

There is also strong commercial interest in grids. Cisco, HP, IBM, Intel, Microsoft, Oracle, SGI, Sun, Qwest and many other major companies are investing in grid computing technologies and services. Sponsors of the Global Grid Forum include nearly 50 companies [44]. Those companies invest in grid computing because they anticipate a large commercial customer base. Indeed, a few industrial end-user companies are already developing grids for supporting their applications, some of which have global span as well.

The commercial interest in grid computing is good news, because their investments will hopefully produce much useful, interoperable and supported grid software. But there is also bad news: commercial interests do not always match science needs. For example, some commercial suppliers believe that businesses want to build grids that operate only within their company. This is in contrast with science grids that usually span administrative domains and thus have to face many issues that intra-company grids avoid. Also, harvesting of idle computer cycles to reduce costs is often cited as the target commercial application, but science grid applications usually involve retrieval and analysis of vast, distributed data collections, not just "cycle sharing." Finally, if the commercial grid software is proprietary, source code is not available, and community standards are not followed, it may not be suitable for use by science grids.

Despite those potentially negative aspects of commercial grid software, the LHC computing community – and science grid projects in general – should see the difficulties as a challenge. They need to find ways to steer commercial investments to address science needs as much as possible. The Global Grid Forum provides a setting for this, since many of the participants in GGF working groups are from industry. Another mechanism is to form joint projects with commercial companies – users as well as producers – especially ones that demonstrate that some business applications require grid functionality similar to science grids. The UK e-Science Programme has been particularly successful at mounting joint projects with industry [45, 46].

10 The OMII concept as one way to address some of these challenges

A concept referred to as the Open Middleware Infrastructure Institute (OMII) has been proposed as one possible mechanism to address some of the challenges associated with creating and maintaining production-quality software. The OMII, if implemented, would be an international organization (an Institute) sponsored by governments and industry, whose mission would be to produce and maintain open source, standard-conforming and interoperable middleware, building on existing efforts.

Its goal would be to ensure that Grid middleware becomes production-quality and acquires sufficient functionality quickly enough to meet the expectations of the emerging grid user communities.

Implementation of the institute would be a distributed/virtual organization. It could have software development/production centers on each continent, for example. Its constituents would include/involve developers, producers, and integrators of production-quality grid middleware.

Software development could be done by university groups, research laboratory groups, and industrial concerns. There are examples of excellent software products from all three types of institutions. Selection of developers/maintainers would be based on a competitive proposal process.

To be more specific, the OMII would:

- Produce open source software that
  a) could be installed by user organizations to provide grid functionality, and
  b) computer and software companies could adopt and give added value by supporting it, porting to new platforms, optimizing performance on particular platforms, etc., such as was done with MPICH for MPI message-passing libraries, for example;
- Maintain and support the software it produces;
- Follow GGF and other relevant standards (e.g., become a member of W3C);
- Take reference implementations developed by others and turn them into production-quality software. Possibly develop early reference implementation of emerging GGF standards;
- Offer a "GGF standard compliance certification" function for producers of software who want to verify that their product complies with one or more GGF standards.

The UK e-Science Core Programme is the first to try to implement the OMII concept. It has funded the UK component of OMII, which will begin operation in early 2004.

11 Summary and conclusions

The grid approach to meeting LHC computing needs will require substantial technical, research, and managerial efforts.

LHC computing requires grid computing yet Grid technologies are not yet mature. There are many open issues to be addressed and missing functionality to be developed and more gaps will emerge as uses of computing grids proliferate. However, there are grounds for optimism that grid computing will evolve to be the highly useful technology that it promises to be. The commercial and research applications that are driving the grid are also providing the intellectual and financial resources that will lead to more and more production applications of grid computing. Another positive sign is the growing interest in the computer science community in research related to grid computing. Unlike traditional scientific computing, creation and use of grids involve a number of mainstream computer science topics and issues, such as database technology, digital libraries, cybersecurity, ontologies, semantic webs, and web services. Therefore, there is reason to believe that the LHC computing challenges will be met successfully over the next few years.

If the LHC computing challenges are met through grid computing, all scientific fields will have gained a flexible, powerful computing environment in which additional resources of all types can be added readily and accessed easily, including new algorithms and software, which are at least as important as the hardware. The interoperability mechanisms that will have been developed will enable these broader benefits.

The grid approach is most likely to be successful for LHC computing if the LHC community recognizes that many of its needs are shared by other sciences and commerce. While LCG may well lead the way – and should influence what is developed – in the long run it will benefit the most if it can adopt widely deployed and maintained grid software and standards. Once again, the physics community will be a key motivator and early adopter of an important new technology, but it must collaborate with other communities to get the best results in the long run.

Acknowledgements. This work was supported in part by the Mathematical, Information, and Computational Science Division subprogram of the Office of Advanced Scientific Computing Research, Office of Science, U.S. Dept. of Energy, under Contract W-31-109-ENG-38.

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First published in Eur. Phys. J. C 34, 67–75 (2004)
Digital Object Identifier (DOI) 10.1140/epjc/s2004-01769-5

Georges Charpak

Particle detectors and society

You may find the title of the talk intriguing, namely "Particle Detectors and Society". It looks a little bit like "Arsenic and old lace". Until you have seen the film you cannot see what is the connexion between the two parts. I was given this title and since then I have been inclined to try to deliver what the organizers wanted to listen to.

I discovered CERN in the year 1959, when I was at a conference on high-energy physics in Venice. I was a low energy nuclear physicist, working at the Joliot-Curie Laboratory at the Collège de France.

Two sessions at the Summer school of theoretical physics at Les Houches, organised by Cecile Morette, plus friendly contact with colleagues working at Leprince-Ringuet's Laboratory at the Ecole Polytechnique, had convinced me that particle physics was the most exciting field.

I tried to go to Dubna, during the first timid exchanges of scientists between France and the Soviet Union, but for reasons unknown to me I never received the visa I was promised.

The conference in Venice opened the door to a promised land for me. It was there that Donald Glaser presented the first results obtained with the bubble chamber he invented a few years earlier. To validate my candidacy at such a conference I presented a quite novel gaseous detector, with intriguing properties, which however did not arouse anybody's interest, led to no experiment, but was of great importance to me.

Leon Lederman came to me after the talk. He was going to visit CERN on a sabbatical year, with the goal of investigating ways to measure the anomalous magnetic moment of the muon. He was looking for slave labour and after my talk had the illusion that I had some of the skills he needed for the young European team he had to assemble. It was one of the most ambitious experiments planned with the new accelerator, which had been built at CERN.

He offered me a fellowship for one year and I spent thirty years there. I was hired at the age of 35, after my PhD, which means that I was not a beginner. The laboratory of Joliot-Curie had some excellent features. The lectures by Joliot, on the history of nuclear physics, were inspiring. The laboratory was empty as far as modern equipment was concerned and most of the young emerging physicists were clever at experimental physics and had to build their own instruments. So I started to build 130 Geiger Counters of which only 20% deigned to work properly, despite my respect for written recipes.

At that time we were competing with Martin Deutsch of MIT, on a problem on angular correlation between two gamma rays emitted by a nucleus. He was using scintillation crystals and photomultipliers, freshly available in the USA.

It was thus hopeless to try to compete and for my thesis I started with a slightly elder colleague, F. Suzor, the construction of an instrument consisting of two large single wire proportional counters, tangential along a plane, which permitted us to study the correlation between very low energy electrons, starting practically from zero energy, and β-rays in coincidence.

There is not much point in describing our results and I only want to mention that I learned everything there was to be known about negative pulses, positive pulses, the timing of the pulses produced by an avalanche in single wire proportional counters and later it proved to be a real treasure when I entered the field of multiwire proportional chambers, in 1967.

I later invented the gaseous detector which was a pretext to go to Venice in 1959. I joined CERN in 1960 and worked for three years on the measurement of the anomalous magnetic moment of the muon. It was a great time because the experiment was difficult, requiring many innovations in the instrumentation and because the group of physicists who jointly ran it were enthusiastic, hard working and talented. For one year we also enjoyed the leadership of R.L. Garwin, who spent a sabbatical year at CERN and who was an artist and a living encyclopaedia, as far as experimental physics is concerned.

After the success of the first stage of the experiment it appeared that there were ways to considerably improve the accuracy and a fraction of the team continued for decades with new actors led by Francis Farley and Emilio Picasso while others decided to change subjects and I was among them. The ambiance at CERN was very stimulating. All the experimental physicists there aimed to understand the theoretical grounds of their experiences and were eager to follow courses on theoretical physics. I remember that often at the end of a night shift, when there was an academic lecture at 11:00 a.m. we slept for two hours in our offices and then went to those lectures.

It was a time when the new accelerators and the new interrogations in physics were demanding new detectors capable of giving a more precise spatial accuracy than the scintillating counters. They had to be as fast, and deliver spatial information on the coordinates of the large number of particles produced simultaneously in a collision, well enough to permit identification of rare and complex events. I went back to detectors and introduced two new types of automatic spark chambers. It was a golden age. If you showed that you had an idea you could hire three technicians for your group, a young experimentalist, a couple of visiting experimental scientists from the Unites States.

In 1967, at the same time as probably half a dozen other teams I decided that it was worth making use of the proportional mode of amplification existing in proportional wire chambers. The experience I had acquired during the years working with single wire proportional counters was extremely useful. The first chambers of 10×10 cm^2 with wires spaced at a distance of 2 mm, worked like a charm and since we understood the origin of all the phenomena we were observing it led us immediately to the one dimensional wire chambers, 1000 times faster than a spark chamber, then to the two dimensional wire chambers, essential for the localisation of X-rays and to the drift chambers which became an essential instrument in some experiments requiring large surface with accuracies of the order of 100 microns.

I must say that making these detectors, gave me the opportunity to collaborate with very talented and clever young physicists who came to CERN to work on new detectors for varying lengths of time and this developed my taste for this activity. I would have to mention a good dozen or so names to do justice to all the visitors who made original contributions. It led us to some very useful developments, like the multistep avalanche chambers and the light emitting proportional chambers. Some of our collaborators are now leaders of reputed groups in detector physics in Israel, at CERN, in the USA and Europe.

I personally invested much of my time and energy in the X-ray imaging for biology application and had the pleasure of equipping the Synchrotron Radiation Facility at Orsay with an imaging spherical drift chamber, which for 10 years was a major tool for studying the structure of large molecules. I mention it because this project was simply presented under the umbrella of being a test bench for the study of two dimensional high accuracy localisation of a low energy X-ray, which it was indeed! But I am not sure that at the present time at CERN, or elsewhere, such freedom would be encouraged, except in some wealthy university laboratories.

I continued to work on detectors when the LHC came with dramatic requirements for new detectors, capable of surviving the much higher rates and handling much higher multiplicities.

Table 1. Performances of Micromegas obtained in various particle beams with Minimum Ionising Particles. The 0.2 ns time resolution has been obtained with a UV pulsed laser creating single photoelectrons on the micromesh

Spatial resolution	12 μm (rms) with MIPs
Time resolution	0.2 ns (rms); 0.7 ns with MIPs
Energy resolution	11.8% (5.9 keV) (FWHM)
Gas gain	$\gg 10^4$
Counting rate	$\gg 10^6$/mm^2/s
High radiation resistance	

With my friend Ioannis Giomataris, we started to work on a new gaseous detector, "Micromegas", while other groups fought to impose different gaseous detectors. We all lost the battle against the solid state detectors. When I see the results obtained now by the groups in Saclay which have developed Micromegas to a level where it can easily match the characteristics required for LHC physics, at a lower cost and when I also look at the characteristics of "GEM", the gaseous detector developed by Fabio Sauli's group, I think that giving up the gaseous detector too hastily was of questionable wisdom since the choice of solid state detectors has been probably a source of considerable increase in expenditure.

Before this talk Giomataris sent me fifty pictures illustrating what is being undertaken now with Micromegas and I was impressed by the ambitious programmes of research in high-energy physics, now undertaken with this detector. It would take two hours to present all the pictures and one week for me to understand their content. So I will limit myself to a few slides.

