The CHAIRMAN invited Professor Weisskopf to give his introductory talk.

Professor WEISSKOPF:

"I am supposed to give an introductory talk to this discussion and I will take this very literally, I do not want to give you any long discussions. It is up to all the following speakers.

I am very glad to see this hall completely filled; this shows how much the topic of the future high-energy physics in Europe is in the centre of interest where it belongs. Let me start with a few historical remarks. We know the history of CERN itself; that CERN was created, one says, to bring modern basic physics back to Europe; but when you really look at the history, as I did for a few minutes at least when I tried to prepare this talk, I kept on thinking that this was a vastly exaggerated statement, because in fact when we look at the number of people and the number of machines, basic physics really never left Europe. True enough the United States have constructed more machines than Europe - the ratio is perhaps something like 1 to 3 or 1 to 4, it depends how one counts it - and that has not changed so much. There is also the Soviet Union here; if you look at the number of machines, the Soviet Union compares with the number of machines in Europe, it is the same, perhaps even slightly less. I think that the essential point in this history of these post-war years of high-energy in Europe is the following: that it became clear some years ago that the development in Europe could not go on on a national basis, that the machines became too big; the great step forward in scientific political thinking was therefore to construct an international machine, and it was necessary in order to keep up with the development that nations should get together to build a machine, and the result of this was CERN. So, I think the significance of creating CERN did not lie so much in the tremendous effect this had on European physics in general, because I think some of the development of European physics is just due to the increase of activity in all parts of human endeavour, the main significance of creating CERN lay in the fact that the national barrier was broken, and this is naturally a great thing in itself. Now, of course, we have to go on. Clearly, if the national barrier has been broken for a 25 GeV machine, it will have to be broken also for the next machines. This is why, if we want to go on the way we do and, of course, we want to, the international frame, at least the inter-European frame will be a necessary one."
Now let me, right away at this point, in order to keep the discussion within bounds, ask this question: 'Will it be necessary or desirable to break the European frame for the further progress of high energy physics?' There are two questions: 'Will it be necessary' and 'will it be desirable'. As I will mention a little later on with a little more facts, I do not think it is necessary from the point of view that the European countries could not afford a machine of the order of 9-10 times larger than our present machine, and the necessary additional broadening and strengthening of fundamental physics. So I think honestly one should say it is not necessary. Desirable, that is quite a different thing. A machine that goes beyond the European frame is certainly desirable from a human, political and idealistic approach. However, I think, though I do not want to discuss this in detail here, that the general constellation of events and of attitudes is such that this so-called world machine, which might not be built by the whole world but maybe Europe, Russia and America, that such a machine is probably not on the cards for the next step, but one might hope that it will be on the cards for a step after the next step, and one must fervently hope that it will not always be the n+1st machine that is built.

Now, the tasks which are before us when we discuss future plans of machines are very wide. They are technical ones: how to build the machine?; there are physics questions: what shall one do with such a machine?; there are political questions and manpower questions: do we have and shall we get the necessary engineers, do we have the necessary physicists to work with the machine and where do we get them?; there are financial questions: where do we get the money, will the governments be able to pay for such a machine?; and there are organizational questions: how does one carry out such a tremendous enterprise?; and so on. But I think that, fundamentally, the first question which underlies all the questions I have mentioned and which must be clear, is a philosophical one: 'Why do we want to build such a machine?'. I think it is necessary to spend at least a few minutes on this question, because physicists would perhaps not ask it but everybody else will, and let me say it is a very difficult one to answer and so let me perhaps say a few words. There is, of course, one fundamental attitude which underlies all questions of this kind: one could say 'it is possible to construct a machine of 300 or even 1000 GeV or more', so if it is possible why not do it? Men should do everything that is possible. This is of course an argument that, in this naked form, will convince nobody. However, I think it is at the basis of very many human activities. If we take, for example, the climbing of Mt Everest and similar things that are certainly only done because they are possible, going to the moon or Mars or Venus or other planets, probably belongs in the same category. And again I would rather not discuss with you the space efforts and their usefulness or not, but there, of course, already quite different things come in. In any case it is like any exploration 500-600 years ago: it is possible to sail around the earth, so we have to do it. But just by quoting this last example, of course in all of your minds, you will right away think: 'well, in this case, it not only had very important consequences but probably some of these consequences were even in the minds of people who did it, at least in those of people who
paid for it. The consequences were different from what they expected them to be, but that is another matter. The problem of high-energy physics is certainly going to be looked at the end fundamentally by governments, by people, by non-physicists, probably from the same point of view, let us say, as the Spanish Government looked at the Columbus Trip: it is terribly crazy, it will probably not succeed, but it has some very great prospects that are not very clear but certainly will be tremendously attractive and important for the world. This is probably the attitude of non-physicists or, let us say, not philosophically inclined people towards questions of this kind, and it is probably the correct attitude. Every great enterprise into something very new will have some correspondingly great effects which just cannot be formulated. This is certainly also an argument in the space research, and, it is a very strong argument; it is probably the strongest of all. For us, however, the argument lies in a different place and we might as well be clear about it ourselves, if I can express it correctly in the name of physicists. In the name of physicists of course we are doing it because we are interested in the structure of matter, not only because it is fun. It truly is fun, but this is not the only reason. We do it because we are aware of the fact that, for the understanding of nature, we have to go to the inner-most part of it. This is particularly justified if you look back and see what a tremendous success the idea of elementary particles has in that field where we think that we understand it more or less, namely in that world that consists of neutrons, protons and electrons, where we have begun in the last decade to see the whole thing round it. We even have a kind of almost historical view of nature before us, namely, the development of the world as we see it, including the universe we see, from, let us say, a hydrogen cloud to human beings. This world of historical development begins to shape and in all its details presents us with an enormous amount of insight in what is going on and what we can do with it. Then, of course, this picture still in all its tremendous generality and depth is in fact an incomplete one, an essentially incomplete one, because we must ask the question: why is it that those three elementary particles exist or two, if you want, the nucleon and the lepton in all its different forms? That, as you know, is the task that we are facing and it is the task we have just started to scratch the surface of and a task where it is clear that we have to go to higher energy in order to get seriously into it. Where this task leads us, and now I mean purely scientifically not from the point of view of society, as I mentioned a few minutes ago, where this leads us is very hard to say; but it is certain, in this case, probably with even greater certainty than its general significance for society, that it will lead us to vastly more insight into matter than we have, and from the philosophical point of view it probably also will lead us to an even more historic approach to matter than we have now, namely, not only into the question what matter is but how did it come about. I personally am convinced that the questions of the expansion of the universe, of the creation of matter and the question of beginning and end are all included in this research. My own conviction that the answers to those questions are in that line of research is stronger for me and this prediction to my mind is a safer one than the prediction that this kind of research will be of great significance for society in general, but I don't know. This is really pure
speculation. I wanted to make these remarks - I won't say much more about it only to give you an indication of what I think is the scope of the problems when one is discussing these questions - I don't think one will ever be able to formulate this better, but my personal feeling is that it is not so much the formulation as the scope that counts; one must be aware that these are questions of extreme importance not only for us as physicists, but for us as philosophers, for us as human beings, and for us as members of society; and I think the question must be approached with the spirit and I think that this spirit is easier to transmit to non-physicists than one generally thinks, because we are all human beings and that is a very important point.

Now let us go back to more mundane questions and see how the development of high-energy physics in Europe looks and how a new machine would fit into it, or the development of physics in general. Well, it is quite clear, as I said before, that the development of science and of physics in Europe has had a tremendous increase. It has an increase which one can almost predict from all other increases, now that the economy of Europe increases at something like 6 or 7% a year; and it is a well known fact that scientific activities increase faster than this. That is so in the United States and it is the same in all other countries, in fact this is a thing that will worry us sometime and I would like to mention this point, because it has some bearing on our own problem, namely that - if you look at statistics of scientific activities, whatever way you measure them, whether you measure then in terms of people, money, number of publications and so on, scientific activities double in 12 years - whereas other activities, for example the number of population double in something like 25 years. From this we can conclude that there comes a time when everybody has to be a scientist, and this would be at the end of the century if we follow up all lines. This brings up very important problems, namely that sometimes one has to slow down the development of science and the question then is and this is why it is important to our problem, where will you cut it down. You can cut it down completely evenly of course, but we have to slow down sometime towards the end of the century, the development of science has to be slowed down and then one has to decide which science. Fortunately we are not yet at this point but your children will be. We are not yet at this point, because it is clear that the amount of science done for example in Europe is relatively low. Now what does relatively low mean? We have on this a very interesting paper which Amaldi gave at the Frascati accelerator meeting several months ago, in which he brings forward the interesting and provocative numbers, though in fact the number I am now quoting is not by him - it is an estimate that has been done all the way round - namely what is a reasonable amount of effort one should put into science and in particular, basic science. Many agencies here and in the States have estimated that 2% of the national income spent on science is a normal figure. This includes however also applied science and development. Of these 2% of the national income, a tenth, that means 0.2% should be devoted to basic science. This is an important figure. Now, basic science - high-energy physics plays a large rôle in this number mainly because instruments are expensive and there are 2 basic science fields and these are high-energy physics and space. I do not want to open a discussion on whether space is basic science - we assume it is - and there are astrophysics,
which is also expensive, radioastronomy, biology and so on, but biology compared
to these sciences is not so expensive; astrophysics is, so there might be a
good estimate to say that it would be reasonable to devote roughly a third of
this to elementary particle physics. As I said, that does not mean that a third
of basic physics is elementary particle physics but it is the expensive part
and this is why the number is relatively high - that means .066% should be spent
on elementary particle physics. One would like to know then. Are we doing
this? We are not doing this at all. In fact Europe is spending a tenth of
this - a little more than a tenth on elementary particle physics, in spite of
CERN and all the efforts that are here. The United States, by the way, is also
not quite up to this but is quite near. That means that, in fact, we do not
yet need to be afraid of the point I mentioned before that all our children
will have to be elementary particle physicists; on the contrary we are even
rather low compared to estimates which seem to be rather reasonable. Therefore
I think we are not bound by any upper limit considerations of this kind. We
really should see what would be reasonable from the point of view of physics
itself. We do not need to say that we should be careful that we do not have
enough money for other sciences - this time will come, but it is not yet here.

