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Panel discussion on the future of particle physics chaired by Carlo Rubbia – (ENEA and formerly CERN)

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

C. Rubbia

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Introduction by Carlo Rubbia

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 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.

Carlo Rubbia

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 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

Donald Perkins

Martinus Veltman

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.

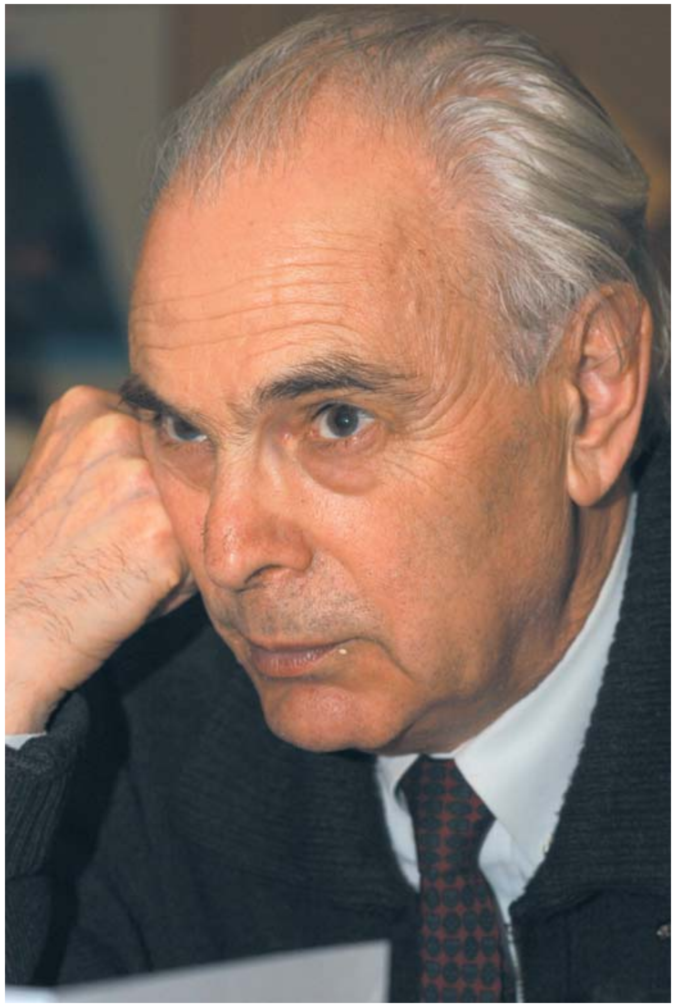
By necessity, the CERN programme over the years has been progressively narrowed down further and further to one or two priority projects. Potentially important, smaller programmes, such as in radiation physics and biophysics have been dropped, to save money and manpower. I hope that, when the big projects 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 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

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.

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

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 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 has not seen the kind of intense co-operation 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.