The principle of the detector is shown in Fig. 1. They have now reached an intrinsic time resolution of 0.2 nanosecond (Table 1), the intrinsic position resolution is three to four microns, the reason why you don't reach it when you make an experiment with particles is due to a jitter introduced by the position of the initial electron. The Saclay group is now running an experiment in which they have, over one square centimetre (Fig. 2), 10^8 particles per second and with a time resolution of 0.7 nanosecond. In another experiment, COMPASS, they have been running for two years with twelve chambers, without any problems and have reached seventy-micron accuracy with 9 nanosecond resolution (Fig. 3). If we had had these 40 by 40 cm prototypes four years ago, I think we would have had an influence on the detector chosen for the LHC. But it is not very important since ambitious experiments are undertaken anyhow, because of the unmatched intrinsic advantages of gaseous detectors.

Physicists want to search for the axions produced in the Sun, coming massively to the Earth, and detect them by interaction with a magnetic field produced by the magnets of the LHC. Soft X-rays (1 to 8 keV) are produced and detected with 100% efficiency since the noise of the detector is very small. They dream that in a few months they may see the axion.

Detectors are now being used or developed for neutron tomography, X-rays imaging. Another experiment projected by Giomataris and his group relies on a source of tritium, which is equivalent to what you need for a thousand thermo-nuclear bombs. It is a very intense source of neutrinos. With a small detector Micromegas outside the source and a drift length of 10 meters, whenever you have a reaction produced by an elastic scattering of the neutrinos with electrons, the ionisation electrons moving back will give you a good enough position and time resolution to have all the ion pairs detected individually (Fig. 4). The information allows you to see if you have oscillations or not. The maximum oscillation occurs at 6.5 meters. It is a nice dream to compete with people who are doing this type of research with accelerators, where they detect the neutrinos at a distance from the target of 730 kilometres with an event rate a million times smaller. The difficulty indeed is to put your hands on so much tritium, which exists and is useless. They expect ten thousand events per year if they can reach 10 bar pressure. Being high-energy physicists, they are afraid of nothing.

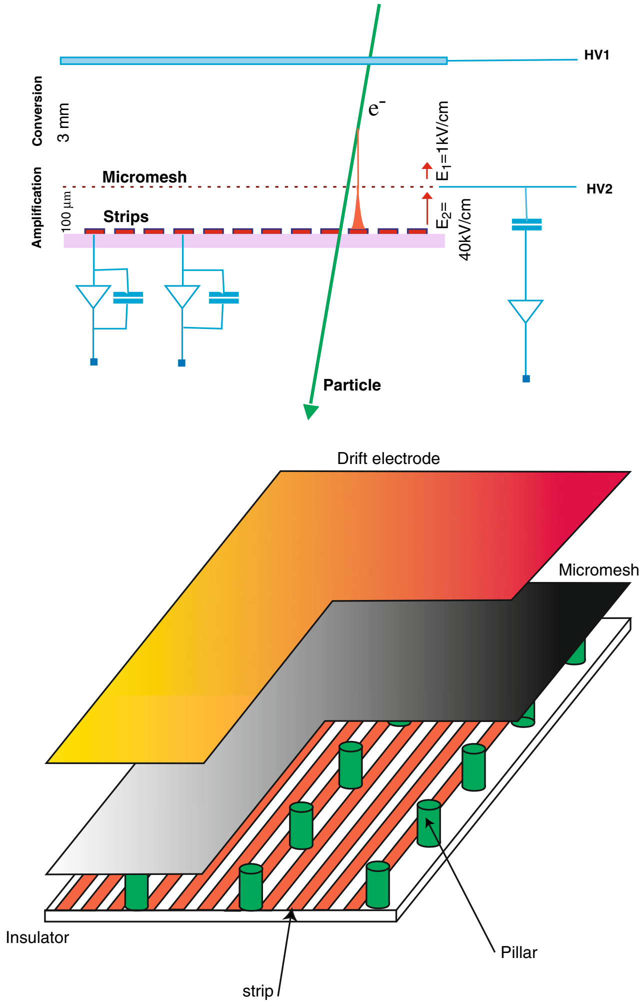

Fig. 1. Micromegas principle [1]: a high field region is formed between the micromesh and the readout strips with the help of 100 µm high pillars etched on the micromesh. The amplification process occurs in the small amplification gap leading to a fast elimination of the positive ions

I think I was not bold enough to stay in high-energy physics and I decided a long time ago to work on applications in medicine and biology. I will show a few images that illustrate my activity. Figures 5a, 5b and 5c show various quantitative images of the distribution of pharmaceutical molecules with different radio elements in animal sections. They were obtained with the β-imager, which I have developed, or with the μ-imager, which has also been derived by an Orsay Group from Particle Detector Physics. The β-imagers are a direct fall-out of high-energy physics, when we were taking the image of the avalanches with image intensifiers. In 20 minutes we obtain

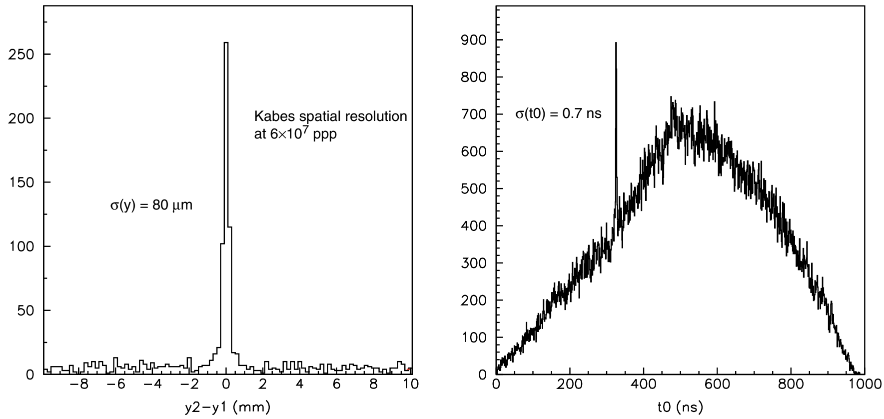

Fig. 2. The performance of the Kabes mini-TPC read-out by the Micromegas detector at very-high rates. The detector had a successful run inside the kaon beam of the NA48/2 experiment [2]

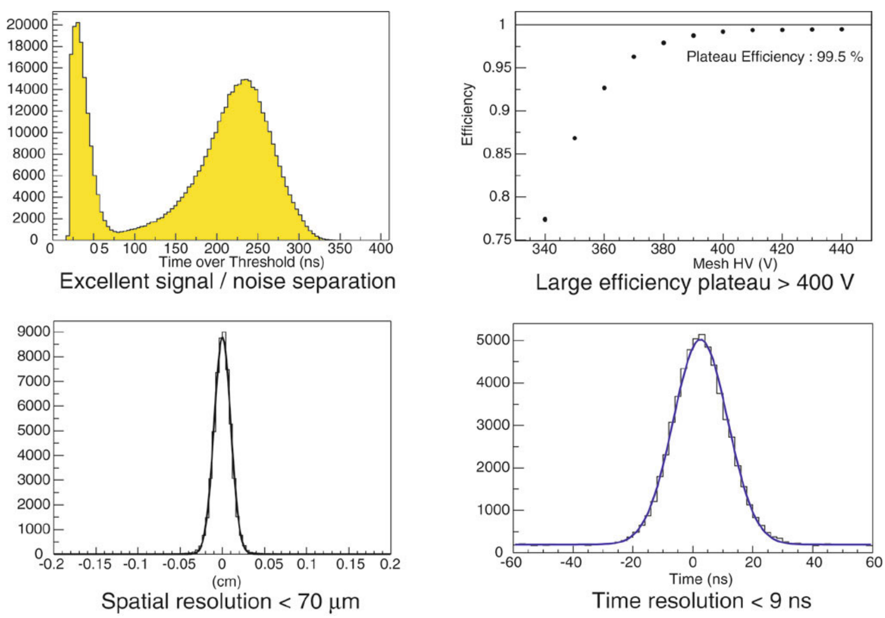

Fig. 3. Performance of the COMPASS Micromegas detectors [3], the largest chambers build with the novel micro-pattern technology, running since 3 years in a stable fashion

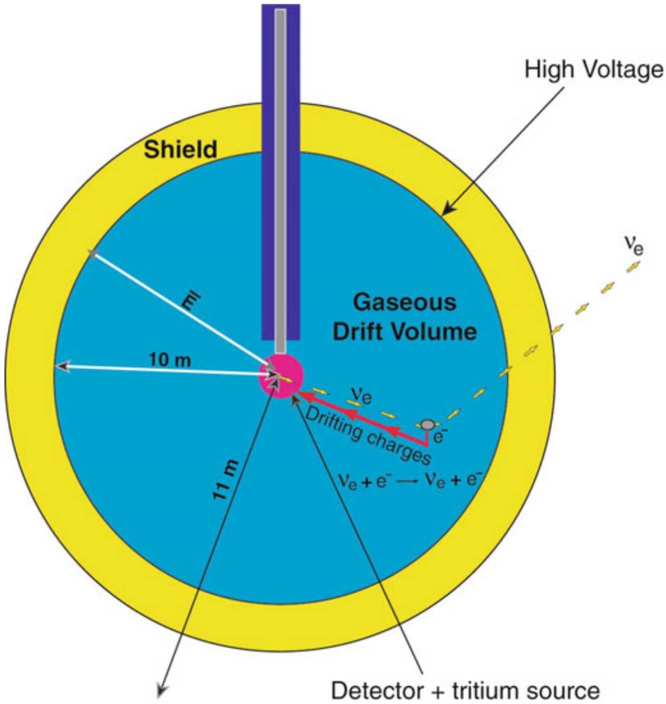

Fig. 4. NOSTOS: a new proposal [4] to measure neutrino oscillations using an intense tritium source as low-energy neutrino beam. The idea is to use radial drift chamber geometry with the Micromegas detector surrounding the neutrino source at a distance of 50 cm from the centre. Electron recoils produced in the gaseous drift volume (10 meter in radius) are creating ion pairs that are collected and amplified by the Micromegas detector

images, which required one month with a film, making such studies impossible. Because of the different response of the detector for different energies, it is possible to separate the signals from two different isotopes (Fig. 6). I have had the pleasure of seeing biologists from large pharmaceutical laboratories coming to us, with a sample, labelled with long-lived isotopes. They were invited to a good meal, and when they came back they had an image that they could obtain in one month with traditional methods. That is the reason why about a hundred of these instruments are now used in biology research. Some biologists might make discoveries, which they could not have made without this instrument and this is an illustration of the contributions which big laboratories like CERN can make in fields of major importance. We already have the Web, which is a big thing. Here we have something less visible which may become important.

Now I come to radiology. A Russian group from Novosibirsk made a wonderful study on radiology of human beings with wire chambers. Figure 7 shows the results we are now obtaining. The images are taken from two orthogonal directions and we have learnt how to use an algorithm, which allows a 3-D reconstruction of bones.