If we look therefore at the development of physics in Europe we then
have to see not only from the purely philosophical point - i.e., we want to find
out the structure of the nucleon - but also from the point of view of demands
in respect of students, jobs, people and so on. Also from this point of view
it looks very clear that high energy physics needs an expansion, the number of
students in basic physics is increasing very strongly and these students are
also clamouring for work in high-energy physics. Now one would say that this is
a strange remark to make, because after all the students are not clamouring by
themselves; we are telling them they should clamour for high-energy physics,
we tell them that this is the most interesting part of physics, then it is no
surprise that they want to work in it. There is some truth in that, that students
are perhaps driven into high-energy physics rather than into other physics such
as nuclear structure physics, solid state physics, antidynamics, etc., in great
numbers. I think that this is true particularly in theory. If they only knew
better they would not, but under the influence of fashion, glamour and other
wrong reasons they do it, but this I think is a relatively small effect. Even
if one might say that only 60% of those students that want to work in high-
energy physics should actually do so, the problem would not be changed very
much. It is clear that the present facilities in Europe are not sufficient
and in a few years - we are after all planning for ten years hence - they are
going to be even more insufficient. So there will be a strong physicist demand.
Now I would like to say that if one looks at the physicist demand - the demand of
young people to have opportunities to work - the answer to this demand is by no
means the super high-energy machine - the machine we are discussing here - because
clearly relatively few people can work on such a machine. It is very important
to keep in mind here the concept of pyramid, that means that every high-energy
machine, not only requires but automatically fosters lower energy machines on
a national or even regional basis, and it is very simple to write down - I might
have made some mistakes of omission but the present European pyramid with the PS
on top and then next, well I will put down some machines that are almost finished, NIMROD, DESY, Liverpool, I mean the bigger machine the electron accelerator, then next line would be Saturne, Orsay, electron accelerator Frascati, electron accelerator Lund, and at still lower energy would be the SC, Liverpool, Oxford, Harwell, Uppsala, I have probably left out a few - particularly in this range - but this is only a sketch but you see already it has the right shape and that was all I wanted to say. Now if you then put here the new machine X, you see there is a lot of space here. I think this is a very important point in many respects, from the point of view of physics, of occupying students, from the point of view of the healthy development of intellectual life. So when we decide to build a new big machine in Europe there must be additional decisions done here or things will not work. From the financial point of view this can be expressed very simply - this is also a quotation from the paper of Amaldi - namely that roughly speaking - now this goes for the average of big and small countries - the nations should spend nationally roughly four times as much as they spend in their contribution to the international machine. Now, of course, smaller countries will spend less, bigger countries more, but of course the average is such that the bigger countries have to spend only very little more in order to get all-over a factor 4. At present smaller countries spend considerably less; they spend usually only a fraction of what they spent on PS - at CERN - which is not necessarily correct but if one takes the average the big countries count for more, so this factor 4 is the present situation, whether it is right or not is very hard to say, but it looks reasonable and it is to be expected that if one goes to higher machines the factor would be roughly the same, in some respects one could say that it should be lower because the higher machine is very expensive; on the other hand there are reasons for the other side, because the number of students increases exponentially; so I think that the factor should be kept the same, which means that the pyramid keeps a similar shape when it grows. I only want to mention this to be sure that any discussion on a new machine necessarily implies also a discussion about national or regional activity - in the case of smaller nations it will probably have to be regional and to show we cannot discuss these things independently.

Let me perhaps make one further remark. Europe of course is not an isolated unit, the plans in Europe must be at least influenced by what is going on in other places, in particular in the United States, and to some extent also in Russia. It is often said that the tempo of the growth here of high-energy development is dictated by the tempo of the fastest continent which in this case of course would be the United States. I think that this statement is only superficially true - it is like those arguments that we have to build PS in order to compete with the United States, which is only superficially true. But in fact I do not think really it is so. Imagine that the United States just suddenly disappeared from the universe, if no more consequences should be connected with this - the development of European physics still would have to go on. I know how unrealistic this whole thing is, but still physics would have to develop. What I want to say is that the reason for the development of this pyramid is really independent and even the tempo of the development I think is independent. The tempo of the development is fundamentally dictated by the
increase of the living standard and of the activities in Europe - and this in fact is even faster than in America at the present date. So I think to say that the tempo of development in Europe is dictated by the tempo in America is, I think, not correct. It has of course a strong influence and it reminds Europe of certain realities, the necessity of basic science, if one has enough money to eat, but fundamentally I think that this would be the wrong reason and I don't think it makes sense.

Now since science is not a national and also not a continental enterprise but really a human enterprise on a world scale, what I just said before does not mean at all that the development of European physics is considered independently from what is going on in America; on the contrary, we would like to coordinate the efforts. Therefore it is important that I say a few words about what is going on in America and Russia. Only it happens that at the present time the Russian development is relatively slow and not of such importance - that might change - the American development is more relevant for these discussions at this moment. Well what is the American development? The American development is this: At present in America there is no definite decision on any new machine except, as you know, the monster - the 40 GeV electron accelerator at Stanford which is just now in the process of being built. Apart from this the largest American machine will still be the AGS for some time to come; that means that they are roughly at the same height of the pyramid as we are. However, there are more definite plans for high-energy machines, namely that they are talking in America now very seriously about the 100 GeV machine, or something like that, in Berkeley, as the next step. I do not want here to go into the reasons why they are talking about it seriously and they probably will give a relatively large amount of money for a design group for this machine - decision has not yet really been taken. This in all probability will be the next high-energy machine, but, at the same time of course, there are other machines that cost roughly the same, namely, high intensity machines. There is the 15 GeV high intensity machine in Argonne, that is almost finished and which of course somewhere in this pyramid would be in fact somewhere here. It has less energy but supposedly a hundred times the intensity. (What is the price? It costs more than the PS.) Well I think this machine is somewhere here, well certainly somewhere higher than the PS. It might be the wrong machine to build, but it costs that much money. Now this is something which is important for us because it shows - for reasons which might be special for America - that America is now going in relatively small steps - from 30 to 100, in contrast to what we believed a couple of months ago: i.e., that the next American step would be a 300 GeV machine. Now this will certainly have an influence on European thinking, but need not have a decisive influence - on the contrary one can say that one should not always imitate - well in the case of the PS it was not really an imitation. One should not always build the same machine, but then there might also be reasons to build the same machine, in the case of the PS it turns out that having two machines of the same kind has been a very useful thing. The duplication of machines is not always a bad thing, but I do not want to prejudice the case here at all - I only want to say that, in contrast to our thinking 3 months ago, the tempo of the American development
seems to be slightly slower but I am sure that this is not the last machine the Americans are going to build. The building of the 100 GeV machine as the next machine will certainly mean however that a 300 or 500 GeV machine in America, if it is built at all, will be built later.

Now this is the situation in America. The situation in U.S.S.R. you know: the top of the U.S.S.R. pyramid is at present the 10 or 9 GeV synchrophasotron at Dubna and there is in construction a 70 GeV machine in Sepukkov - that is the name, I think - it is in the neighbourhood of Moscow. The breadth of the pyramid in U.S.S.R. is as we all know, somewhat narrower than in the U.S.

Well these are the facts on which we have to base our decisions and I would just like to say a few words on the way we have contemplated proceeding in our planning and discussion. We have, as you know, here at CERN already a study group for new plans for new machines. The study group has done several things and is in the process of getting more and more active. As far as machines are concerned the study group has investigated essentially two projects: one is the storage rings for CERN and we shall also have to have discussions about storage rings. Storage rings have one tremendous advantage: that they allow experiments at an energy in the centre of mass which is very much higher, although the number of these experiments is somewhat limited compared to an ordinary machine that would reach that energy; but on the other hand it allows one with relatively cheap means and very interesting means from the technical and instrumental point of view to peer into the future of high-energy physics. This ought to be, to my mind, an extremely attractive proposition. However, it is not to be considered as a full scale machine but it is certainly something that would fit into a pyramid of this kind. The second thing which the study group were studying was a really super high-energy machine and they have concentrated their studies on a machine of 300 GeV as a reasonable average number for a future development. As a third activity started only recently, this Group with the help of Dr. Burhop, who is a visitor with us now, began the study of the physics side of high-energy machines, instrumental and otherwise. Now this has been the activity at CERN, and we feel that it was high time, in fact I feel that it is somewhat even late now to broaden this discussion all over Europe, since the initiative of this new attempt should not come from CERN. It should come from Europe itself and this is why we have called this meeting today and to-morrow where we have invited a group of high-energy physicists, a relatively small but we hope influential group in Europe who, in order to discuss very seriously, not only the technical and physical problems concerning what machine we should build first, but also the political and financial and tactical problems of how to convince the governments of the necessity of building it. Now let me make two remarks. This discussion we thought would be best held in two ways: first by having public talks as we have now and we will have today and to-morrow morning and secondly by having discussions within this group: we would like, although it was a difficult decision, to keep those discussions on a smaller scale than this group that I am now facing, partly because we do not want CERN to be represented in such a tremendous majority, since it is necessary that the initiative of this should
not come from CERN but from the outside. This is why we have planned to have a closed session of our guests with only a few of us to-morrow afternoon and the second part of to-morrow morning, in order to get really informal discussions about what is to be done. We hope very much that out of these discussions there will be a continuing activity, probably a continuing committee which will take over all these questions and we hope very much that we can then establish a certain timetable of development in order to get this machine under way - not only the machine - the whole pyramid under way as fast as possible and as reasonably as possible. This is the task of this meeting and I think this is all I wanted to say."