Steven Weinberg

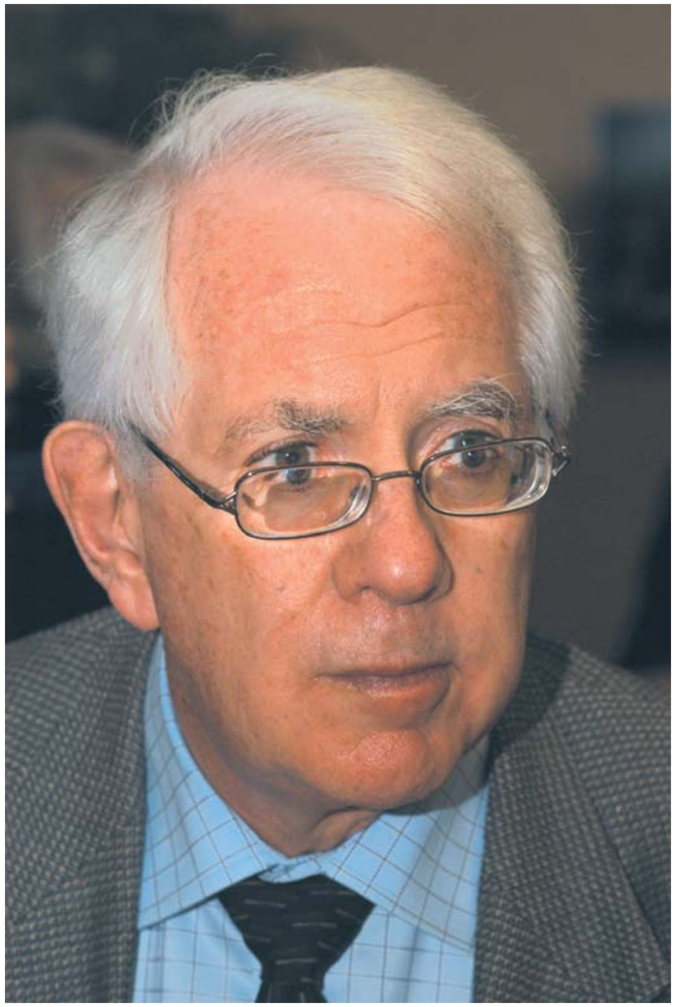
Pierre Darriulat

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

Pierre Darriulat



Sheldon Lee Glashow

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.

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 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

Simon van der Meer

Carlo Rubbia

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.

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.

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 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

Fabiola Gianotti

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

Ignatios Antoniadis

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

Herwig Schopper

distinguished people in this room, I'm sure that most of us would like to hear from them. Herwig Schopper, please . . .

Herwig Schopper

Statement

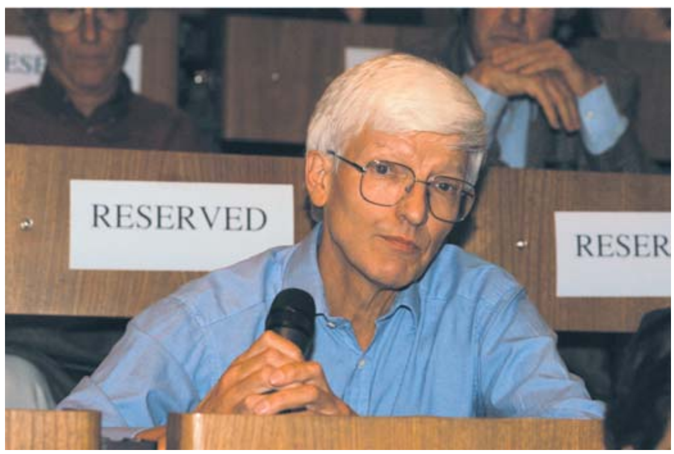
I think it's always difficult to make predictions, in particular concerning the future, a well-known statement. Hence I agree with what was said by several people just now, in particular by Shelly Glashow, that there will always be new topics, new questions, new excitement, so there will be no end of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