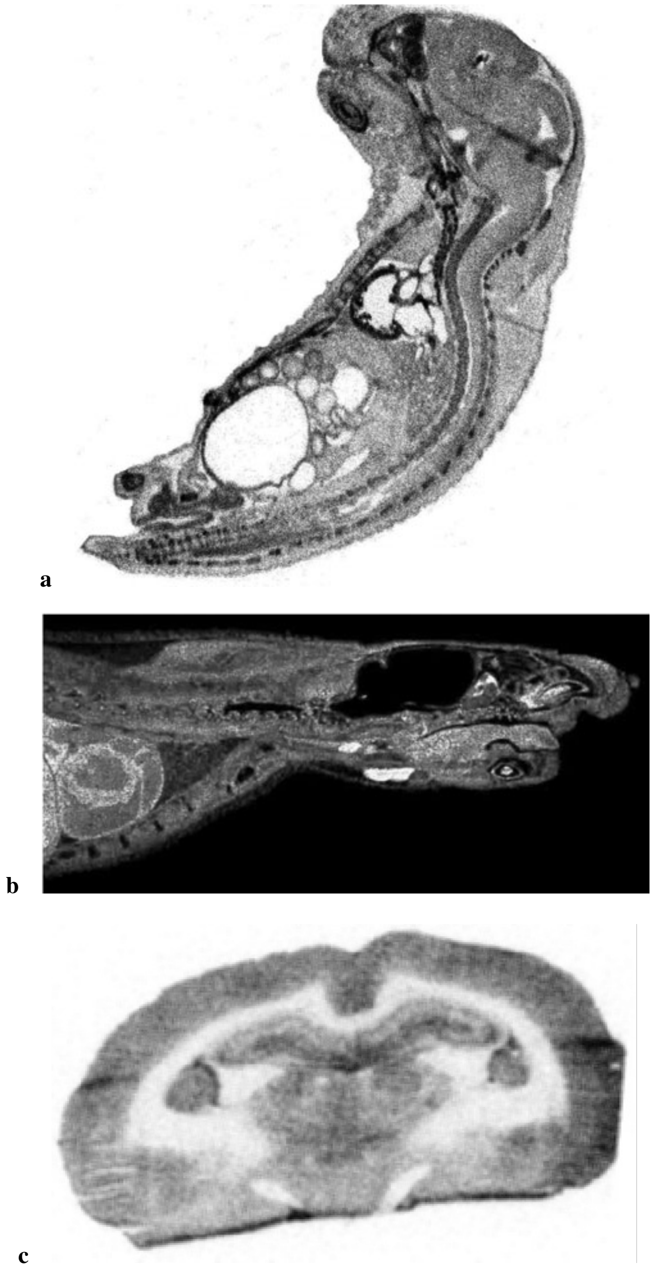

Fig. 5. **a** μ-imager picture of receptor binding of a ^{125}I compound in a mouse embryo (15 μm resolution) [5]. **b** β-imager picture of ^{3}H labelled whole body rat sections acquisition [6]. **c** β-imager picture of a rabbit brain: ^{99}Tc labelled HMPAO complex accumulation in the cortex, the thalamus, the hippocampus. (Spatial resolution for ^{99}Tc is 50 μm) [7]

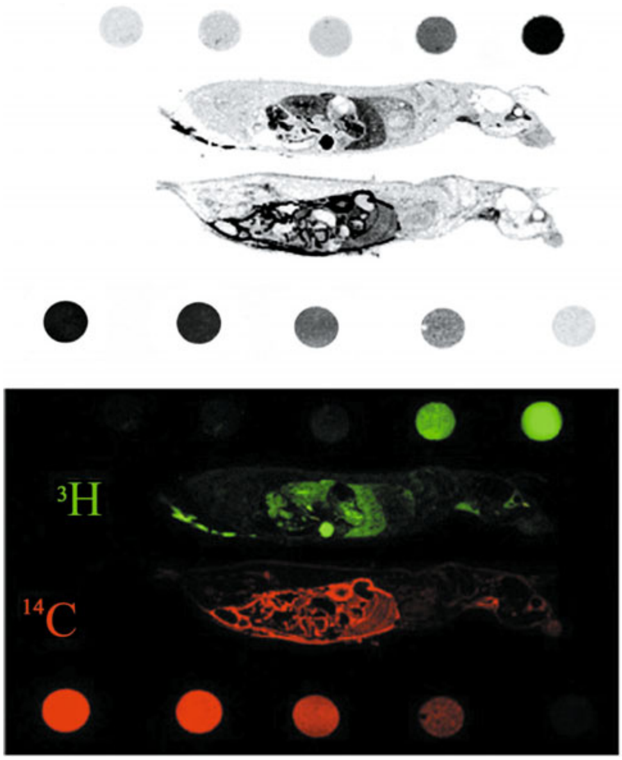

Fig. 6. Simultaneous measurement and separation of ^{3}H and ^{14}C labels with the β-imager

You see the details in projection, where the resolution is approximately 250 µm. The dynamic range is 30’000. It is in use in a hospital for children in Paris, where they treat scholiotic children. The advantage with respect to a scanner is that you deliver 100 to 1000 times less radiation. On average the doctors were giving 6 radios per child per year. With this they give only the equivalent of one. That makes them happy. Can you become rich with it? This is not clear! The high-energy physicist is a little bit like a kid. When the competitors are General Electric, Philips and Siemens, he discovers that he counts for nothing. Unless the law obliges us to decrease irradiation levels for children you may get nowhere. You depend on little things, whether insurance reimburses this type of radiography or not, for example. But there is still a great subjective pleasure working in this field.

Now let me go to an activity which I find fascinating. Six or seven years ago, Leon Lederman invited me to visit a ghetto where he was trying a pedagogical method called “Hands-on”. In fact it was one of seven or eight similar experiments in the United States, partly financed by the National Science Foundation, which are using slightly different approaches but all based on a simple idea: children are like scientific researchers. It is certainly true that scientific researchers can be like children, which you know, when you have spent some time at CERN. Children want

Fig. 7. 2-D radiographs acquired with EOS, a 2D–3D low irradiation dose scanner (3D reconstructions are superimposed)

to learn. Physicists go to the lab because they want to have an answer to questions. Young children are constantly questioning the mysteries of the surrounding world. If you provide them with good equipment, which doesn't cost very much, you then discover that you can drastically improve the way they learn. They learn to make a hypothesis, they learn how to do an experiment, to check the hypothesis and they learn how to write and read, to discuss things with each other and communicate. And it is of great efficiency. We started with 10 people, sent by the Ministry of Education, who came with me to visit a school in Chicago. They were trusted, because they were important people in the French education system and they came back full of enthusiasm. And to my surprise we have now reached the stage where we have 12% of teachers in France who are contaminated.

In China, I have been to Shangai, and other places where they plan to equip 30 towns to join this venture. In Latin America we have seen the same move because it corresponds to something, which is now a universal need. Education is considered by the European Community as having a top priority. By the year 2010 they want to have revolutionized the education system throughout Europe. They are going to have the money but I am not sure that they know how to do it. The ideas come from pioneers, in the States or in many other places who have developed and practice this method of teaching. I have had the privilege of working on the dissemination of the method under four Ministers of Education, who all helped us, some more, others less.

It appears to me that to progress rapidly we should be inspired by the experience of CERN. The cost of CERN has been roughly one billion dollars, every year for 40 years. We, half jokingly told the politicians: “give us one billion dollars every year for 30 years and we will give you a renovated educational system”. That looked like pure demagogy, but when I told them there are 50 million children and it costs 15 euros per child, every year, they could easily find the same number as we did. Now, finally, we don’t really need that much.

I have just come back from Stockholm where I spent three days with friends of the Royal Academy and also with a group, which is involved in the same reform. They have done some wonderful work over 5 years and we are proposing that the European Community give us enough money to start with some towns which are going to be “pilot towns”. I am convinced that if we succeed, we will rapidly contaminate the continent. We don’t need the billion dollars per year. We can stay decentralized. We don’t need to have many civil servants in one town. We can make use of Internet. In France, we have a site, with 80 scientists permanently answering questions put to them by teachers because the main problem is to teach the teachers. When the Chinese Vice-Minister of Education visited a school with me, she said: “Mr. Charpak, it is the best apprenticeship I have seen for scientific debate”. Because in their country like in many others, the tradition is that a teacher is a master, knowing the truth, to whom you cannot say: “We don’t agree with you, you don’t understand the problem” while in these “hands-on” classes, the teachers learn to say, when they are stuck, that they are like scientists, do not know everything and will give the answer next time.

Working in this field I must say I have the feeling that I use many things I have learnt from my life at CERN. We have to design a new international organization to help us make this big jump. We have serious support from some political leaders because they like our moves. They appreciate the fact that children are going to learn how not to follow gurus who preach the truth to them because they have learnt it from books. They learn how to check affirmations and make up their own mind by experimenting. And this can be a major contribution of science to the appeasement of many conflicts in our society.

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First published in Eur. Phys. J. C 34, 77–83 (2004)
Digital Object Identifier (DOI) 10.1140/epjc/s2004-01770-0

Luciano Maiani

The future for CERN

1 Introduction

We are celebrating today CERN's past successes. Thirty years from the first observation of the weak neutral currents and twenty years from the discovery of W and Z, it is reassuring to see the Laboratory completely committed to the construction of the LHC, a project at the cutting edge of physics and technology, of a dimension never tried before in particle physics. It is a heartening signal of the vitality of the Laboratory and of the strong support that we have been constantly receiving from our Member States.

We have gone through some difficult circumstances over the past two years and I have been impressed by the determination shown by the CERN staff to keep the LHC on the road and to remain at the front of particle physics. This must be the starting point of any thought about CERN's future. In addition, CERN is a very open laboratory – we have about 6000 users – and it is impossible to speak about CERN and the future of CERN in isolation from the rest of the community; the two things are quite interleaved.

Before going into the matter, let me recall that the issue of the future of CERN has been discussed many times during my mandate. Discussions in the Laboratory have started in early spring 2001 [1] just after the closing of LEP, and working groups have been created to study the different aspects. The issue was later addressed by ECFA, with a detailed study on the future of European particle physics finalised in summer 2001 [2], and by the CERN Scientific Policy Committee, then chaired by George Kalmus, with a study presented to the CERN Council in December 2001 [3].

After this report, there was an interval of about two years in which we have been more busy taking care of the present of CERN, rather than of its future. Discussions started again in March 2003, when Council considered the possible participation of CERN to the current projects on an electron–positron Linear Collider [4], followed by various meetings on the same subject during summer 2003 [5].

Finally, let me stress that I am going to present to you here strictly personal views, which do not commit in any way the next management of CERN, due to start in a few months.

2 The future of CERN: the overall view

In a way, CERN's future is a trivial matter to describe: the future of CERN is the LHC [6]. A glance at the dates shows that this is indeed the case, to a good approximation.

Table 1. Comparison of LHC with upgrade possibilities

	LHC	SLHC	LHCx2
Energy (TeV)	14	14	28
Luminosity in 1 year (fb^{-1})	100	1000	100
M_{Squarks}(TeV)	2.5	3	4
M_{WLWL}	2σ	4σ	4.5σ
$M_{Z'}$(TeV)	5	6	8
Extra-dim, $\delta = 2$ (M_D, TeV)	9	12	15
M_{q*} (TeV)	6.5	7.5	9.5
$\Lambda_{\text{compositeness}}$(TeV)	30	40	40

Table 2. Comparison of performances of VLHC, TESLA and CLIC

	VLHC	LC (TESLA)	LC (CLIC)
Energy (TeV)	200	0.8	5
Luminosity in 1 year (fb^{-1})	100	500	1000
M_{Squarks}(TeV)	20	0.4	2.5
M_{WLWL}	18σ		90σ
$M_{Z'}$(TeV)	35	8[a]	30[a]
Extra-dim, δ=2 (M_D, TeV)	65	5–8.5[a]	30–55[a]
M_{q*} (TeV)	75	0.8	5
$\Lambda_{\text{compositeness}}$(TeV)	100	100	400

([a]) indirect reach (from precision measurement)

We will commission the LHC in 2007 to produce physics, we believe, for some 10 to 15 years. There could be a luminosity upgrading, to be discussed presently, that can prolong the LHC life time and extend the mass range for discovery by some 20%. This is a sort of obvious thing and in fact it is even partially foreseen in the present CERN long-term plan. Thus fully exploiting the LHC can bring us to 2020 or so.

In the same framework, another aspect I want to put on record here is the consolidation programme. CERN has not been renovating its infrastructure for long time, due to the effort to produce the LHC. We have been consistently pointing out to the Council that after the start up of the LHC significant resources will have to be dedicated to a long due consolidation programme, and I think this has to be confirmed.

So, why bother to make a talk about CERN's future? Well, the LHC cannot make us forget that there are important particle physics problems and important sectors of the scientific community that are not covered by the high-energy frontier embodied by the LHC – neutrinos among others – and I think that we must maintain the idea of diversification in particle physics. In a way this is what Georges Charpak was trying to tell us: we cannot continue to be always fully engaged into a single project. It is not in the interest of CERN and not in the interest of particle physics.

The other reason, of course, is the discussion that has started about an electron–positron Linear Collider (LC) in the energy range of 0.5 to 1 TeV. This issue is now in front of the community and we must discuss how CERN can contribute to it.

In this context, the LHC energy doubling has also to be kept in the picture. The energy upgrading is on a completely different scale than the luminosity upgrading, as it would require replacing the magnets of the LHC by new magnets that, by the way, we still do not know how to make. However, it is an option that has to be considered: costly as it may be, it will be much less expensive than making a new machine.