AMALDI:

Actually, I am not at all prepared to comment on the very nice and general presentation that has been given by Weisskopf. The only part maybe that I shall comment on is the one which is not of a philosophical type, because commenting on philosophy is too difficult and maybe we are of different opinions. What I could do is to explain further some things about the figures that I gave one month ago in Frascati. In Frascati, there was held recently a meeting; of the so-called European Accelerator Study Group at which the main discussion was just: on the future of accelerators. For that occasion, I was asked to see if it was possible to build the machine about which we are talking. So I began to collect some data from colleagues throughout Europe. These data pointed to the conclusion that the plans that are now being discussed in Europe, are at least possible; they are possible from the financial point of view and also from the manpower point of view. They are possible on the basis of the present total national income of Europe according to the United Nations data. We find that CERN increases roughly its expenditure at a rate of 10 - 12% per year, plus a national effort in each one of the Member States corresponding to 4 times CERN; and this is quite enough to cover the base and the lower level of the pyramid that Weisskopf has designed on the board. If we include also ESRO - i.e. fundamental research in space - then we arrive at 35% of the figure of 0.2% of the gross national product which has been suggested as a desirable level of expenditure on basic research. The remaining 65% is still a very large amount of money and this should be certainly quite enough to cover the needs of mathematics, biology and other fields of physics. Only a small fraction of chemistry should be put in this 0.2% for basic research since most of chemistry has to do with applications. Therefore it should mostly go in the remaining 1.8% that is advocated as a reasonable figure for applied sciences. The same is true about engineering and perhaps also vehicles for space research should go in this 1.8%.
My conclusions was only that it is possible to envisage a new accelerator programme within these limits of expenditure on basic research. So we should not be too afraid if we feel that a new extremely large machine should be built for scientific or for human reasons or for any other reasons. I think that one can prove that within the framework of the economies of European countries it should be possible. This is essentially my point; I do not claim to have new reasons besides those given by Weisskopf in order to convince ourselves, first ourselves and then the others, that we should build this machine, but only that, if we are convinced, then it should be possible to build this machine.

With respect to manpower, the data I was able to collect are rather bad, because it is very difficult to get information which is homogenous and can be put together from all the European countries. However, in this respect also one can see that, at the present rate of development of all universities in Europe, it would be possible to build this very big machine without disturbing all the other activities that we should expect to develop in Europe.

Another short remark that I want to make about this point is that, all these figures that I have computed are based on the total national income of European countries, averaged in the years 1959-1961. My data refer to the total national income as published by the United Nations, averaged in the three years 1959-1961, and we know that the European countries should have a rather large development in this second part of the century; Therefore, since the total of this sum will build up and increase towards '70 or maybe later, the figure will represent an appreciably smaller proportion of the total. So this figure is actually very conservative.
The CHAIRMAN invited Professor Van Hove to give a paper on The Physics that could be done with Future High Energy Accelerators as it appears in January 1963.

PROF. VAN HOVE:

"Whenever there is a meeting like the present one on future accelerators, there is a theoretician on duty to speak about things which he does not know. I have done it several times before and I will do it again. Of course I cannot talk about the physics to be done with future accelerators because I cannot talk about the discoveries to be made in the future. Therefore Burhop in preparing the programme of this day invented a beautiful title: "the physics that could be done with future high energy accelerators as it appears in January 1963", the future as it is seen now. I guess the best I can do is the following: what would one be able to do with the future accelerators that can be planned if nothing unexpected would be found between now and the day they would come into operation. This is the kind of thing one usually talks about and I will try to do it once more.

It must be the fourth time that I have given a talk of this sort in the last two years, and, surprisingly enough, to give the talk now is much more interesting and pleasant than it was two years ago. It indeed has become easier to speak about a programme of work for future high energy accelerators under the assumption that nothing unexpected be discovered between now and the starting of their operation. The reason is that so much has been discovered recently. We can make a longer list of attractive experiments now than we could two years ago. Still, even if that were not true, I think we would have to hammer on the point that it is necessary to plan for future accelerators because of the very general reasons which Weisskopf already talked about and on which I think I also have to say a few words because they will be so important in the discussions with the non-specialists.

The two frontiers of Physics

Why is high energy physics bound to be important, whether or not we can at this moment make a long list of future experiments? Simply because it is one of the two great frontiers of physics, one of the two great frontiers on which the battle to get more understanding of the world of physics is being fought. There is the frontier of small distances and there is the frontier of large distances, and these are the two great frontiers of the science called physics. Down towards small distances the present frontier is around $10^{-4}$ cm, and up toward large distances we have reached orders of magnitude of $10^{27}$ cm. The small distance frontier is of course the field of high energy physics and is characterised by centre-of-mass momenta of the order of 2 GeV/c, as we have now with the present accelerators like AGS and PS. For the frontier of large distances I have taken as characteristic length the distance from the earth of some of the galaxies which are far away.
Actually at present, if I am well informed, one has observed a galaxy at a distance of $5 \times 10^{27}$ cm. This is a galaxy in Bootes, and the astronomers have given it the most romantic name of 3C295. Its velocity of recession is guessed at present to be half of the velocity of light, so one is really already very far.

In the region of distances much larger than $10^{-14}$ cm and much smaller than $10^{27}$ cm, we can say that the world of physics is pretty well understood. We have good laws, with proper mathematical formulation, and they have good predictive power. Once we are at the frontiers themselves, we understand a little, actually very little. We have only a glimpse at the order which is supposed to be present in physical phenomena in the two frontier regions. And when we go below $10^{-14}$ cm or above $10^{27}$ cm it is the absolute dark. We have no ways, either experimental or theoretical, of looking into the very dark region below the lower bound and above the upper bound. These bounds are the two frontiers, we are most anxious to explore the unknown territory around and beyond them, and I think that this desire is absolutely legitimate. The difficulty, however, is that exploration beyond the two frontiers is expensive. Toward small distances you need very high energy accelerators, the other frontier requires very powerful instruments of observation at large distances for e.g., observatories in outer space.

Let me now go over more specifically to my task of talking about high energy physics, since for a long time to come, maybe forever, the world of very small distances will be connected in an inseparable way with the world of high energies, through the uncertainty principle.

**The Unexpected Richness of Strong Interaction Physics**

The reason why I find it more pleasant to give this talk now than two years ago is the unexpected richness of strong interaction physics. Strong interaction physics has proved to be, in recent years, a field of enormous interest because so many unexpected features have been found. Let me list them roughly although of course they are very well known to you.

In the field of strong interaction physics the first development has been the existence of a great variety of particles. Many objects have been found that have more or less equal rights of being called strongly interacting particles. In the list established by Gregory at the High Energy Conference of last July we find

for the mesons: $\pi$, $\eta$, $\omega$, $K$, $K^*$

for the baryons: the nucleon $N$, 4 nucleon isobars $N^*$, and the following strange particles: $\Lambda$, $4Y_0^*$, $\Xi$, $Y_1^*$, $\Xi^*$

5867/p
A few more have come since, and you see right away how far we are from the timid beginnings of pion physics in the early 50's. Whether these objects are particles or not is completely irrelevant, they are states of matter with a certain amount of stability and hence of individuality, and therefore they are to be studied if we want to understand the laws of physics at small distances.

Not only are these many particles unexpected, but we see right away that they will be tremendously interesting and instructive. I remind you of the beautiful source of physical information provided by the K particles, especially the $K^0$, $\bar{K}^0$ system. Feynman once said - I recall this remark to you - that one of the very first examples one should discuss with students to teach them quantum physics is the $K^0$, $\bar{K}^0$ system. It is even more attractive than the hydrogen atom in some respects. I am sure that things of that sort will also appear with some of the more recent particles, for example the four mesons of strangeness zero, $\pi$, $\rho$, $\eta$, and $\omega$, with their beautiful symmetry of quantum numbers

\[
\begin{align*}
\pi &: J^P = 1^-; \quad \eta : J^P = 0^{++} \\
\rho &: J^P = 1^-; \quad \omega : J^P = 1^- \\
\end{align*}
\]

(the notation is $J^P$, $J =$ spin, $I =$ isospin, $P =$ parity, $G =$ G parity).

Their existence obviously puts in a completely new light the old Yukawa programme of the relation between mesons and nuclear forces.

Of course, it is very nice that we have already this list, but clearly we do not know why it is there. We actually still know very little about the simplest properties of these particles. It will be a long time before we know all their quantum numbers. However, even when we know that, we shall have a very long way to go before we understand the role they play in dynamics, in collisions. To put this general question in more concrete terms, we have to explore the role of all these particles in inelastic collisions. In inelastic collisions two years ago it was conventional to say that most secondaries are pions. Maybe nothing like that is really true, maybe the secondaries are of the whole variety found in the above list. We have to wonder about the problem of the true secondaries, the genuine, first generation secondaries in inelastic collisions. Only when we know about this will it be possible to study properly the dynamics of inelastic collisions.

The second big surprise that has been given us by nature in strongly interacting particles is the discovery of certain asymptotic energy variations for cross-sections at high energies, in the so-called multi-GeV region above some 5 GeV. I refer here to the shrinking of the diffraction peak, which
already has given rise to many experimental and theoretical considerations and developments. It is to be presumed that this whole field of asymptotic energy variations is going to be with us for a long time and, barring revolutions, it certainly will be one of the great sources of information.

This problem has also great practical significance because cross-sections have to be extrapolated otherwise than we thought previously. We of course do not know whether these extrapolations are correct, but at least it is no longer possible to say that the elastic cross-section becomes constant. It seems to decrease because of the shrinking diffraction peak and it is useful for orientation to make some extrapolations to energies which are contemplated for future accelerators. The actual shrinking of the diffraction peak is given in Appendix I which also contains some Coulomb cross-sections for comparison. I will only mention here the extrapolated decrease of the elastic cross-section. It seems to be proportional to something like 

$$\left(2 + \ln \frac{S}{2M^2}\right)^{-1}$$

where \( S \) is the square of the centre-of-mass energy and \( M \) the proton mass. This is equal to

$$\left(2 + \ln \left(1 + \frac{E_{\text{lab}}}{M}\right)\right)^{-1}$$

This extrapolation is a rough guess based on Regge polology and the CERN data on proton-proton scattering. One finds

<table>
<thead>
<tr>
<th>( E_{\text{lab}} ) in GeV</th>
<th>10</th>
<th>25</th>
<th>100</th>
<th>200</th>
<th>1250</th>
</tr>
</thead>
<tbody>
<tr>
<td>( 2 + \ln \left(1 + \frac{E_{\text{lab}}}{M}\right) )</td>
<td>4.4</td>
<td>5.3</td>
<td>6.6</td>
<td>7.3</td>
<td>9.2</td>
</tr>
</tbody>
</table>

The value 1250 GeV is the laboratory energy for an ordinary accelerator which would give the same centre-of-mass energy in proton-proton collisions as storage rings at 25 GeV. The elastic cross-section should decrease inversely proportionally to the numbers of the second line, at least if what we believe is correct, and this effect would be due to shrinking. The width of the diffraction would also decrease inversely proportionally to these numbers.

I give you these numbers mainly to show that it is a slow effect. Between 25 GeV and 100 GeV you go from 5.3 to 6.6. This is a very expensive way of getting an increase of 1.3, but such is life.