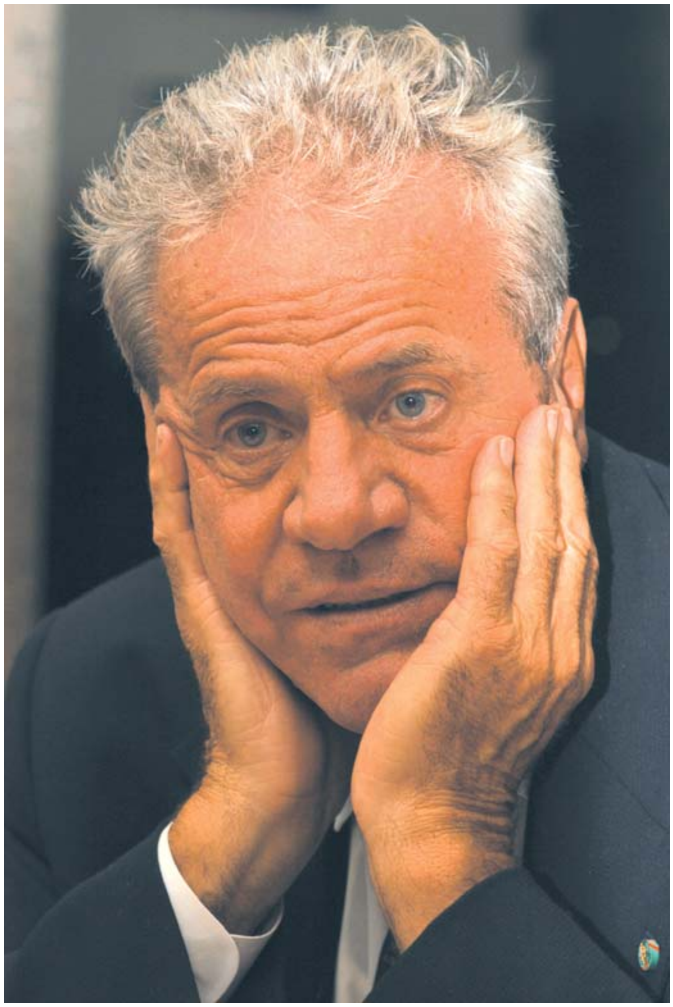
As far as the future is concerned, I feel that I have spent too many years talking about it. I'm fed up with



Christopher Llewellyn Smith

Valentine Telegdi

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.



Giorgio Bellettini

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 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

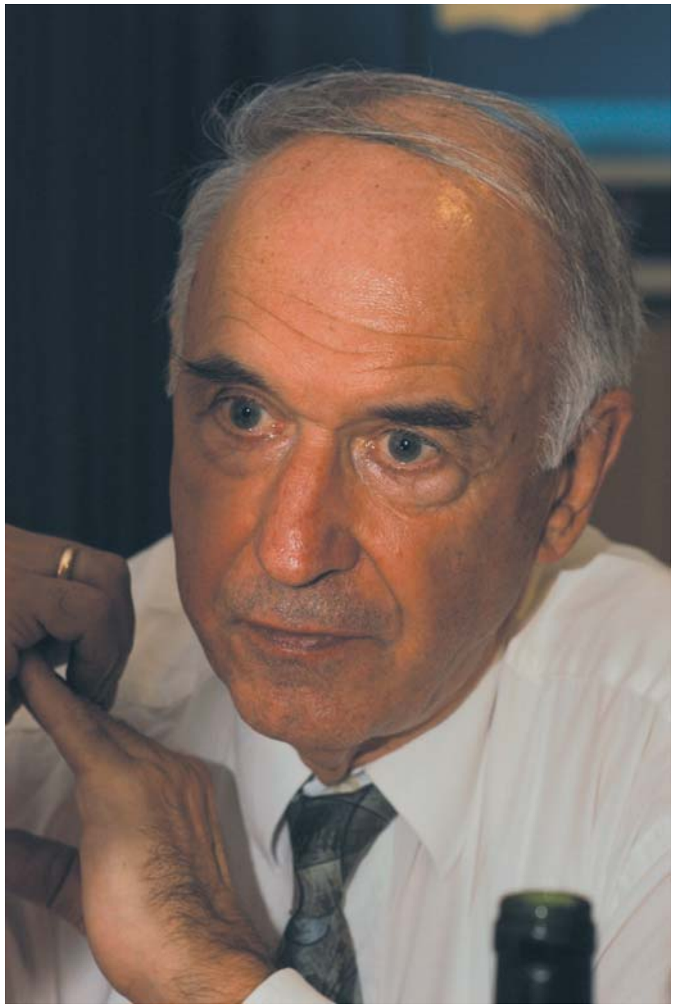
The comment was made by Martinus Veltman that one should not elaborate on the long-term future of CERN and forget DESY. I think we should extend this concept. I believe that CERN represents about 50% of the total effort on Earth in particle physics, which means that we are too large to believe that what 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

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.



Volker Soergel

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.