3 LHC upgrading

Ideas about LHC upgrading have been presented in the ICFA seminar of 2002. As for luminosity, we speak of an increase in luminosity which would bring LHC in the order of 10^{35} cm^{-2} s^{-1}, to collect in three-four years of data taking around 3000 fb^{-1} per experiment [7]. If you go in this direction, you would have a first phase, to reach the ultimate LHC luminosity of 2×10^{34} cm^{-2}s^{-1}. A second phase would follow, in which one keeps the arcs, that is the main magnets, unchanged and upgrades the luminosity by changing the quadrupoles in the straight sections, to get say a factor of 5, maybe even more. The second phase would be relatively inexpensive, perhaps in the order of few times 100 MCHF

The next possibility is to replace the present 9 Tesla magnets with say 15–17 Tesla magnets, to about double the energy of the LHC. New superconducting magnets based on Nb_3Sn are being considered in FermiLab and in Europe, but this is in no way a trivial matter and it requires a good deal of dedicated R&D. Granted that the new magnets can be developed, the substitution of 27 km of LHC cryogenic dipoles will be certainly a major step, for which the case will have to be carefully assessed. A reasonable target cost should be in the order of the cost of the present LHC magnets, about 2 BCHF.

Table 1 gives what one can gain from the luminosity and from the energy upgrading [8]. We consider a number of benchmarks: the mass that can be reached in the search for the supersymmetric partners of quarks, for strongly interacting longitudinal Ws, for Z', limits on the Mass constant of gravity in extra dimensions, excited quark or indications of quark compositeness.

For comparison, we give in Table 2 the performances with respect to the same benchmarks of other machines, i.e. the Very Large Hadron Collider, the highest energy TESLA (0.8 TeV) and the highest energy CLIC (5 TeV).

4 European participation in a subTeV electron–positron collider

The strongest motivations for an electron–positron collider in the sub TeV region is that it is clearly needed for precision Higgs boson physics. In addition, if supersymmetry applies, an LC will be crucial to distinguish the Standard Model from other models because of the different projectiles.

But we have also learnt from all the exercises that we have done in these years that we really have to be able to go further than 1 TeV in energy, to sort out which, if any, of the supersymmetric models apply or to understand whether there is other physics beyond the Standard Model.

My very personal position is that Europe should not offer a site for a sub-TeV linear collider, for three reasons. First, the presently considered LCs are in the same energy (and cost!) range and are complementary to the LHC that we are building. Also, I think that the effort in particle physics needs to be shared by the other regions. While Europe is doing the LHC, it would be reasonable, and very desirable, that the other regions take the lead to construct a LC as soon as possible. Finally, and above all, I do not think Europe can afford being a major shareholder of the linear collider as we are for the LHC.

At the same time, Europe must participate in this linear collider, if it is done in other regions, much as these regions are participating in the LHC. It would be very good for our programmes to define the degree of European participation in this enterprise as soon as possible. The rest of the world is contributing about 15% to the Large Hadron Collider. Just as an indication, I think that a European participation of the order of 10 to 15% would be very reasonable and would serve best the interest of the scientific community in Europe.

I hope the issue can be discussed as soon as possible in the CERN Council.

5 Intermediate scale projects

There have been suggestions that the resources for the European participation in the LC should come, at least in large part, from the margin remaining in the CERN budget from 2011 onwards, after the LHC has been paid for (I take the occasion to stress that the positive unspent margin in CERN's budget for 2010 is reserved for the LHC contingency and I am pretty sure that we will need it all).

This is certainly a possible suggestion but, please, don't take everything out. We need some resources, at least for the consolidation plan I mentioned before and for the LHC luminosity upgrading.

In addition, and this is a point I want to make very clearly, we are in bad need of intermediate projects, not of the big collider dimension, to adapt and to prepare for the next step. In Europe, in Russia, in the US and certainly at CERN there are infrastructures and capabilities that are going to become unused in the short term because (i) the production of LHC machine and detector components is phasing out and (ii) any activity related to a big collider is certainly not going to start so soon. So, one would like to have some project of intermediate size and intermediate time scale which would fill the needs for diversity in particle physics and would utilise these infrastructures. At present, we can identify two such projects:

- the superconducting proton LINAC in CERN (I'll say more about that soon);
- the TESLA X-ray free electron laser in DESY.

Accelerator particle physicists should consider the TESLA X-FEL as really belonging to their domain. In the spirit of a network of accelerator laboratories which work together on a common set of projects, we should put the two projects in the same basket and find a way to share resources and know-how for them.

As an important added value, the SPL and the TESLA X-FEL would establish stable links between accelerator particle physics and at least two other scientific communities. This is the dream that Bjorn Wiik pursued tenaciously in DESY, for the bio-medical and chemistry community, while Carlo Rubbia was pioneering the connection with the nuclear physics community. CERN is pursuing the nuclear physics connection with ISOLDE and the Neutron Time-of-Flight facility, but I think that with the SPL we could do it on a grander scale.

Finally, on a smaller scale, I think we strongly need CERN participation in astro-particle physics projects. Ideas have been circulated, to have CERN as a European basis for:

- the integration of detectors for Space physics (e.g. the Extreme Universe Space Observatory – EUSO);
- Deep Underwater Neutrino telescopes (NESTOR/ANTARES/NEMO);
- Auger in the Northern Hemisphere;
- or others.

CERN has made a first step in the astro-particle area with the introduction of what we call "recognized experiments". The way is thus open to an active participation of CERN in that area, certainly less expensive than the high-energy area, after LHC commissioning.

6 The superconducting proton LINAC

A short commercial for the Superconducting Proton LINAC may be appropriate [9]. The SPL is a high intensity accelerator which drives protons up to 2.2 GeV (power

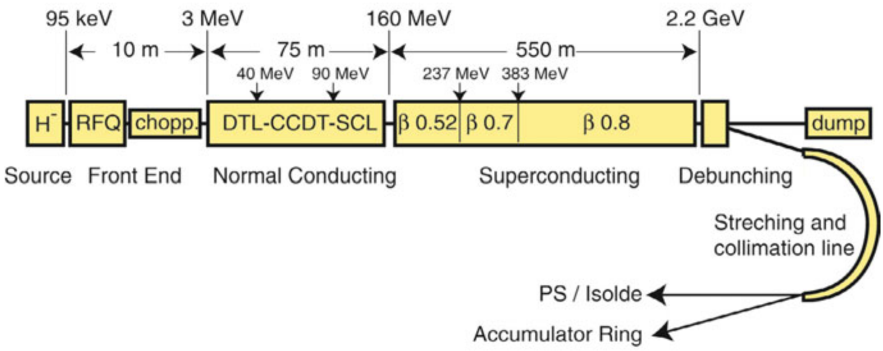

Fig. 1. Layout of the SPL and its possible location at CERN

on target around 4 MW). The last part of the SPL is realised with superconducting cavities; perhaps one may re-use the LEP cavities.

You can make many things with the SPL. It will make more robust the CERN injection system into the LHC, it can produce a second generation facility for radioactive ion beams (realising essentially the European project EURISOL), it will increase the intensity of the CERN-Gran Sasso neutrino beam and it can realise a new low-energy, high intensity neutrino beam. At the SPL energy one can produce pions but not K-mesons, and this would make a very pure beam of muonic neutrinos, with electronic neutrino contamination arising only from secondary muon decays. This is what is called a "superbeam" in the jargon. An underground laboratory in the Fréjus tunnel would be at the right distance for aiming this superbeam at and obtain very precise measurements of one of the two missing angles in three family neutrino mixing, θ_{13}.

A sketch of the SPL, and its possible location at CERN are shown in Figs. 1 and 2.

There is one further application of the SPL, which I consider to be extremely interesting, that is to produce the so-called "beta beams" [10]. What is a beta beam? Starting with the SPL you make an ion beam of suitable beta emitters, then you accelerate the beam to a very large energy, with the SPS, and store it in a circulating ring. The ions decay in flight to produce an absolutely pure electron neutrino beam. The beam is very well collimated because of the small ratio of transverse momentum (determined by the Q-value of the beta decay) to the longitudinal momentum of the ions. Purity and collimation make this neutrino beam an ideal one for a long distance underground laboratory, e.g. Gran Sasso, to measure θ_{13} with great precision, and perhaps to observe the further angle which determines CP violation in the lepton sector.

A beta beam is less powerful than the neutrino beam from the usually considered muon neutrino factories [11], but certainly it is easier and less costly to realise. In fact, the combination of measurements made with a neutrino superbeam and a beta beam can approach in sensitivity those with a neutrino factory with 10^{21} muon decays/year

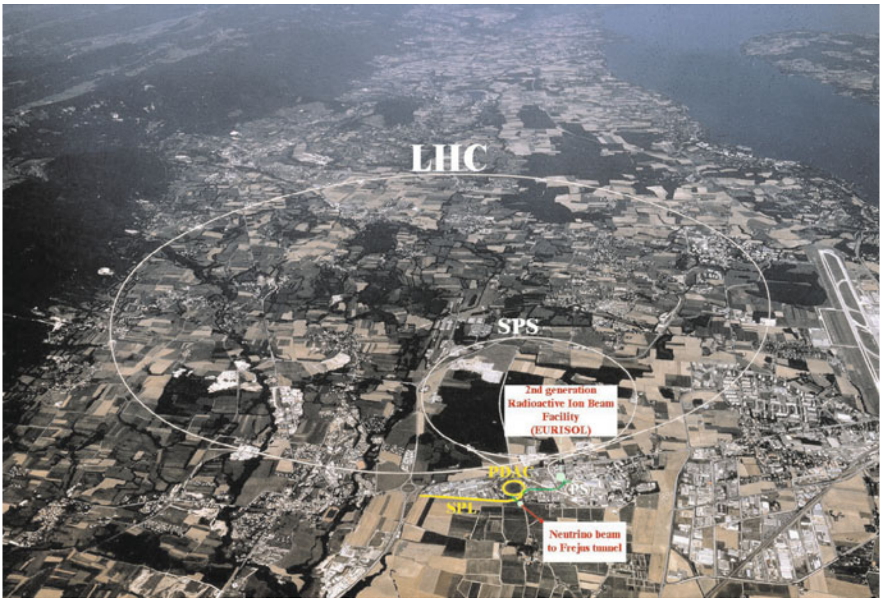

Fig. 2. Possible location of the SPL at CERN, in the Meyrin site

[12]. There are of course several aspects of oscillation physics for which the neutrino factory is incomparable:

- the precision on the measurement of θ_{13};
- the possibility to observe $\nu_e \to \nu_\tau$ oscillation (unitarity of the mixing matrix);
- the possibility to observe and study matter effects and the matter resonance around 10 GeV.

We have a preliminary study for beams with beta minus and beta plus emitters, but much remains to be done. At this point, the beta beam is a very interesting idea whose value (and cost) remains to be assessed.

In conclusion, I think that it would be very good if just after the LHC started and during its run, CERN and Europe could develop smaller scale projects to satisfy a diversified community and to prepare for a real next step into the multi TeV.

7 A compact electron–positron linear collider

The International Technology Panel chaired by Greg Loew has recently produced, under ICFA sponsorship, assessments of the technological issues which are unsolved in the different electron–positron linear collider projects [13]. In the case of CLIC, the linear collider which is being developed at CERN, the Panel has indicated a number of crucial feasibility issues that have still to be solved. CERN is at present constructing a CLIC test facility [14] (CTF3) to address these and other issues. Within the present programme, CTF3 can produce an answer to the issues posed by the Panel by 2009 (or by 2007 if additional resources, of about 6 MCHF, are put in the programme).

If these issues are positively resolved, it would be possible, in 2010–2012, to make a proposal for a Linear Collider, capable of reaching 3 to 5 TeV. This should be done wherever it is possible and should enter operation by 2022–2025, some 15 to 18 years after LHC commissioning (with the present schedule, the LHC will come into operation 18 years after LEP).

In principle, CLIC can be staged. In case of no decision about a subTeV LC by 2010–2012, CLIC would offer a real possibility for a subTeV intermediate stage, for precision studies of the Higgs boson.

On the other hand, should a subTeV LC be decided earlier, the CLIC time scale would slide forward and, perhaps, doubling the energy of the LHC could become an attractive possibility for CERN, provided what we would have learned from the LHC by that time would justify it from the physics point of view.

All these considerations require that we do not reduce, rather increase the amount of R&D in the direction of CLIC as well as in the direction of high field magnets.

8 Conclusions

There are developments which are in our future, I would say, by default. The LHC, of course, consolidation of CERN wide infrastructure and presumably the LHC luminosity upgrade.