Still, these slow effects are very puzzling, they are likely to have a deep significance and their study will dominate the picture for some time to come. One might even guess that we shall still worry about them when the next generation of accelerators will be in operation, despite all the discoveries to be made in the meantime. For example, it seems to be true that the diffraction scattering with its slow shrinking is the shadow
of the inelastic collisions. What we do at present is just see that the shadow changes, but nobody in optics would study shadows without wondering how the absorption takes place. And also the elementary particle physicist will one day become ambitious and not only look at the shadow but look at the absorption, and therefore he will study again the inelastic collisions and he will try to see what is in the absorption process the feature that gives rise to the slow modification of the shadow scattering. This problem also brings us back to the many entries in the list of strongly interacting particles, because how can we study slow energy variations in the inelastic collisions if we do not know what are their true secondaries? Once again we are brought back into the same general class of problems.

Two years ago we thought that in the field of high energy collisions of strongly interacting particles there were at least two items which were somewhat banal on the basis of intuitive concepts and therefore perhaps not terribly exciting. They were the diffraction phenomenon and the so-called pionization phenomenon, i.e. the production of pions with small transverse momenta. We had the hope that at least two aspects of high energy collisions were of a nature that could be visualized intuitively in rather simple terms, this hope I think is gone. Pionization and diffraction are just as much of a puzzle as the rest of strong interaction physics. It is one more subject which experimentalists have to study extensively before it can be described and understood properly. The concepts we use now are just good enough — barely good enough — to allow us to give a posteriori a crude description of some of the experimental facts that are found. None of them has predictive power at all, they are probably quite inappropriate to do anything with them on a more accurate level. Hence this field which is so rich and so puzzling is probably to be a very good breeding ground for new concepts, for new ways of analyzing experiments and for new theories. To these new physical concepts I think we will come only through slow and painfully thorough experimental studies. What is needed for such studies is clear: much higher centres of mass energies — one has to explore collisions which produce many particles, probably also of higher masses, and one has to explore phenomena that vary slowly with energy — and very good intensities, because the effects of interest are not dramatic, they require refined and accurate work. One badly needs also, I am afraid, a lot of flexibility in experimentation, in particular the possibility to study as many types of collision as possible, p-p, n-p, π-p, K-p, p-p and many more if at all possible. This requires good intense secondary beams of high energy.

The Physics of leptons — Electromagnetic and weak interactions

I have to discuss now the leptons, i.e. the electrons, muons and neutrinos, as well as their interactions, which are the electromagnetic and weak interactions and which I put together for my purpose. This is a field where the two last years have not given any big surprises. The discovery of the second neutrino, fundamental as it is, I refuse to call a surprise
because everybody talked about it before it was discovered and some people believed in it. This is no criticism of weak interaction physics. On the contrary the absence of surprise shows that here our a priori understanding is somewhat better, a useful and realistic discussion is possible before experiments are done, our theoretical insight is of help in discussing what to do and what to expect. This is probably going to remain true, and in the field of leptons extrapolations over ten years are probably somewhat easier than in the field of strongly interacting particles.

If I make such an extrapolation for a possible future programme of lepton physics, point one will be a problem that has been continually with us in recent years; namely the search for deviations from present theory, from quantum electrodynamics and from the weak interaction of local Fermi type. Both types of study are going on now and will continue for quite a while. The extension of this type of work will carry us necessarily to increasing momentum transfers, especially in electrodynamics. You will do electron collisions, electron-electron and electron-proton, you will do muon-proton collisions at increasing energies, not only because it is simply fun to go to higher energies but because you want to explore higher momentum transfers. There we know from our present understanding that the cross-sections will decrease further and further, it is unavoidable that we shall be faced with the difficulty of measuring smaller and smaller cross-sections. In electromagnetic interactions the cross-sections decrease as the fourth power of the momentum transfer additional four factors coming in for strongly interacting particles.

The same general course, I think, is going to be followed with neutrino physics. Electron-nucleon scattering will be replaced by neutrino-nucleon collisions, and the main fight will be with small cross-sections. Neutrino physics, however, has again a surprise on the programme, we ask it to give us the surprise of creating an intermediate boson. I would not call this a surprise again but a fundamental discovery fitting well within the framework of our thinking. I expect that this type of programme will be with us next year, the year after, and probably still in 1968 or 1970, although in the meantime unexpected things may come in addition, perhaps related to the strange particles whose weak decays are so puzzling.

The appendices contain orders of magnitudes of cross-sections for electromagnetic and neutrino interactions. The Coulomb scattering data in Appendix I refers to proton-proton; the $\sigma_{\text{tot}}$ appearing in the formula is 40 mb. One obtains electron-proton and electron-electron data by dividing the proton-proton Coulomb cross-section by the second or fourth power of the proton form factor. Appendix II refers to neutrino induced reactions; cross-sections under 1) are estimated for a local interaction of Fermi type, the numbers under 2) deal with electromagnetic production of an intermediate boson. All figures given are very rough approximations.
The obvious search for deviations from accepted theoretical views may possibly lead to the answer to an outstanding question asked long ago: the puzzle of the two electrons, i.e. the puzzle of the difference between electron and muon. It is not clear whether the light on this puzzle of the two electrons and their two neutrinos is going to come necessarily from jumping right away to higher energy. Maybe measurement of the mass of the neutretto, the neutrino of the muon, will already throw some formidable light on this fundamental problem, and this is then an approach which is not specifically requiring high energies. However, it may also be that at very high energy a new type of information on the electron-muon puzzle will be obtained. But the trouble here with weak interactions is that the estimates about regions where really new effects would come in lead one to very, very high energies indeed, neutrino energies of the order of several hundred GeV which are not very attractive financially.

As a reminder I also mention that electromagnetic and weak interactions have a lot to do with the problems of strong interaction physics. They are remarkable probes for studying strongly interacting particles. They will remain among the most fertile tools for understanding the strong interactions, the main reasons being their weakness and our rather detailed knowledge of this structure. Obviously, whether you find an intermediate boson or not, you are going to measure, in the next generation of accelerators or possibly even before the nucleon form factors for weak interactions, and the results are going to be as rich a source of information on nucleons and mesons as the electromagnetic form factors have been in the last ten years. One should remember the crucial importance which the measurements of electromagnetic form factors have had in our thinking to realize what the role would be of the weak interaction form factors of the nucleons if they could be measured in the coming ten or twenty years.

**High precision work in energy regions already explored**

At this junction I have to say that especially in the field of weak interactions it is not only toward high energy that we want to go, it is also toward high intensity. A good example to illustrate this point is provided by the weak interaction form factors of the nucleon which I just mentioned. We expect from general considerations that the most interesting region of momentum transfers for the weak interaction form factors will be the same as for the electromagnetic form factors, namely the region going up to 1 or 2 GeV/c. This is a region for which neutrino energies have not necessarily to be very high, but one needs a tremendous intensity because cross-sections are small and one wants to do a great variety of experiments. More obvious examples are the rare decay modes of unstable particles, which will of course remain an important source of information on weak and electromagnetic interactions.

Here we come to the more general question of high intensity accelerators in the energy regions already explored, or of highly refined work in the energy regions which have already been studied at a lower level.
of refinement. One can ask a philosophical question namely: are very high intensities at moderate energies going to be as good a source of new knowledge as higher energies at conventional intensities? On this philosophical level, I guess that the answer should be "no" if one has to make a choice between one or the other. On the other hand, I think that it is wrong to ask the question in this light. We have rather to regard high intensities at moderate energies as a complementary field of study, and we should pay considerable attention to the necessity of giving it the right emphasis. Still, undoubtedly, the unexplored region of very high energies is a priori, I think, the most likely source of the important new discoveries, and it is my conviction that it should be given priority.

The long range view. General significance of high energy physics

Let me end by a few general remarks commencing with what I would like to call the long range view, or the general significance of high energy physics. I return once again to the question of how to explain and justify the role of high energy physics to the non-specialists and I would like to approach it by looking back a little in recent history.

With regard to the general significance of physics to the layman, I would distinguish the practical significance of a field of physics, and what I would call its philosophical or epistemological significance, being the significance for human knowledge in general. Let us discuss this for some fields which went in the past through great developments, and let us consider first electromagnetism, i.e. electric and magnetic phenomena. The practical significance is clear: our understanding of this branch of physics is very closely connected with our understanding and controlling of electricity and of electromagnetic radiation in all forms (radio waves, light, radar), and our practical applications are only so complete and diversified because our basic understanding is so good. As for the epistemological significance, the main contribution is relativity. The Maxwell equations probably did not excite the layman very much, but certainly relativity has been for philosophy and human knowledge in general a striking development, and no relativity is thinkable before you have electromagnetism. As you know relativity was born from the clash between Maxwell and Newton, and you needed both Newton and Maxwell before you could have Einstein.

Consider next atomic physics. The practical consequences of our understanding of atomic and molecular structure through quantum theory are obvious. It led to the detailed understanding and controlling of all the forces at the atomic and molecular levels, that is of all the forces that dominate chemistry, solid state physics, etc. And what are the epistemological consequences of the study of atoms, molecules and their interactions? They are no less than quantum theory, where I put particular stress on the statistical interpretation and the concept of complementarity, because they are striking examples of philosophical consequences of overwhelming significance.
We come now to the field we are interested in, and I think that I have to define this field very broadly, as including nuclear forces as well as elementary particle and high energy physics. Here of course the question of general significance is largely for the future to decide, although on the practical side nuclear forces, these forces which we believe to be indicated by mesons, have already found very spectacular applications. But regarding these applications of nuclear forces I would rather say that we are still in something like the state of chemistry before atomic and molecular forces were understood. Chemical forces, the energy of chemical reactions, were used extensively by humanity long before one understood the real structure and the conceptual world of atomic and molecular phenomena. In a certain sense it is true that we know nuclear forces pretty well; we know how nuclei stick together and we use them in all kinds of practical ways. But nobody at present will claim to understand nuclear forces. One can describe them by Mr. X or Y or Z's potential, which is not only ugly and complicated but almost completely misunderstood. We have also learned that the understanding will not come from low energy nuclear physics. It will only be reached when we look deeper towards higher energies and study elementary particles. The Yukawa programme of explaining nuclear forces in terms of particles is still not carried out, and this is no wonder, when we realize that it was tried for twenty-five years with one meson, whereas we just found three other ones with equal right to participate. I think we are going to have in our field something analogous to the understanding of chemical forces through atomic physics and quantum theory. We are going to have many applications of nuclear forces which will be more refined and better under control when we understand the true nature of these forces and thereby have for them, an accurate, reliable, rational description with full predictive power.

When this time comes we shall probably also have fundamental consequences on the epistemological level. I have asked myself whether for the layman interested in general questions of human knowledge there is already something coming out of particle physics which can interest and excite him. You can present him with a list: existence of antimatter, violation of parity and of the symmetry between matter and antimatter, the existence of many strongly interacting particles, the existence of fundamental variables which are outside of space and time (like baryon number, lepton number and isotopic spin), the hierarchy of interactions - strong, electromagnetic, weak - and the strangely parallel hierarchy of symmetry properties. All this we can list and show the layman; I am afraid that he will not be very excited, or that his excitement will only be due to extreme politeness. To our list we can add some questions which we feel exciting, like the place of gravitation in the hierarchy of interactions, or the possibility that our hierarchy of interactions does not maintain at high energies and that one has a merging. Even these questions will not interest the non-specialist too much.
I think it is much too early to start making a case for spectacular conceptual developments in high energy physics. We have discovered a forest of particles and interactions, we are now looking from a distance, very roughly, at what it contains, and it would be silly to make predictions on what will emerge on the basic level of general scientific concepts and human understanding. However, the very fact that we are so puzzled by the variety of unexpected findings, the very fact that so much shows up which fits so poorly within our framework of thinking, is in my opinion a sign of hope. It is a promise that new fundamental discoveries may very well be in store at the end of the long process of elucidation of the nuclear chemistry we are engaged in, and through them we may eventually come to a grasp of the basic forces in this field of physics. And then we may come to new epistemological consequences which may perhaps be of as high a significance as the two discoveries which I have mentioned: relativity and complementarity. Remember that both were made at the end of many many years of very thorough experimental work, at a detailed level where the layman could find little intellectual excitement. We are now at such a detailed level. Looking back at the past, the realization of this fact should give us an extremely strong motivation for a vigorous continuation and extension of our work. Thank you.
Appendix I. Rough extrapolation of shrinking diffraction peak

starting from CERN proton-proton scattering data using the "Regge pole" parameterization

\[ \ln \left[ \frac{d\sigma}{dt} \right] = \ln \left( \frac{16\pi}{\sigma_{tot}} \right) d\sigma \left( \frac{16\pi}{\sigma_{tot}} \right) d\sigma \left( \frac{16\pi}{\sigma_{tot}} \right) = \ln F(t) + 2 \left[ \alpha(t) - 1 \right] \ln \frac{s}{2M^2} \]

\[ \text{see : Phys. Rev. Letters 2, 111 (1962)} \]

\[ \Sigma_{tot} = \text{total cross-section} \]
\[ t = (\text{momentum transfer})^2 \]
\[ s = (\text{c.m. energy})^2 \]
\[ M = \text{proton mass} \]
\[ s/2M^2 = (E_{lab}/M) + 1 \]

Full lines refer to diffraction cross-section at constant \( t \).
Dotted lines refer to Coulomb scattering. They are plots at constant \( t \) of

\[ \frac{16\pi}{\sigma_{tot}^2} \frac{d\sigma_{Coulomb}}{dt} = \frac{16\pi}{\sigma_{tot}^2} \frac{\alpha^2}{t^2} \left| F_{1p}(t) \right|^2 \]

\( \sigma_{tot} \) same as above (i.e., total cross-section including strong interactions)
\( \alpha \) fine structure constant = 1/137.

\( F_{1p}(t) \) proton charge form factor taken or extrapolated from Hofstadter experiments, as follows

\[ t \text{ in (GeV/c)}^2 : \begin{array}{cccc}
0.5 & 1.1 & 1.45 & 2.7 \\
F_{1p}(t) : & 0.4 & 0.3 & 0.2 & 0.2 \\
\end{array} \]

For 25 GeV storage rings, \( s/2M^2 = 1250 \).

All extrapolations are completely unreliable.
Rough extrapolation of shrinking diffraction peak

\( t = 0.1 \)

\( t = 0.3 \)

\( t = 0.5 \)

\( t = 0.7 \)

\( t = 0.9 \)

\( t = 1.1 \)

\( t = 1.45 \)

\( t = 2.7 \)

\( t = 2.15 \)

\( t = 1.45 \)

\( t = 2.7 \)

\( t = 2 \)

\( t = 1 \)

\( t = 0.5 \)

\[ \frac{\delta}{\delta t} \left( \frac{16\pi}{\sigma_{tot}} \right) = 1 + \frac{E_{lab}}{M} \]

\( t \) is in \((\text{GeV}/c)^2\). All lines are straight.
Appendix II. Order of magnitude of cross-sections for neutrino reactions

1) Point interaction

Elastic process \( \nu + \text{nucleon} \rightarrow \text{lepton} + \text{nucleon} \)

\[ \sigma \sim 10^{-38} \text{ cm}^2 \] for \( E_\nu \gtrsim 1 \text{ GeV} \)

Inelastic process \( \nu + \text{nucleon} \)

\[ \sigma \sim (1\text{ to }3) \times 10^{-38} \text{ cm}^2 \] for \( 1 \text{ GeV} \lesssim E_\nu \lesssim 10 \text{ GeV} \)

perhaps increasing as \( \frac{E_\nu^4}{\nu} \) for \( E_\nu \gtrsim 10 \text{ GeV} \).


2) Production of intermediate boson \( W \)

See: Lee, Markstein and Yang, Phys. Rev. Letters 7, 429 (1961);

\( \nu + \text{Fe} \rightarrow W^+ + \mu^- + \text{Fe} \) (coherent process for gyromagnetic ratio \( 1+\gamma=\gamma=1 \)),

\[ Z = 26, \ 20 \text{ GeV} \lesssim E_\nu \lesssim 100 \text{ GeV} \]

\( E_\nu \) : lab. energy of neutrino

\( m_W = \) mass of \( W \)

\( m_p = \) mass of proton

\[ \sigma \text{ in } 10^{-38} \text{ cm}^2 \]

\[ E_\nu \text{ in GeV} \]
CHAIRMAN:

If this talk was repeated, it would certainly further the development of nuclear physics in our part of the world.

BERNARDINI:

There is a point in Van Hove's talk in which I am personally very interested. Speaking about what I consider one of the main puzzles of natural philosophy today, i.e., the reason why the muon and the electron have different masses, it is not at all granted that this kind of problem will be solved if one increases the energy. For instance it is obvious that at present one of the major problems is to establish the upper limit of mass of the $\nu_\mu$ (neutretto). This does not look at present a high energy, but it may be a high intensity problem.

One could say the same for some developments of weak interactions, particularly if you consider the non-leptonic decays and the decays of the strange particles.

Then, making a very personal comment on the numbers written by Van Hove, i.e., $10^{-14} \leq 10^{27}$ cm. (the highest and the smallest distances), I feel these numbers are strongly connected with the anthropological fact that for centuries we knew only one space, the Lorentz space. Now, since the discovery of the internal degrees of freedom, the horrible links between the Lorentz space and the other spaces, which look to me physical spaces as well as the Lorentz space, have not so far been obviously found. If they exist, will they be discovered by merely playing on the energy, in other words on high-momentum transfers?

In this connection, one may remember that when people were fighting for the interpretation of the Zeeman effect, (the starting point for the discovery of the first degree of freedom), there was a continuous argument whether it was better to have a very high magnetic field of say 20-100 kGauss or a very good intense source and a very good interferometer.

BERTHELOT:

I would like to ask Van Hove if he could give some figure of what he called a high intensity machine. What would be the gain relative to the machines which are used now?
VAN HOVE:

I had not in mind any particular figure that I would wish for. I was actually referring to what is usually considered. Normal intensity I referred to as being the $10^{11}$ per second order of magnitude of our conventional machines - and then by high intensity I meant a couple of orders of magnitude higher. This, however, is not based on any clever or unclesven calculation; it is just the conventional language that has been adopted of saying $10^{11}$ or something up to $10^{12}$ is by now rather conventional, and if you go to $10^{14}$ you are in a new order of magnitude of intensities. This is what I was referring to. Does that answer your question? There is no theory behind this.

HINE:

Taking Van Hove back to the deepest part of his talk and to the first two numbers he wrote on the blackboard ($10^{-14}$ and $10^{27}$), would he take their ratio and comment on it? I mean $10^{41}$ is appreciably larger than $10^{39}$ which is the magic gravitation to electric force figure. Ought we to be looking for something?

VAN HOVE:

No comments. I do not want to go in for numerology.

WEISSKOPF:

I should like to add that problems of this kind, which are today magic and numerology, might become physics one day, owing to new discoveries. Do you agree?

VAN HOVE:

I entirely agree.

SALVINI:

Talking about the alternative between high energy and high intensity, among the numbers attached to the elementary particles there are two which are just two scalars, the mass and the magnetic moment. The precision measurements of these two are related more to high intensity than to high energy. Have you any feelings about future prospects of measuring these quantities at high levels of accuracy?

VAN HOVE:

With the exception of the leptons, I see no immediate significance in high accuracy measurements of these numbers. The exception is the conventional type of work on the magnetic moment of leptons, including possibly the magnetic
moment of the neutrino. Also of course the lepton masses, in particular the mass of the neutrinos. For the rest, and particularly for strongly interacting particles, barring unexpected discoveries, this sort of information is not going to be useful because we have no way of using it conceptually. It is very good to know the mass and the magnetic moment of strongly interacting particles with a fair precision, but it would be a luxury to push the accuracy to 5 more decimals than we need in our discussion of strong interactions. This would be true even if you should take the very optimistic view that some theory was discovered before the exploration of all the facts. To take a concrete example: if the world equation of Heisenberg should be found to be correct, also there, also in that most optimistic case, you will not be helped in any way by precision measurements, because it is already clear that in attempts of that sort precision is of no use because the mathematics is not there to extract precise answers from the equations.

**SALVINI:**

The magnetic moment of most of the hyperons is not known.

**VAN HOVE:**

I think the measurement of these quantities within the precision which we need to calculate the behaviour of strongly interacting particles is needed. For example, you need obviously the mass of the pion well enough to calculate your phase space factors with the accuracy required for discussing your data. However, to go beyond that by a factor 100 in precision is, I think, a luxury. The same for the magnetic moment. You need it in order to predict the behaviour into magnetic fields, but to go much higher than the precision required for these rather down-to-earth uses is, I think, a luxury at present. I do not mean at all that we should stop measuring these things. But I do not quite see why we should go to an accuracy that is for example comparable to the accuracy of work on electrons and muons. I just do not see what you would do with the results. I hope this will change, however.