Besides these "normal life" options, I strongly recommend an active but restricted European and CERN participation to a subTeV linear collider, should such a facility be decided under the leadership of another region. On a shorter time scale, a new start is highly desirable in intermediate scale projects, such as the SPL at CERN and the TESLA X-ray FEL at DESY, such projects being considered within a coordinated network of allied particle accelerator laboratories. After LHC commissioning, CERN should take some initiative in astro-particle physics.

The R&D towards a multi TeV electron positron collider in the mid 2020s should be vigorously pursued from now.

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5. Faculty EP-TH meeting, July 10, 2003
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9. B. Autin et al., Conceptual Design of the SPL, CERN 2000-012, December 15, 2000
10. P. Zucchelli, Phys. Lett. B **532** (2002) 166; B. Autin et al., CERN/PS 2002-078 (OP), November 6, 2002
11. C. Albright et al., Physics at a Neutrino Factory, FERMILAB-FN-692, March 5, 2002; for more recent developments see e.g. the Proc. NuFact 03, New York, USA, June 5–11, 2003 and references therein.
12. M. Mezzetto, "Physics Reach of Super + Beta beams", Workshop on Large detectors for Proton Decay, Supernovae and Atmospheric Neutrinos, Low Energy Neutrinos from High Intensity Beams (NNN02), CERN, January 16–18, 2002; see also A. Blondel et al., Neutrino Factory Note 95 (2001), Proc. NuFact 01, Tsukuba, Japan, published in Nucl. Instr. Meth. **503**, 173 (2003)
13. International Linear Collider Technical Review Committee, see report at: http://www.slac.stanford.edu/xorg/ilc-trc/2002/2002/report/03rep.html
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Comment by S. Glashow:

I must tell you that there is no more JLC, that the Japanese have changed the name to GLC (Global Linear Collider) – minor correction.

First published in Eur. Phys. J. C 34, 85–90 (2004)
Digital Object Identifier (DOI) 10.1140/epjc/s2004-01771-y

Panel discussion on the future of particle physics chaired by Carlo Rubbia

Participants: R. Aymar, G. Charpak, P. Darriulat, L. Maiani, S. van der Meer, L. Okun, D. Perkins, C. Rubbia, M. Veltman, S. Weinberg

Statements from the floor: F. Gianotti, I. Antoniadis, S. Glashow, H. Schopper, C. Llewellyn Smith, V. Telegdi, G. Belletini, V. Soergel

Introduction by Carlo Rubbia – (ENEA and formerly CERN)

I would like to open this panel discussion, which is almost a "Mission Impossible". Not only do we have something like ten very distinguished people here around this

Carlo Rubbia

table plus two young people who will speak from the floor, but we also have a subject which is not that simple. It is "The Future of Particle Physics", which is crystal ball reading as far as I am concerned. Maybe, I could suggest that we ask around this table who wants to speak first. We have been assigned 45 minutes, so I think that a time of 5 minutes per speaker should be enforced, and I suppose that's the only reason why I am here! The first statement is by Donald Perkins.

Donald Perkins

Statement

This meeting has recalled the discovery of neutral currents and the W and Z bosons, 20 or 30 years ago. My question is: what will be the programme of research at CERN

Donald Perkins

in 20 or 30 years from now? Obviously it will depend on the results from experiments at LHC and possibly CLIC, and with luck these may spring some surprises, but perhaps we should think more widely.

I want to echo the words of Professor Maiani today, about the importance of astrophysics on the particle physics scene. During the last years, a trickle of experimental particle physicists has been moving over to research in astrophysics, for example in the study of atmospheric neutrinos and the search for very high-energy neutrino point sources; gravitational wave detection; very high-energy cosmic rays; gamma ray bursts, possibly the most violent events in the universe; high redshift supernovae, and so on. I do not know if this trickle of particle physicists to cosmic physics will ever become a torrent, but presumably these people have moved to astrophysics research because they think it offers greater (or perhaps more congenial) challenges.

Perhaps I hardly need to remind you that, after 50 years of continuous and outstanding progress from experiments at accelerators, encompassed in the Standard Model, we are able to account for a miserable 5% of the energy density of the universe. Nobody knows what the remaining 95% – roughly 25% dark matter and 70% dark energy – consists of. My main point however is that today, the subject of astrophysics is moving ahead very quickly, much faster than is accelerator-based particle physics. A good deal of that progress has in fact been due to the technology of mass data analysis pioneered by and introduced to astrophysics by particle physicists. Sometimes, astrophysical experiments have shown the way. After 30 years of fruitless search for neutrino oscillations at accelerators, it was with naturally occurring beams from the sun and the atmosphere that they were finally observed.

Finally, it may be of interest to remark that there is a precedent for CERN participation in such physics. 50 years ago, the wise men who wrote the CERN convention specified, in addition to the SC, PS, ISR and SPS, the study of cosmic particles (in fact it is mentioned twice), and some of CERN's early experiments were without accelerators. For example the very first experiment to search for proton decay with Cerenkov counters was carried out by a CERN Group in the Lotschberg tunnel back in 1960.

By necessity, the CERN programme over the years has been progressively narrowed down further and further to one or two priority projects. Potentially important, smaller programmes, such as in radiation physics and biophysics have been dropped, to save money and manpower. I hope that, when the big projects like LHC and CLIC have been completed, CERN can go back to a broader and more balanced programme.

Martinus Veltman

Statement

Although I have very strong feelings on the subject altogether, I find it extremely difficult at this point in time to formulate them: the worry, of course, that we have is which physics is going to be done after 2011? Our Director General has made some display of what could or could not be done. There are things in there that we did not

Martinus Veltman

use to do and the question is whether we should go into other experiments of this type, non-accelerator type of experiments. I have always thought that CERN should maybe limit itself to accelerator type of experiments but even that is not entirely clear because you can have for instance neutrino experiments, such as a long baseline or stand alone experiments, and things like that. Personally, if I had my way, I would very much like it if around 2016 there would be a linear collider from 500 to 800 GeV, and I don't really care where in the world. The problem is that if you want to make this machine on an international scale it would probably get bogged down in the bureaucracy as we say. So I see these discussions going nowhere. Whenever CERN did something, it was something that grew out of its own initiative somehow and it was done here. So I see it very difficult in coming.

Another aspect that we should not forget is that we have another laboratory in Europe: DESY. If DESY doesn't get involved somehow with TESLA, we basically will be assisting at the end of DESY as a high-energy laboratory, a bit like Brookhaven National Laboratory in the USA. That's a serious question that we also should worry about: you cannot see that away from CERN, as CERN is not an entity in itself. I feel very strongly that CERN belongs to Europe. So, if something goes around in Europe I feel entirely free to say to CERN you have to do this or you have to do that. The only trouble is that usually that kind of thing doesn't happen and if anything moves, it usually has to come out of CERN itself. I really have very little else to say at this time.

Lev Okun

Statement

I hope that you don't expect from me the prediction of the future of physics. I thought about predecessors of this panel, the people who really made prophecies in physics, and chose three names: Glanvill, Klein, Budker. Concerning the future of CERN, I will mention Higgs, vacuum and CLIC.

Roots of the future (Glanvill, Klein, Budker)

One of the most impressive examples of prophesy I know was published by Joseph Glanvill (1636–1680), a founder member of Britain's Royal Society (1660), in his book "Vanity of dogmatizing" (1661).

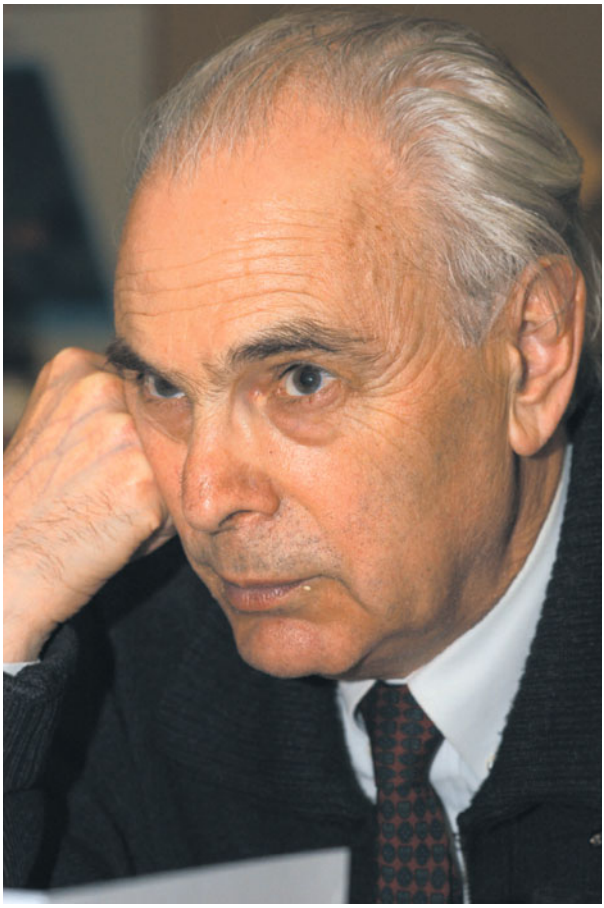
The full title is: "*The vanity of dogmatizing: or, confidence in opinions. Manifested in a discourse of the shortness and uncertainty of our knowledge and its causes; with some reflexions on peripateticism; and an apology for philosophy*."; London: printed by E. C. for Henry Eversden at the Grey-Hound in St Paul's Church-Yard, 1661.

I learned about Glanvill from my friend Igor Kobzarev (1932–1991). The original text was kindly provided to me by Rupert Baker, Library Manager of the Royal Society, and Tullio Basaglia, CERN Librarian. The relevant quotation is from pages 181–182:

"And I doubt not but posterity will find many things that are now but *Rumours*, verified into *practical Realities*. It may be some Ages hence, a voyage to the *Southern* unknown *Tracts*, yea possibly the *Moon*, will not be more strange than one to *America*. To them, that come after us, it may be as ordinary to buy a *pair* of *wings* to fly into remotest *Regions*; as now a pair of *Boots* to ride a *Journey*. And to confer at the distance of the *Indies* by *Sympathetick* conveyances [resonance transmission in modern terms, radio, WWW, LBO], may be as usual to future times, as to us in a *literary* correspondence. The *restauration* of gray hairs to *Juvenility*, and renewing the exhausted marrow, may at length be effected without a *miracle*: And the turning of the now comparatively *desert* world into a *Paradise*, may not improbably be expected from late *Agriculture*."

The last three problems have turned out to be too difficult to solve so far.

Oscar Klein (1894–1977) presented a gauge theory of W, Z bosons and photons at the Conference "New Theories of Physics", Warsaw, 1938. It was a prophetic talk, even the magnetic moment of the W was correct.



Lev Okun

Strangely enough in all his subsequent publications O. Klein never mentioned this remarkable contribution of his, though he was active during the period of creation of the Standard Model (he was a member of the Nobel Committee from 1953 to 1965).

Girsh Budker (1918–1977) had direct relation to the discoveries we are celebrating today. He used to say that the accelerators and especially the colliders of our age are like the cathedrals of the Middle Ages. He founded the famous Institute in Novosibirsk.

In 1965, the first e^-e^- collider produced physics results at Novosibirsk. In the same year Budker invented the process of electron cooling of protons.

In 1966, the work on the $p\bar{p}$ collider was started in Novosibirsk.

In 1967, their first $e^+ e^-$ collider produced physics.

In 1968, the election of Sasha Skrinsky to the Soviet Academy took place. Budker said: "His only drawback is young age, but with time it will diminish". Sasha was then 32 years old.

Budker's team created accelerators for practical purposes: to treat sewage, insects in grain, to develop various applications in industry, medicine, etc. This gave a certain financial independence to the Institute in the field of fundamental science.

Starting from 1974 Carlo Rubbia paid regular visits to Novosibirsk to see proton cooling in operation, before switching from electron cooling to stochastic cooling for the CERN $p\bar{p}$ collider.

Sasha Skrinsky is still playing a crucial role in the Russia–CERN collaboration, especially for the LHC.

The future of particle physics is unthinkable without intense international collaboration.

On the future of CERN

It is impossible to cover briefly even a few directions in particle physics in five minutes and also it is needless after the talks we heard today. I will stress only the importance of the discovery of the Higgs at LHC and R&D on CLIC.

Higgs is a bridge to the vacuum. The breaking of the vacuum symmetry is responsible for the masses of all the elementary particles. This is closely related to the most unusual property of vacuum ("dark energy") observed by astrophysicists.