**SIMON:**

I think it is a mistake to believe that the purpose of high-intensity machines is to improve the accuracy of measurements. One can certainly use this high intensity to increase the precision measurements of masses. Apart from neutrino physics where high intensity is clearly needed, the usefulness of high intensity for strong interaction physics is to look for rare interactions. If one can assume that in ten years' time experiments may be done with a resolution approaching a millimicrosecond, I would guess that the intensity required to pick out rare interactions would be something like $10^{14}$ particle/sec.
VAN HOVE:

Thank you. These are of course things I am not competent to discuss and I was all the more interested in hearing your remark. There are indeed many things, actually most of lepton physics including electrodynamics, where in my mind the idea of accuracy was certainly related to the desire to measure small cross-sections. For example, in plain elastic electron-proton scattering, the desire to go to higher momentum transfers leads us to the necessity to measure small cross-sections. This, I thought of taking also under the category of high precision work, but of course it is rather to be described as rare events. However, for the rest, that you could not reach higher accuracy when you have more events, that means more particles, this cannot be absolutely true after all. Better statistics is often very careful to reduce the errors.

SALVINI:

When you talk of masses you should include also the width of the mass for some of the strongly interacting particles.

VAN HOVE:

Yes.

The meeting rose at 12.40 p.m.
The CHAIRMAN invited Dr. Johnsen to give a paper on Future High-Energy Accelerators and what they will look like.

Dr. JOHNSEN:

"1. Introduction

Before we look into the future it is reasonable to remind ourselves of the situation at present. A list of all present-day accelerators operating or under construction above 200 MeV is distributed separately. Recently it has become rather fashionable to talk about the pyramid of accelerators. I therefore decided to use this as the basis for my presentation of the situation today, and from the data presented in the distributed list I have constructed the pyramids shown in Fig. 1.

The accelerators have been grouped into about half orders of magnitude in energy, and counted up. For the sake of simplicity, I have made no distinction between electron and proton accelerators and no distinction between high and low intensity. I have, however, made a further sub-division of the projects: (i) those working at present to an energy justifying their placing in the group (double shading), (ii) those under construction but at present working to an energy for the group below (single shading), (iii) Projects agreed upon some of which are in an advanced stage of construction and some plans just accepted by the appropriate authorities.

On the diagram showing the world situation I must make the reservation that I may have slipped a project or two, since it is difficult to obtain information from certain parts of the world, especially about newly accepted projects of a small or medium size. The diagrams for the CERN Member States and for U.S.A. I believe are complete.

The diagrams are self-explanatory, but a few comments may be appropriate. Firstly, the world as a whole seems to make a beautiful pyramid, both in machines working at present and due to work in the immediate future. When we look at Western Europe or U.S.A. the pyramids are not quite so nice. On the other hand there is no a priori reason why a pyramid is the right shape.
We have made a habit of comparing the situation in this part of the world with the one on the other side of the Atlantic. I do not believe that such a comparison should be made in order to support the contention that, if something exists somewhere else we, too, should make it. However, such a comparison is nevertheless useful as a basis for an analysis of a situation.

If we look into the future it is seen that there is strikingly little difference in the W. European effort and the American one. The situation is, however, different if we look at the present situation of accessible machines, and even more so if we go some years back. Many of the American machines are already old ones, in fact all the way up to the last but one energy range listed, and the base of the pyramid is very impressive. This diagram is probably sufficient to explain why the U.S.A. has had a certain lead in elementary particle physics in the last decade.

The future, however, looks brighter for Western Europe, although there are also here significant differences. For instance, Europe seems at present to concentrate the effort in the 3-10 GeV range. U.S.A. is putting most of its effort one step higher.

Let me then scan fairly quickly through the types of future accelerators that may fall in the various groups if they are constructed. There is no doubt that even machines in the lowest energy ranges will be significantly different from existing machines. This applies in particular to the required performance of the machines, and therefore also to the magnitude of the projects, measured for instance, in terms of their estimated cost.

2. 200-1000 MeV:

It has become common to talk about pion factories. This notation does not have a clear cut definition, but means in general an accelerator in the range 300-1000 MeV with maximum emphasis put on intensity.

There are mainly two types of pion factories under consideration. One would be a sector focused cyclotron run c.w. and with an intensity of the order of 100 μA protons in an extracted beam. The maximum energy one is talking about for such a machine is about 900 MeV, with extraction at about 810 MeV. Smaller projects under consideration are in the 400-500 MeV range.

The high duty cycle has been listed as the main relative advantage of such a machine over other types. There has, however, been a tendency to over-state this. Taking the RF structure into account the duty-cycle is limited to about 10-15%.
There are two important disadvantages, which in fact are related. One is the difficulty of beam extraction, and it is still not clear how a large fraction of the beam really can be extracted. It further seems that if one can extract the beam efficiently at all, it will be at one energy only, namely at one of the resonances near 400 MeV and near 800 MeV. The machine has therefore very little flexibility in energy.

The other disadvantage is the contamination problem which is serious even with present day cyclotrons. In fact one will have to find extraction methods that leave little more protons in the machine than in ordinary cyclotrons.

To form an idea of the magnitude of a project of this nature it is estimated that the machine proper would cost 40-60 MSF according to energy and that one would have to multiply this by 1.5-2 in order to take into account the laboratories and facilities round the machine.

The other type of pion factory under consideration is a proton linac. In the energy range that can also be covered by cyclotrons the linac would have the advantages of easy extraction with easier contamination problems, higher average current in the μA range and more flexibility in output energy. The main disadvantage is its low duty cycle, which with present day techniques cannot be brought to a much higher value than a few per cent.

Recent cost estimates are in the same range as for spiral ridge cyclotrons with the same energy, i.e. about 120 MSF facilities included, for an 800 MeV machine. It is, however, still believed that the linac is the more expensive of the two, but this is marginal.

3. 1-3 GeV

There has been a tendency to ask for higher and higher energy for pion factories partly to get access to the study of many of the isobars and partly because one would in fact also like to produce strange particles. This brings us up in the next energy range of my diagram.

Cyclotrons then become too clumsy and expensive and one would expect the linac to require at least very close attention in this range. Although it has so far been difficult to justify linear proton accelerators in this energy range from a physics point of view, as circular machines have been so much cheaper and have done approximately what has been required, I still nevertheless feel it is a pity that no proton linac has been built above 70 MeV. This type of machine has potential possibilities especially concerning intensity both for pion factories and kaon factories.
A new possibility would be a super-conducting linac. This is in its very preliminary stage of investigation at present in various laboratories, in particular at the Rutherford Laboratory. It deserves close attention, although it is not for the immediate future. It is inherently a c.w. machine. However, it will still have an R.F. structure, and will therefore look like a 100% duty cycle machine only as long as the resolution time of detection equipment is long compared with the R.F. structure. Its duty cycle may therefore not necessarily be so good for all purposes. One could add a storage ring for smoothing, but this would then spoil the advantage of very easy extraction.

4. 3-10 GeV

It is somewhat difficult to guess at what energy synchrotrons start becoming interesting for future projects. My guess is that no more synchrotrons will be built for less energy than 3 GeV. Above this energy there will be mainly two considerations for new synchrotrons.

AG synchrotrons have proven rather compact and reliable machines. I therefore believe that rather conventional AG synchrotrons will come strongly into consideration as national or regional machines mainly serving the strong university needs for backing up the facilities that are more difficult to get access to. For such machines simplicity, reliability and low cost will be of the utmost importance. Taking into account the experience on already existing AG machines I believe that such machines could be built for 40-60 MSF according to energy in the range 6-10 GeV. To this figure would have to be added associated laboratories and facilities.

Such machines would have few new features, although one would put rather more emphasis on long straight section arrangements, good ejection schemes and the like.

However, both in this energy range and the range above it one would expect a considerable demand for high intensities. Nimrod and the Argonne ZGS are planned to meet this demand in the near future.

These are probably the last weak focusing machines to be built. High rep.rate AG synchrotrons with relatively large apertures and perhaps also relatively large average circumference will certainly be in the race for high intensity. I see no reason why such machines should not produce more than \(10^{13}\) p/s, whereas it may be difficult to reach \(10^{14}\).

Intensity, however, costs money, just as much as energy. The extra cost comes from the need for a higher-energy linac for injection, say 200 MeV, from the high rep. rate, requiring more elaborate power supply, RF system, magnet design and vacuum chamber and from the problem connected with induced radioactivity.

One disadvantage of the high intensity synchrotron is its low duty cycle, probably in the per cent region. This could be remedied by adding a beam stretching storage ring.
There is another solution under serious discussion in the U.S.A., and that is the proposal from MURA of a 10 GeV FFAG machine. The MURA group promises $2 \times 10^{14}$ p/s and hopes for $10^{15}$ p/s. It is somewhat difficult to see why this machine should give very much more than an ordinary AG synchrotron with large aperture and high rep. rate. However, the FFAG gives 100% duty cycle, which could only be obtained for the AG synchrotron by adding a beam stretching storage ring. I believe that the choice between the two types of high intensity machines is more a matter of economy than of performance. To get an indication of the cost of high intensity it can be mentioned that the MURA proposal estimates 500 MSP for 10 GeV, $2 \times 10^{14}$ p/s, experimental facilities included.

5. 10-100 GeV

When we come to the next two energy groups the same trend as for the previous one will hold for proton machines. Some laboratories in the biggest countries will want the cheapest and most conventional machine in their back­yard, some will go for high and expensive intensity, and in the latter case AG synchrotrons and FFAG will compete until the situation has cleared up a little.

For electrons the situation is somewhat different since the highest present energy is below the energy range we are now considering. The Stanford M. project aims at remedying this by an electron linac aiming at 20 GeV with possible extension to 40 GeV by adding R.F. It is, however, not at all certain that the limit for similar electron machines is below 10 GeV as thought a few years ago. Recent studies seem to indicate that electron synchrotrons may take up the competition with electron linacs in the range 10-50 GeV. The development of electron machines, however, I believe will depend rather much on the results of physics research on the Cambridge machine, the DESY machine and the Stanford Monster.