Luciano Maiani yesterday at the Scientific Policy Committee said that it would be impossible for CERN to start building CLIC immediately after getting LHC working. I fully agree with him. To build one collider after another is unreasonable: we need ample time for physics on LHC. But there is a difference between construction and R&D.

It seems to me that R&D on CLIC should be intensified (and must be additionally funded) to get the decisive answer on the feasibility of the machine as soon as possible. This would drastically change the landscape of the future of particle physics in the world.

Robert Aymar

Statement

Not being a particle physicist, I am certainly not well positioned to tell you what will be the scientific future of particle physics and I will not attempt to do that. Nevertheless, listening this morning to the brilliant presentations on the past scientific achievements, I was asking myself about all the students the speakers have educated during their life, "Will they be as successful as their professors? What will be the support to their work?" The scientific challenges ahead are as important as before. Nevertheless, it may be the condition for the future of particle physics to have less support than it used to have 20 years ago. I was struck that the Member States of CERN, by making this very strong decision to launch the LHC in one step, reduced

Robert Aymar

the resources quite largely at the same time. I heard that in some Member States, a reduction has been occurring for a long time, and even today another Member State is inclined to follow this trend. I think we really have to devise ways to improve the situation; if not, I am afraid that the future of particle physics cannot be as bright as what we have seen until now. Thank you.

Steven Weinberg

Statement

I have been sitting here wondering what to say while I try to overcome my jetlag. Fortunately, several speakers have mentioned cosmology or astro-particle physics, which gives me a clue. The world of elementary particle physics for the last 25 years

Steven Weinberg

has not seen the kind of intense cooperation between theory and experiment that went on in the 1960s and the 1970s, though we keep hoping it will start again. In the meantime, cosmologists are in heaven, in more than one sense. Their theories are actually tested by observation, and the theories work, while observations suggest new theoretical ideas. It is really just like particle physics used to be and we hope will be again. My own work now is entirely in cosmology.

But some of the excitement over cosmology is exaggerated. I sometimes hear people say that it is astronomers who will solve the fundamental problems of physics by studying what is left over from the early universe. I do not believe this at all: I think in fact that one of the reasons that cosmology has been so successful is precisely that observable phenomena, for example fluctuations in the cosmic microwave background, depend very little on what happened in the very early universe, which makes it possible for theorists to make successful predictions about them. Of course, the other side of this is that observations tell you very little about what happened in the very early universe. I think that the great breakthroughs in really understanding inflation, reheating, all the rest of it, and also in judging the fascinating idea that there were many big bangs, will have to come from fundamental physics rather than from astronomical observation.

I do not know whether theorists are going in the right direction to find a more fundamental theory, but I cannot think of anything better to do now than what is being done. The work of the string theorists certainly has not led to the kind of success that at one time we hoped for, but lots of theories that seemed like good ideas have had a delayed success. Think of the Yang–Mills theory. It had obvious things that seemed wrong with it (specifically, massless gauge bosons) and it took a long time to figure out what it was good for. I think string theory – not perhaps in its present form but in some future form – will turn out to be the answer to many of the problems we have as particle physicists. The theory will be tested experimentally not by observing strings but by seeing whether it leads to a successful calculation of the 18 or so free parameters of the Standard Model. If that happens, then the theory will also provide the necessary intellectual foundation of cosmology. What I do not know is when this will happen. I do hope, for personal reasons, that it will be within the next decade or so.

Pierre Darriulat

Statement

It is embarrassing to be sitting at such a prestigious table while having no particular wisdom nor expertise to offer. All I can do is to make two rather obvious remarks:

- Today's future is LHC where we all hope that Higgses and supersymmetric particles will soon be discovered, not to mention the unexpected. The importance of this challenge is such that it kind of hides any other possible future. Yet, as soon as LHC starts producing physics results we shall start having a much clearer idea of what we really want as the next accelerator. Meanwhile, a continued R&D effort is required, in particular on CLIC as far as CERN is concerned. But scenarios where a subTeV Tesla-like machine would be a good choice are also easy to conceive and I regret that no serious (I mean at the scale required for such a project) R&D effort has ever been undertaken on Nb/Cu RF cavities (superconducting niobium film sputtered on a copper substrate). In my opinion this is the only way to build such a machine.
- For now two decades or so, astrophysics and particle physics have kept getting closer and closer to each other. There is no sign that this trend has changed, on the contrary. A stronger involvement of CERN in astroparticle physics would accordingly seem highly welcome.

Pierre Darriulat

Carlo Rubbia
I would like to call on Shelly Glashow if I may for a few comments.

Sheldon Lee Glashow

Statement

It's a pleasure to say just a word to this assemblage. I think that perhaps I am among a minority of theoretical physicists who still believe that the progress of particle physics is driven by experiment. And in particular, although things are rather quiet in experimental particle physics today they will become extremely exciting in the near future here at CERN at the Large Hadron Collider. And I would like, if I may be so immodest, to join the group of prognosticators that Okun referred to. I would like

Sheldon Lee Glashow

to make my own prediction and I would predict that the LHC will make astonishing discoveries which do not confirm the theories of anybody in this room. Thank you.

Simon van der Meer

Statement

Pierre just said that he was embarrassed. I think that I have the right to be even more embarrassed because I feel a complete outsider in this company. I have never done any particle physics and even in the range of machine physics I always felt like an amateur. So I will not say anything about the future of CERN, this is really beyond me. But what I want to say to all people who are working on machine physics is to think of two things: first of all, do not believe it when people tell you that something is impossible. Always try to follow up crazy ideas. And don't forget that all the experts in machine physics sometimes forget things which you can do by making some kind of "bricolage", those things, which people thought could not work, still work, if you work on it long enough. I think that's all I want to say. Thank you.

Simon van der Meer

Martinus Veltman

I think that I have seldom seen such a spectacle of modesty as in Simon's view on these things. So few people have contributed so much to this Laboratory as he has.

Thank you.

Carlo Rubbia

Statement

I believe that we have gone through everybody. So perhaps, I will add a few comments on my own if you permit me.

The first point is that indeed we should not underestimate the surprise capacity of LHC. I think this is the most important thing. It seems to me that we already take the LHC for granted and we are just looking farther than the LHC. I remember what Shelly Glashow was saying: that high-energy physics is now lacking surprises. I think that what we should say is that we do need surprises in this particular field and I expect that the LHC will be capable of very substantial surprises. For instance, suppose we don't find the Higgs, then what? It's not excluded that it wouldn't come out. It has to be very low mass, it has to have well-known cross-sections; suppose you run and you don't find it. Then what do you do? What is next?

Second question: if you don't find the Higgs you probably have a new structure. Structure, or Technicolor means a lot of levels, means a new Rosenfeld table and a huge number of discoveries to be made. Suppose you do find the Higgs, then the question will be to ask oneself why you found it. After all, the mass is relatively low and that demands very badly the necessity of something like supersymmetry which might keep the mass where it is and to do that at such a low mass means that supersymmetry is nearby. So you may find a very prolific number of particles of the sypersymmetric kind appearing, making a new spectroscopy the joy of an electron–positron collider, or a muon collider or whatever you can think of. Just like it was done at the time of the previous storage rings, SPEAR, ADONE, and DESY, to see charm and then all the other quarks states. I think this is a big challenge, it's not at all obvious what the answer is. So, we should first of all let Nature suggest what is to be discovered. I would say we stand a very good chance of getting something extremely exciting!

Now the other question of course is that, it's evident that 1 TeV is the highest level of masses which you can reach with present technologies. There is no question that we see limits in the technology of accelerators. For instance, if you were to increase the magnetic field two or three times above the field of LHC, the magnetic forces would be so large as to crunch the vacuum chamber and to destroy the Quads. That is a fundamental limit. If you want to build a linear accelerator of a reasonable size, you face gradient problems. Gradients cannot go over a few hundred MeV per metre unless you use lasers or maybe some other tricks. So the energy barrier is coming up loud and strong. The other difficulty is, of course, what was mentioned today as well, the luminosity barrier. The higher the mass of the objects, the smaller their cross-sections, for dimensional reasons. Therefore, you already deal with a machine like LHC which has one event in 10^{12} interactions. What are you going to do if you have to go ten times higher in energy and deal with 10^{14}, 10^{15} interactions per event? You have a formidable problem of finding the needle in a haystack. We've seen already how difficult it is with computing and so forth. So in a way we are witnessing many limits of technology which are connected to accelerators, large dimensions of laboratories, etc.

Carlo Rubbia

Now the question is what are we going to do next? Well of course nobody can tell what things will happen but I would like to give an example: in the 1930s, large telescopes, large astronomical telescopes were a little bit similar to today's accelerators. Mount Palomar, Mount Wilson were big things, they took many years to build, a lot of technology, a lot of users' problems, and so on, and they grew bigger and bigger, until they reached 4 to 5 metres in diameter which was the limit of the capacity of observation because of the air movement and so forth. And that hasn't represented the end of astronomy. Astronomy in fact was pushing to different fields. Radio astronomy was developed and gave you things which you could never see with an ordinary telescope. X-ray astronomy developed and gave you these Giacconi X-ray sources and now we have these magnificent examples of gamma-ray bursts of infinite intensity and so forth. So, whenever you have a technology which comes to an end then other technologies will be boosted. Different ways of doing high-energy physics, physics in space, underground, and so on, might in fact develop as a valid alternative to ever bigger accelerators, and I think that could become a bright future for high-energy physics. Thank you very much.

Carlo Rubbia
I am told that the two young physicists who have prepared a statement are now ready, so Fabiola Gianotti first and then Ignatios Antoniadis. Fabiola, you are the experimentalist, right?

Fabiola Gianotti

Statement

I would say that physicists of my generation look at the future with mixed feelings. With excitement and enthusiasm, but also with some worries and concerns. Excitement and enthusiasm because of the fascinating questions still open in front of us. You are among the fathers of the beautiful Standard Model, but if you allow me, ... and with all due respect, ... the Standard Model remains a work in progress. So there is room for big discoveries and big surprises in our field, there is room for new ideas, which makes our future thrilling and I would say ... sexy.

Excitement and enthusiasm for the LHC, which will bring years of wonderful physics. Many people in this room have made discoveries which have changed the world. But the LHC will also change the world... However, we should be careful because the LHC is not yet in our pocket, so it should be the overriding priority of the Lab in the next few years. And I must say that we, the "young" physicists of the Lab, are strongly committed to it. Further delays would be detrimental, in particular for the young generations.

Excitement and enthusiasm also for the diversity, I would say goal and scale diversity, in our experimental approach. We have high-energy colliders, neutrino

Fabiola Gianotti

facilities, B factories, experiments at the border between particle and astroparticle physics. And yet, in this diversity there is unification, there is a kind of *fil rouge* which connects all our efforts and comes from the necessity of interpreting all the results within a unique theoretical framework, from a unified view, like the various pieces of a puzzle. This puzzle includes also understanding the universe, which requires the concerted attack of particle physics and astrophysics and cosmology. The young generations think that we should strengthen the links between these two fields.

We have also several worries and concerns. First, what I would call the "space–time" concern of our big experiments. The long lead time for the realization of our big experiments is orthogonal to the spirit and dynamism of the young generations. Young people are impatient . . . The big size of our collaborations makes the contribution of the individual less easily recognized and visible than in the past. This "space–time" feature of our big experiments does not attract the young bright physicists.

We are also worried because of the missing financial resources, which can jeopardize the vital diversity in our approaches and force us, both at CERN and worldwide, to concentrate on a very small number of projects. This way we are not going to complete the puzzle... Note that an adequate funding should also allow for the unexpected which may arise from the richness of our field. I mean, the World Wide Web was in no 5-year plan. And if our chairman had been a very strict manager of the Lab, in the most narrow and blind sense of the term, maybe the Web would not be there. So I think that we should resist this trend, we should resist by advocating the importance for mankind to complete the puzzle, by capitalizing better on our scientific achievements and on the front-line technologies that we have developed for our instruments, and by becoming really global in our choices. That is avoiding decisions that are driven by the interests of a continent, or a country, or a Lab.

So I think that we have great opportunities ahead of us, as great as you had in the past, if only we can master the challenges. And history tells us that we have never been stopped by scientific or technical challenges, so hopefully we will not be stopped by other problems.