6. The Top of the Pyramid

We have so far mainly looked at machines that might be considered for widening the bulk of the pyramid, and we now come to considerations about the top. This is, of course, of special concern to the people working at CERN with the responsibility of making sure that this Organization fulfils its promises to the physicists in the Member States. These promises were mainly to create desirable facilities that are of such magnitude that they are outside the scope of individual Member States but still within the financial possibilities of Europe.

As all of you know, CERN is conducting a special study of two projects that it would be natural to start on a European basis, preferably in parallel. One is a set of storage rings for the PS. This I shall return to later in my talk. The other one is a large proton synchrotron, and I shall give a few data illustrating what is involved in this. I should, however, remind you that we are not the only ones putting thought into the future top of the machine pyramid.
Similar projects are also being actively studied at several places in the U.S.A., and some thought in this field goes on in U.S.S.R., although they have the advantage of constructing at present the biggest accelerator in the world. Whether one likes the word competition or not, our study is also a study of what is needed in Europe in order to give European physics access to facilities that are as interesting and as exciting as anything one can find elsewhere.

We have for our study chosen the energy of 300 GeV. However, we can fairly easily scale down and have in fact already put some thought into 150 GeV. We could also scale up, but this seems less likely to become practical politics.

A 300 GeV machine would, in most respects, look like a scaled up version of the PS. It is mainly in radius, however, that it is scaled up, to about 1200 m average radius. The cross section of the magnet would be smaller than on the PS. The machine would be in a tunnel that it would be desirable, from a shielding point of view, to have underground. There would probably be a service tunnel above the magnet tunnel.

Injection would be at 3-6 GeV either from a linac or from another smaller synchrotron. We are at present slightly in favour of the latter solution.

Concerning performance of such a machine we believe that it would be able to accelerate about $10^{13}$ protons once every 3.3 seconds with about 0.75 flat top. The duty cycle would therefore be fairly favourable. The repetition rate could be increased at lower than maximum energy, but not very much with synchrotron injection. In this respect a linac injector would be better.

Considerable thought is already going into making the machine as easy to use for physics experiments as possible. There will be 6 or 12 long straight sections, each about 50 m long (Fig. 2) with about 35 m free between the matching quadrupoles. However, these quadrupoles will not really obstruct the beam, as we believe they can be made open in the median plane (Fig. 3). An efficient slow ejection scheme should be built into the machine from the beginning making it possible to work with external targets whenever there is no special reason for internal targets. From machine contamination point of view it would be an advantage in fact if we could persuade the physicists to work exclusively with external targets.

We have not got far yet in our considerations of experimental areas. However, we are led to believe that we shall again need lengths of 1-2 machine radii. We are therefore thinking in terms of experimental tunnels rather than halls, perhaps branching out as fingers from a common area around the long straight section.

The cost estimate for such a machine is about 1100 MSF for the machine proper with its buildings and staff expenses included. In addition to this we have estimated that other activities around this new CERN to build up the desired facilities will amount to another 500 MSF by the time the machine is finished. This last figure is very uncertain and some people claim that we
underestimate this. We should not forget, however, that this type of activity is by the end of the construction period in full expansion, and we have not tried to estimate any further ahead.

It is uncertain what to include in the estimated time scale. I believe that the machine can be completed in 9 years from now if we get the project accepted sometime in 1964. In other words about 7 years construction time plus 2 years of preparation on both the technical and political level.

I have given figures only for 300 GeV. The main figures like machine diameter and cost would drop to about half if the energy was dropped to 150 GeV, with 1-2 years knocked off the construction time.

7. Colliding Beam Devices

So far I have only considered rather ordinary types of accelerators. There is a different class of facilities, however, that have been brought rather heavily into the discussion recently, namely colliding beam devices. These were first proposed by the MURA group in connection with their proposals for two-way FFAG. Later there has been a tendency to go more in the direction of special storage rings fed from a linear accelerator or a synchrotron.

Several such projects for electrons are in the construction stage or the design stage. The first one that was started was the so-called Princeton/Stanford storage rings for 500 MeV electrons, connected with the Stanford Linac. Both rings will be filled with electrons. This project is now in its running-in stage. They have stacked electrons, but not enough for experiments.

At Frascati an electron-positron storage ring has been constructed for 220 MeV, the so-called ADA. This was first put into operation at Frascati where they injected from their synchrotron. By this type of injection they could not reach high enough intensity to do experiments, and they have moved the ring to Orsay where they are going to inject from the linear accelerator there. This set-up is now also in its running-in stage and they have in fact had a few hours of experimenting.

In addition to this first experimental ring, they have at Frascati started on a bigger project, namely the so-called Adone, which is again an electron-positron ring of maximum energy of 1500 MeV. They are in the process of placing the order for a special linac for this ring. The linac energy will be about 400 MeV.

At Orsay a 1.3 GeV storage ring is planned to go with their linac. However, as far as I know they are first going to build a ring for about a third of this energy to gain experience both in machine design and experimentation.
These devices that I have mentioned should in fact have been included in the pyramid, if I had only known how to include them. Their injectors, however, were included. I think that naturally they belong to the base of the pyramid, and this shows how versatile this base has become and how one with relatively small means has taken up rather new unique methods of research. In addition to the projects already mentioned there are also proposals for similar facilities in other places, like Würzburg in Germany.

The idea of the much bigger project of adding a set of storage rings to the CERN-PS goes back to 1960, when it was firmly established that the beam intensity one would ultimately be able to reach with that machine was considerably higher than the rather conservative estimates and promises given earlier. Preliminary estimates of possible reaction rates obtainable with two such rings were so promising that it was felt to be worthwhile to study further, with the result that such a set of storage rings is the other project our special study group for future projects has on its programme. We do not believe that such a device would be an alternative to a big accelerator, but rather a natural extension of any big accelerator, in particular the CPS, which should be considered in parallel with, or rather independently of a big machine.

The main purpose of a set of storage rings would be to probe into the very high energy range by looking at p.p collisions at 50 GeV centre of mass energy. This is equivalent to about 1300 GeV on a stationary target, and is therefore far in the future for other accelerators. There are also other possibilities given by such storage rings, e.g., also e-e collisions at 8-10 GeV c.m.s. and possibly other particles and it would also give extension of the possibility of ordinary 25 GeV physics.

Having given the main advantages I should also give the main disadvantages. These are the facts that the types of particle collisions one can study are very limited and the interaction rates are rather low. These are the main reasons why it is difficult to see that storage rings can replace more conventional accelerators, but they can act as interesting probes into otherwise inaccessible regions.

The first idea was to arrange the rings as shown in Fig. 4 with two nearly tangential rings each of the size of the PS. The advantages of a concentric device were stressed strongly by O'Neill and further studies of this possibility have led us to conclude that this is a preferable solution, although it entails a somewhat larger diameter. (Fig. 5). Some of the main parameters are:

\[ R_m = 135 \text{ m} = 1.35 \text{ PS} \]
\[ \text{No. intersections} = 8 \]
\[ \text{Crossing angle} = 15^\circ \]
\[ L_{Hgs} = 9 \text{ m} \]
Aperture = 14 cm x 4 cm
Vacuum \simeq 10^{-10} \text{ mm Hg}

Fig. 6 shows about half a superperiod with injection and interaction region and Fig. 7 shows in some more detail the interaction region but without detection equipment.

Coming to performance the stacking of PS pulses would build up a beam 1 cm high and 6-7 cm wide radially. This radial width can be reduced locally in the interaction region to \sim 2 cm. About 700 pulses from the CPS could be accommodated within this spread, corresponding to about 3-4 \times 10^{14} circulating protons (20 A circulating current) with present PS output of 5-6 \times 10^{11} per pulse.

Two such proton beams in collision produce an interaction rate of
\[ \text{I.R.} \sim 5 \times 10^{30} \sigma \text{ per sec.} \]
where \sigma is the cross section in cm$^2$. If we put in the total cross section of 40 m barn we get
\[ \text{I.R.} \sim 2 \times 10^5 \text{ S}^{-1} \]

These are rather conservative estimates as they are based on present day performance of the CPS and there is every reason to believe that this will be improved, especially with respect to the special requirements of the storage rings.

For more details I refer to reports that we have written on the subject.

Again the size of the project can be illustrated by the cost estimate. One would guess that this would be about 2 PS. In fact it is somewhat less because of more duplication of certain things and because other things like the linac injector are not there. We believe that this set of storage rings would cost 250 MSF (present day value). Construction time would be about 5 years, which probably would mean earliest 6-7 years from now taking into account the minimum time it would take to get agreement on such a project.

8. Conclusion

I would like to conclude my talk by a few general remarks. When we look at the very large projects these do not seem to be limited by the accelerator technology at present. The AG principle can be exploited considerably beyond the CPS and AGS and probably beyond what today can be justified on the ground of physics and economy. Personally, I believe that synchrotrons above 100 GeV will be built in the foreseeable future. The question
is mainly how many and how they should be distributed both geographically and in energy, and our immediate concern is what role Western Europe should play. We took a big step with the CERN 10 years ago. It would be a pity if we fall behind again.

On storage rings the situation is different. This is a new experimental technique with some known and probably many unknown possibilities, but also with some definite and known short-comings that make some hesitant to advocate this step. It is doubtful that we could make a case for several big storage ring projects, but the question is whether we should not take the risk now, as a success would certainly open up interesting possibilities for future machines as well.

I would in this connection like to show a graph partly reproduced from Livingston and Blewett's accelerator book, but extended by Cocconi, Jones and O'Neill to include some projects talked of with time scales. As pointed out by L. & B., the tendency has so far been for the envelope of the acc. curve to follow a straight line on a logarithmic scale. Each time the curve has had a tendency to flatten off, a new type of device has been brought into the picture. As seen from the graph, the storage rings might do the same now.

My conclusion would be that the accelerator technology can now give physics versatile and general purpose machines for general high energy physics experiments of any energy up to some hundreds of GeV, and the more risky but in some respects exciting probing machines like storage rings; and the question is whether there is not really a good case for both."

CHAIRMAN

We are grateful to Johnsen for his paper. The last part of it is most interesting to all physicists, particularly the question of the desirability of having both a large accelerator and storage rings.

ADAMS

What ratio do you take for beam-beam interactions as opposed to beam-gas interactions for the pressure of $10^{-10}$ torr you indicate? Is that the pressure in the interaction region?

JOHNSEN

It is the pressure we consider desirable in and near that region.