Ignatios Antoniadis

Statement

I would like to discuss a few points related to the research aspects of the Laboratory. I believe personally as a theorist, that LHC will discover the Higgs for several reasons. First of all it's an important part of the theory and second as we know today from the experimental analysis of LEP data based on precision tests, there is strong indication for the existence of an elementary Higgs, which should be light. Thus, physics will not end with the discovery of the Higgs but instead a new era will start. This is because the introduction of the Higgs brings new problems, such as the mass hierarchy and the origin of the electroweak symmetry breaking, which require certainly physics beyond the Standard Model. There are several theoretical ideas to address these problems. The most popular one that has been discussed here extensively is supersymmetry, but there could be also more exciting possibilities, such as large extra dimensions

Ignatios Antoniadis

of space, or TeV-scale quantum gravity, or even string theory with low fundamental scale.

I believe that LHC has good chances to make spectacular discoveries pointing on what can be the physics beyond the Standard Model but certainly cannot explore this new physics. We should therefore start already discussing and preparing the next most appropriate experimental facility, which will explore the physics beyond the Standard Model. This is precisely the second point I would like to mention. In particular, I would like to express some worries about the recent evolution of CERN, related also to the last financial crisis. I believe we all agree that our main actual focus should be the construction of LHC, but at the same time we should keep an eye on the future. In this respect, there are several activities that should be maintained. One is R&D for the next experimental facility and future detectors technology, as was also mentioned by other speakers. It is the right moment to start since high-energy experiments require a long time of preparation.

Another issue is the training aspect of CERN mainly of young researchers. For experimentalists, it would be vital to maintain a partial participation of CERN in other experiments, waiting for LHC. It is also very important to strengthen the visitors program, both in theory and in experimental divisions. And here is the last point I would like to mention, concerning the role of the research staff at CERN, in view of the restructuring procedure. Unlike universities, the research staff at CERN is limited to theory and experiment in high-energy physics. Both units played a leading role in the Laboratory, since its creation: participating in the decisions of CERN at all levels and determining its programme. It is important that the new structure of CERN guarantees the continuation of this role. I would like to finish by saying that personally I am confident that CERN will continue to play a leading role in promoting fundamental research in high-energy physics world-wide, because I believe that there is exciting physics beyond the Standard Model and thus there are new discoveries that are waiting for us in the near future.

Carlo Rubbia
We have a few more minutes left, so I wonder whether there is anybody who would like to add something? May I ask if anybody wants to comment on this, we have so many distinguished people in this room, I'm sure that most of us would like to hear from them. Herwig Schopper, please . . .

Herwig Schopper

Statement

I think it's always difficult to make predictions, in particular concerning the future, a well-known statement. Hence I agree with what was said by several people just now, in particular by Shelly Glashow, that there will always be new topics, new questions, new excitement, so there will be no end of high-energy physics because of lack of open questions. In addition, I would claim that neither will financial problems be the

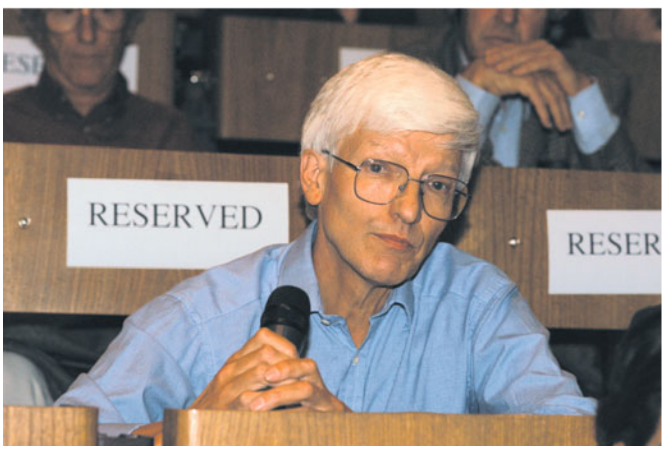
Herwig Schopper

end of high-energy physics. There have always been financial problems and I've gone through many crises. I think that when there is a good argument, society will provide the necessary funds. Certainly we are spending large sums, but after all, compared to the GNP the expenditures for high-energy physics are still small. So I think the financial problem will not be the real source of trouble. I rather think high-energy physics would be in real difficulty, like all other fields of science, if young people don't come anymore and I have been saying this for many years. I was listening very carefully to Mrs. Gianotti's remark. She said the long time-scales which are associated to our projects are against the impatience of young people and these long time-scales I consider indeed as one major problem in our field. I once talked to Mr. Smoot of the COBE experiment. He told me he had left particle physics because he hated large groups and long time-scales, but he found himself in a group with 150 people and time-scales of 12 years. So this problem arises also in other fields. But we really should think very hard what to do, how to organize the life of young people who will not stay permanently in high-energy physics, but who will go to other professions later. How should we make it attractive for them to spend 5, 6, 7 years in our field, be satisfied, learn about excitement, but finally be qualified to find other possibilities?

Christopher Llewellyn Smith

Statement

Well, Carlo, since you ask, I would say first that I agree very much with what Herwig Schopper just said. During the year that I spent in the 1980s as advisor to the enquiry that was set up when the UK was hesitating about membership of CERN, I was asked several times – "How will we know when particle physics is finished?". I answered – "When we stop attracting outstanding students". I still think that operational answer



Christopher Llewellyn Smith

is correct, but the situation is worrying. The long time-scales, and the size of the large collaborations, do not look attractive. We have to find a way to keep up the momentum of the subject and the interest of young people.

As far as the future is concerned, I feel that I have spent too many years talking about it. I'm fed up with this, and I wish the future would now come, in the form of the LHC. We need results. I agree with the many people who have said that it's very difficult to say what we should do next until we have results from the LHC. We don't know. I agree with Shelly Glashow: let's hope there are surprises. There are two things that I can say, however. First, I agree very strongly with what Georges Charpak has said: you need to have some oxygen in a decent laboratory in order to develop new ideas, and it has been crushed out of CERN by the pressure we've been under from governments. We must find oxygen to feed new ideas. I also agree with what has been said about CLIC: we really should be pushing it faster so that it is ready as an option. Whether we want CLIC, or something else, will not be clear until we see results from the LHC. But meanwhile, if we can, let's push CLIC and let's try to get back some oxygen.

Valentine Telegdi

Statement

Well, those amongst us who have done their best work 30 or 40 years ago sometimes feel sorry for the young people nowadays who have to join enormous groups. The problem of individual contributions to be identified in large enterprises has been raised and I think it's also connected with attracting new people into our field. Now, I have given this matter some thought and my thoughts have evolved in the following way: people have asked me "if you were young again, would you go into physics again?". Well, I thought about it and my answer was "yes". Why? Because it's a

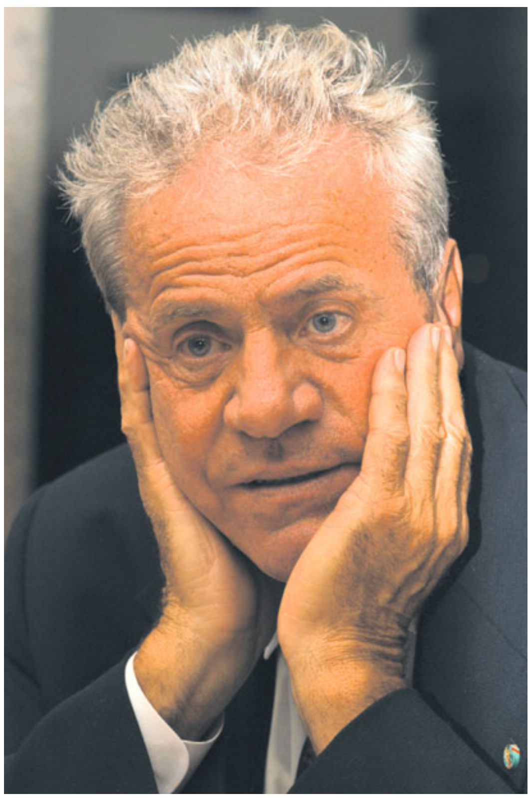
Valentine Telegdi

place where facts are understood in a clear and analytical way. Two: “would you go into high-energy physics?” My answer: in the usual sense of the word “absolutely not”. So “what would you do?” Well, to me, the most important thing in scientific work is independence. This is my personal point of view. And it seems to me that there is a perfect possibility for ambitious young people to be independent. And that is if you either work on accelerators or better yet on instrumentation. You heard Georges Charpak’s lecture, we heard Simon van der Meer’s extraordinarily modest remark. It didn’t take 50 people to invent the horn of plenty. So I believe that if I were young again, I would go into physics instrumentation, it wouldn’t bother me at all if somebody else made great discoveries with my gadget provided that my gadget was very original. Thank you.

Giorgio Bellettini

Statement

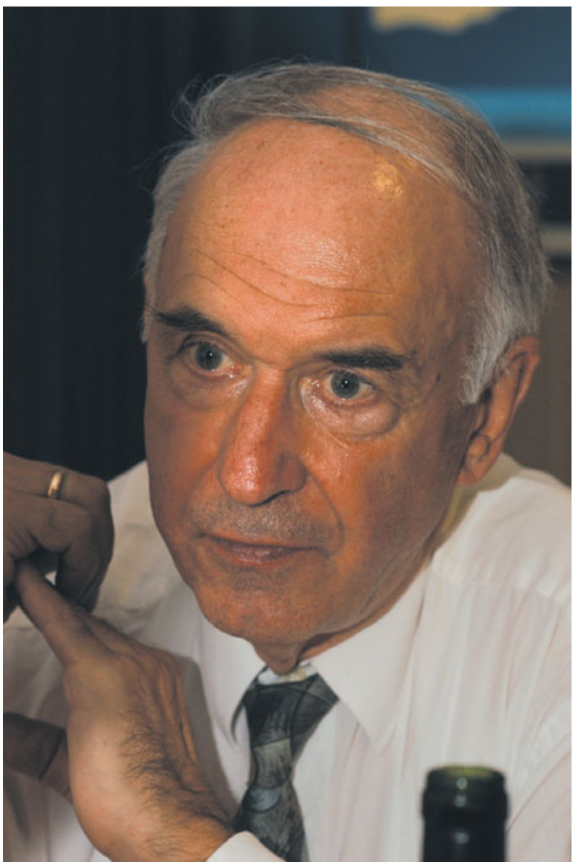
The comment was made by Martinus Veltman that one should not elaborate on the long-term future of CERN and forget DESY. I think we should extend this concept.



Giorgio Bellettini

I believe that CERN represents about 50% of the total effort on Earth in particle physics, which means that we are too large to believe that what we do is irrelevant to the others, it could be very important; on the other hand, the others are also so large that we should not forget about what they are doing. Therefore, I believe also that if we did take such a partial attitude, reality will force us to consider the overall picture of particle physics on Earth, reality is for instance funding. Luciano Maiani said that possibly 10% of the support for the new lower energy linear collider could come from Europe. It's implicit in this sort of attitude that some of us care about this machine, it will be hard for CERN and certainly for Europe to take such an attitude, and I think that inevitably we have to complicate the issue and to consider a wider sort of scenario.

Carlo Rubbia
Volker Soergel, would you like to add a few words?



Volker Soergel

Volker Soergel

Statement

Well, I thought I said no, but now that I have a microphone, I can say a few words. I agree with what Martinus Veltman said. We should not restrict ourselves to the programme of CERN alone. We should have a good programme: at the moment we have two High Energy laboratories, we have DESY, and I think that DESY has done a very interesting development with HERA and a good scientific organization. DESY has made a good development towards the sub-TeV linear collider. The present management of DESY is very strongly in favour of being part of such a project and Albrecht Wagner always said that, whether it is built in northern Germany or it is built in another place in the world, provided it has an interesting technology, DESY would be interested to be a major player in such a project. I hope that this will be true. I agree with some of the statements made before that for the big discoveries – hopefully of LHC – we should have a linear collider in the sub-TeV region, which does not come 10 years after the start of the LHC but hopefully when we have discoveries which we would like to explore more, for which the general belief is that the linear collider is an ideal machine. So I hope that DESY will be part of such a future. How will the funding come? I don't know, but I hope there will be a good cooperation between CERN and DESY towards such a project.

Carlo Rubbia
I think we have passed the allocated time by a few minutes. I think that most of the important things have been said, so I would like to close this session. Thank you every one of you for being here today, thank you CERN for the perfect organization and thank you in particular to the speakers for their very nice presentations we had this morning.

First published in Eur. Phys. J. C 34, 91–102 (2004)
Digital Object Identifier (DOI) 10.1140/epjc/s2004-01772-x

Additional contributions by Sheldon Lee Glashow, Donald H. Perkins, and Antonino Pullia

Sheldon Lee Glashow

Comment on the occasion

I welcome this opportunity to celebrate the marvellous accomplishments of CERN, which is arguably the most successful of all international organizations and a showpiece of world-wide cooperation. I have been a visitor here on so many occasions: first as an NSF fellow in 1959-60, then as a paid visitor for a semester, as a frequent summer drop-in, conference participant, and member (now "old-boy") of the Science Policy Committee. These experiences have been central to the evolution of my own career in theoretical physics. Many of the experimental discoveries underlying our Standard Model have taken place here, among them the discoveries of neutral currents, weak intermediaries, and the many precision tests of the electroweak model carried out at LEP. But of equal importance is the fact that so many crucial developments in fundamental theory were either initiated, nurtured or perfected at CERN, by both its resident and visiting theorists. This is certainly so for me, as I am certain it is for many of my distinguished theoretical colleagues. CERN has always been, and must continue to be, the place where the action is, the Grand Central Station of particle physics, the crossroads of thousands of individual physicists' lives.

Today's Standard Model is a successful theory of almost everything. Although it has so far met every experimental test, many important questions remain unanswered, especially concerning the origin of electroweak symmetry breaking. More than ever before, the world-wide community of particle physicists is dependent on CERN, and in particular, on its timely construction, deployment and instrumentation of the Large Hadron Collider. Indeed, if not for the LHC and its enormous discovery potential, our discipline, already in distress, would be facing imminent demise. But we must also look beyond the LHC. As my colleagues and I have argued elsewhere, CERN must strive to preserve and transmit to future generations the hard-won art and know-how underlying our discipline. Only then can CERN continue to contribute, as it has done so magnificently in the past, to a better Europe and a better world.

Donald H. Perkins

A comment on perturbative QCD in early CERN experiments

We have heard today of how neutrino experiments and those at the proton–antiproton collider led to the discovery of neutral currents and the W and Z bosons, so validating the electroweak theory. I just wanted to remark here that these same experiments gave some of the first quantitative support for perturbative QCD, that other component of the Standard Model.

The first graph (Fig. 1) shows some results from 25 years ago on nucleon structure functions from the Gargamelle neutrino experiments at the PS and those in the BEBC chamber (Bosetti *et al.* 1978) and by the CDHS counter experiment (de Groot *et al.* 1979) at the SPS. Taking the difference of neutrino and antineutrino charged-current cross-sections measures the non-singlet structure function, that is the distribution in momentum fraction x carried by the valence quarks. Perturbative QCD makes a very simple prediction: the moment of the x-distribution varies as $1/\log q^2$ to a certain power, called the anomalous dimension, which depends on the order of the moment, the SU(3) nature of the colour symmetry and the spin of the gluon. Hence if one plots two different moments against each other on a log–log scale as q^2 varies over the range from a few GeV^2 to about 100 GeV^2, one should get a straight line with a slope equal to the ratio of the two anomalous dimensions. In fact the observed and calculated slopes agreed to within the errors of 5–10%, and verified the vector nature of the gluons. Scalar gluons – the dashed lines – were excluded at the 4σ level, long before the three-jet analysis at PETRA gave the same result.

Both the UA1 and UA2 experiments analysed the distribution of two-jet events at large angle, as a quark, antiquark or gluon from the proton scattered from one from the antiproton. The second graph (Fig. 2) shows the CMS angular distribution $d\sigma/d\Omega$ of these events in UA1 (Arnison et al. 1984), expected to vary roughly as $(1-\cos\theta)^{-2}$ (or as Rutherford would have said, $\text{cosec}^4\theta/2$), corresponding to a $1/r$ potential mediated by vector (gluon) exchange. Again scalar gluons are excluded. It is interesting to compare that distribution with the one found by Geiger and Marsden exactly 75 years earlier (1909), for scattering of α-particles by gold and silver foils, again for a $1/r$ Coulomb potential mediated by vector (photon) exchange, shown by the dashed line. There are three differences. First, in the collider experiment there is a θ, $(\pi-\theta)$ ambiguity in the jet direction, so the distribution for $\theta > \pi/2$ has been folded into that for $\theta < \pi/2$. Secondly there are quark spin effects which do not apply for the spinless alpha-particles, which affect the distribution at large θ. Finally, while the Rutherford cross-section varies as α^2 which is essentially constant at the values of $q^2 \sim 0.2$ GeV^2 involved, the quark–quark scattering is proportional to α_s^2 which runs significantly over the relevant q^2 range of 100's to 1000's of GeV^2, so that at smaller angles and momentum transfers, the points deviate upwards from the line. But despite these differences, the similarity between these distributions strikes me as remarkable and a nice demonstration of unity in particle physics.

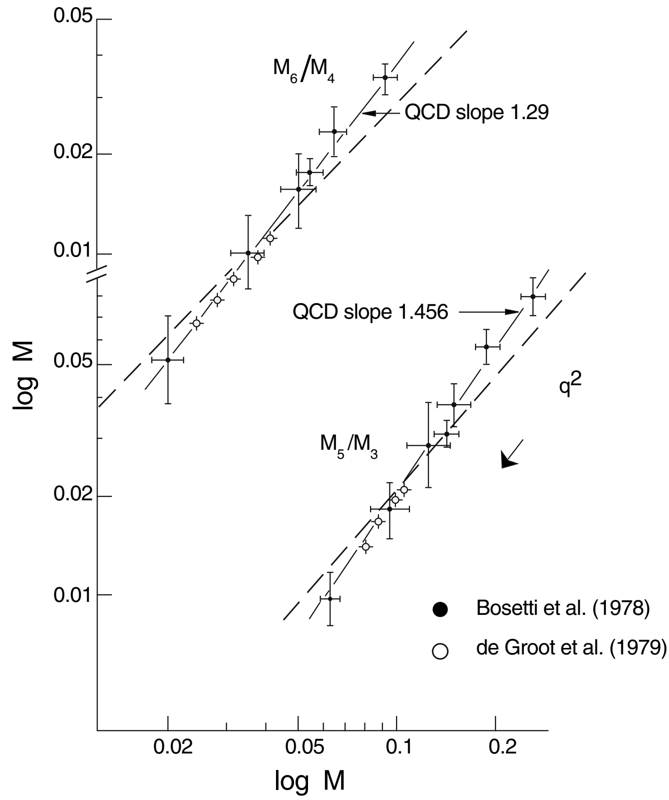


Fig. 1. Moments of non-singlet structure functions

Antonino Pullia

Let me make some personal remarks about the Neutral Current discovery in Gargamelle.

General remarks

I would like to remind you that during the 1960's there were good reasons to disbelieve the existence of neutral currents. Processes such as:

$$K^+ \to \pi^+ + \nu + \bar{\nu}$$

were highly suppressed [1]: the branching ratio for this kaon decay mode was less than 5×10^{-5}. Many experiments placed other, similar upper limits on strangeness-changing neutral currents. Since there was no reason at the time to believe that any relevant distinction existed between strangeness-changing and strangeness-conserving

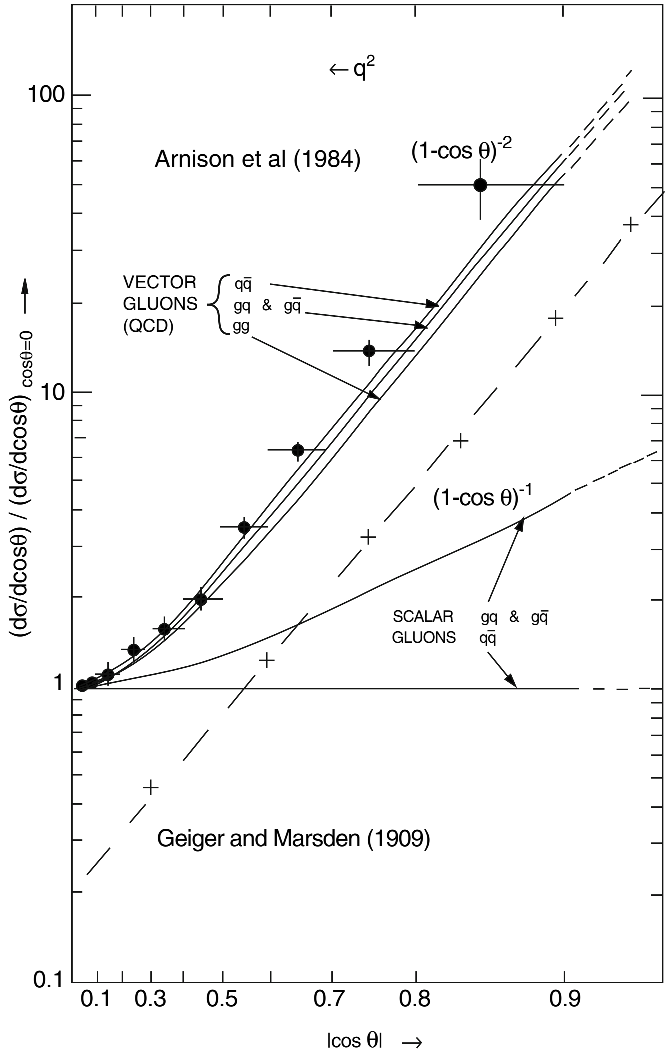


Fig. 2. Centre of mass, angular distribution of two jet events as measured by UA1, in 1984

neutral currents, the reasonable conclusion that many physicists reached was that neutral currents simply did not exist.

Furthermore experimental limits were established also on strangeness-conserving neutral currents processes [2, 3]. I would like to remind you that the original Weinberg–Salam theory concerned only leptons and that quarks played no role at all. The experimenters were furthermore and correctly attracted by the new discoveries on partons at SLAC and by the opportunity to measure their quantum numbers by the interactions with neutrinos.

So in such a framework the search for neutral currents was not a high priority in the experiments in the world.

At the beginning of the 1970's theorists took a new interest in neutral currents; let me recall:

- the very important work of Glashow, Iliopoulos and Maiani [4] postulating a mechanism invoking a fourth quark and suppressing the strangeness-changing neutral currents, but allowing the strangeness-conserving ones;
- the work of 't Hooft providing the renormalization proof of the Weinberg–Salam theory [5];
- the calculation of the experimental consequences on the semileptonic neutral currents induced by neutrinos of Paschos and Wolfenstein [6] and Pais and Treiman [7].

Remarks on "Gargamelle"

I remember well a colloquium held in the small library of Gargamelle's building at the beginning of 1972 with Paul Musset and Bruno Zumino, Jacques Prentki and Mary K. Gaillard. Zumino explained to us the theoretical fascination of the new renormalizable theory of Glashow, Salam and Weinberg, suggesting the search for the muon neutrino and antineutrino scattering on electrons.

We (The Milan group and P. Musset, D. Haidt et al. at CERN) were already engaged in the study of the semileptonic neutral currents (see the meeting of the Gargamelle Collaboration in Paris in March 1972 where, on behalf of the Milan group, I offered preliminary evidence on the neutral currents existence [8]).

Just for fun, I remember that people of the Milan group found themselves out of their offices at the via Celoria, as students had occupied the Institute (Students Protest!). We had therefore an internal meeting in my house to prepare the Paris meeting of the Collaboration!

The very important problem of the neutron background is very well treated in the talk of D. Haidt in this Symposium. Probably he forgot to mention that the main author of the effort in this direction was himself. In early January 1973, the Aachen group found the famous candidate for a $\bar{\nu}_\mu + e^- \rightarrow \bar{\nu}_\mu + e^-$scattering.

The background for this process was really very small and at this point the whole Collaboration was excited and the search for neutral currents then stood at the center of everyone's attention.

Let me finally recall the strong pressure applied by A. Rousset and A. Lagarrigue to finalize the analysis of the events (done mainly by J.P. Vialle) and to publish a letter (July 1973).

I personally believe that without their strong belief, the collaboration would have delayed the publication of this very important discovery.

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First published in Eur. Phys. J. C 34, 103–105 (2004)
Digital Object Identifier (DOI) 10.1140/epjc/s2004-01773-9

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