DE RAAD

With $10^{-9}$ torr in the interaction region, one gets roughly the same number of beam-beam and beam-gas interactions. However, looking at the data concerning some possible experiments, it would clearly be desirable to have
far less beam-gas interactions than beam-beam interactions. We think with cryogenic pumping it would be quite possible to obtain $10^{-10}$ torr in the interaction region.

JOHNSEN

We have actually obtained $10^{-10}$ torr in a fair size vacuum system in the Laboratory. We think it a reasonable extrapolation to assume that this figure will be obtainable on a technical scale within the time schedule generally talked about for such a project.

CHAIRMAN

This is a similar figure to that obtained with A.d.A, where at $10^{-10}$ torr the electrons are reduced to $\frac{1}{3}$ of their extremity in 100 hours.

ADAMS

There are two problems in this connection. (1) The vacuum required for the particles to survive during stacking, which is not a very severe limitation; (2) the ratio beam-beam to beam-gas interactions, which is a much more severe limitation.

BERTHELOT

Can you tell us about the possible influence of cryogenics on the development of future accelerators?

JOHNSEN

This is a very difficult thing to comment on, because it depends on whether one is optimistic or pessimistic. My private view is that, before we start building a big machine based on superconducting or cryogenically cooled magnets, we ought to see this technique used on smaller apparatus in the Laboratory. So far, it has not been used much, as far as I know. Therefore, I feel that cryogenics is more likely to be used not for the next generation of accelerators but for the following one. The other question is how useful would cryogenic techniques really be to a machine. They would obviously be useful for some purposes.

ADAMS

Some of the advantages of cryogenics may be illusory. You really want to design a big machine in such a way that it is very flexible for experiments and therefore you must have a lot of straight sections. It may be a step in the wrong direction to try to build a smaller machine, which superconducting magnets might make possible. From the point of view of flexibility for experiments it might be advantageous to make machines larger rather than smaller. Of course, the economic argument is not resolved yet.
After the many efforts to try to get people more interested in storage rings, it now seems that the issue in the last few months has become clearer. From the point of view of physics, it would be a great pity if Europe did not continue its progress by extrapolating from 30 GeV to 100-200 GeV and if it missed the opportunity of looking into nature with a very high energy in the centre of mass system, which the storage rings offer relatively economically. The two projects are not competitive but complementary.

CASSELLS

I agree with Adams' last remark. We can see the importance of the storage rings if we think of them as playing the part played by cosmic-ray physics up to a few years ago in showing the way ahead.

Would there be any saving involved in producing, say, 6 medium energy AG synchrotrons in one batch for regional distribution?

JOHNSON

I have not thought in terms of mass producing synchrotrons of a few GeV. Perhaps one should. It should be remembered in this connection that buildings account for about 50% of the cost of a machine. More money could be saved by rationalizing building than by cutting the magnet aperture. Batch production might be worth considering from the point of view of the saving achieved in development work and manpower.

WEISSKOFF

It is most questionable from the point of view of physics in Europe whether it is desirable to mass produce any kind of machine in the lower levels of the pyramid. To my mind the pyramid should be thought of as three-dimensional. It is therefore much better to diversify than to produce say six "Saturnes", which would be tantamount to having a two-dimensional pyramid.
Present accelerator situation

CERN Member states

USA

No. of Accelerators

WORLD

No. of Accelerators

Fig. 1
FIGURE 2  CONFIGURATION OF LONG STRAIGHT SECTION. TRANSVERSE LENGTHS ARE EXAGGERATED COMPARED WITH LENGTHS ALONG THE BEAM.
Set of storage rings with synchrotron

A  Tangential rings
B  Nearly tangential rings

Fig. 4
Fig. 8
The Chairman invited Professor Cocconi to give his paper on Experimental Design, Particle Detection, and Measurements at very high Energies.

Professor COCCONI:

"This is a review of what I know about experimenting with the machines which will presumably be built in the not too distant future; typically, a "150 GeV" proton synchrotron, and a colliding proton beam set-up, consisting of two "25 GeV" storage rings fed by an already existing accelerator. Most of what I am going to say is not new; in fact I have taken advantage of the reports from Brookhaven, Berkeley, and CERN, and of the discussions directed here at CERN by Burhop during the last month, with this meeting in view. Nevertheless, I am aware of the fact that several of my statements will look completely off the mark a few years from now.

1) I will start with Figure 1a, showing for a 150 GeV proton interacting with a stationary target, the maximum momentum that a secondary \( \pi \) meson can have, as a function of lab angle of emission. In reality, \( \pi \) mesons, as well as other kinds of secondaries, will rarely be observed with transverse momenta larger than 1 GeV/c, hence the useful region is the narrow ± 1 GeV/c strip around the zero line. The fact that the average transverse momentum is \( \sim 1/2 \) GeV/c, independent of primary or secondary energies, is sometimes considered as unfortunate, because it limits the high-energy secondaries to small solid angles. However, it is also of help when focusing the secondary beams by means of magnetic lenses. Consider, e.g., the magnetic horn which now concentrates \( \pi \) mesons for the neutrino experiment in CERN. The same horn will focus with the same efficiency the \( \pi \) mesons of correspondingly higher energy emitted by a more energetic accelerator, provided the longitudinal distances are increased in proportion to the energy increase. The same is true for \( \mu \) meson channels, separator channels, etc. In other words, a good many of the magnets and of the lenses now in use will still be useful with a much higher energy machine, provided the length of the experimental areas is increased roughly in proportion to the maximum energy.
The enlargement around the origin, Figure 1b, indicates with a dotted line the separation between the pions emitted forward in the c.m. system and those emitted backwards. (The situation does not change very much for other particles). It is important to note that most of the backward emitted secondaries within the ± 1 GeV/c transverse momentum strip have momenta below a few GeV/c. Since the p-p system is symmetric, no information is lost if only the backward emitted particles are examined carefully, and this is possible with some of the apparatus already existing. This will prove, I believe, a real blessing, since precise measurements on the very energetic forward emitted secondaries will, on the contrary, be much more difficult.

In this respect, the situation for the storage rings is much more satisfactory, as everybody knows. The absence of a c.m. motion limits in p-p interactions the momenta of practically all secondaries within very reasonable values.

Figure 2 gives an estimate of the fluxes expected, again for π mesons, in the forward direction, when $10^{11}$ protons of 150 GeV interact in an ordinary target. The curve has been obtained by scaling, with some 'arbitrariness'\(^4\), the data furnished by present machines. The comparison with the 30 GeV accelerator is pleasant, but it should be remembered that the characteristic angles of emission are inversely proportional to the momentum of the secondaries; it follows that the most energetic ones are present only near the zero direction. The pion spectrum is characteristic for all kinds of secondaries; from it, it should be possible to guess the flux of, e.g., the charged hyperons, ($\Sigma^+, \Sigma^-, \Xi^-$) by multiplying the ordinates of Figure 2 by a factor $\sim \frac{1}{100}$, if the efficiencies observed at 25 GeV\(^5\) remain constant. The reason why I mention the hyperons is that, at 50 GeV, they have mean free paths of $\sim 3$ m, and it becomes conceivable to experiment on these particles with apparatus extending 10 m from the target.

In Figure 2 are also given the fluxes, always in the forward direction, of tertiary particles, neutrinos and gamma rays. It appears that at 150 GeV the same number of protons will produce, at zero degree,
neutrinos of ~15 GeV as abundantly as the present 30 GeV machines produce neutrinos of ~1 GeV. However, the experimenting with neutrinos involves other geometrical factors and this statement cannot be taken without further qualifications (see below).

2) The next topic is secondary particle separation and identification.

It seems that the separation of the various kinds of charged particles present in a secondary beam of ~70 GeV/c momentum can best be achieved with radiofrequency separators. Operating at 10,000 Mc, 70 GeV/c K mesons can be separated from π mesons by using two cavities, each a few meters long, located ~500 m apart. I shall leave to the experts the justification of these choices and the description of the details of this remarkable piece of equipment.

I will instead spend more time on identification systems, i.e., set-ups that allow the recognition of some kind of particles present in a beam, without actually extracting them out of it.

Čerenkov counters promise to again be of great use. A threshold counter, consisting simply of a gas filled pipe with reflecting internal walls and a light collector at the end, will deliver enough photons (~100) provided its length is

\[ L \approx 2 \left( \frac{p}{m} \right)^2 = 2 \gamma^2. \]

\( p \) is here the momentum of the beam, and \( m \) the mass of the particle that should just not produce Čerenkov light.

Of course, all lighter particles will produce ~100 photons at an angle

\[ \theta \approx \frac{1}{\gamma}. \]

These relations are gas independent.

At 70 GeV/c π and μ mesons can be isolated from K mesons if \( L = 40 \text{ m} \).

The pressure inside the counter of, e.g., hydrogen, should be

\[ \text{Pressure (atm)} = \frac{1}{2 \times 10^{-4} \gamma^2} = \frac{1}{4} \text{ atm}. \]
By running such pipes, which could also be slightly bent, through magnets and quadrupoles, there is the advantage of saving space and, more important, of eliminating secondary electrons. In fact, the presence along the beam of $\delta$ rays produced in the gas constitutes a source of background.

Focusing Čerenkov counters are presently very useful for the identification of particles with momenta around 10-20 GeV/c. Values of $\frac{\Delta p}{p} \approx 2 \cdot 10^{-4}$ have been achieved. At momenta greater by an order of magnitude, values of $\frac{\Delta p}{\beta}$ about 100 times smaller should be reached; this task is made difficult by the fact that, if one wants to utilize all the light band to which the photo-multipliers are sensitive, then the dispersion of the gas, if not corrected, does not allow values of $\frac{\Delta p}{\beta} \lesssim 2 \cdot 5 \cdot 10^{-5}$. Meunier and Stroot have designed, and are in the process of realizing a 2 m focusing counter containing a correcting ring (fused silica and sodium chloride) in front of the final slit that achromatizes the system. By thus eliminating the limitation due to the dispersion of the medium, they should reach $\frac{\Delta p}{\beta} \lesssim 10^{-5}$. The beam accepted can have transverse dimensions of $\sim 10$ cm and an angular aperture of $\sim 0.2$ mrad. Such a counter is thus Discriminating, Isochronous, Self-Collimating (DISC) and should be able to sort out Kaons from pions up to 100 GeV and $\Xi$ from $\Sigma$ up to 130 GeV. It also separates $\pi$ mesons from $\mu$ mesons up to 20 GeV.

Another class of instruments which promises to become useful for the identification of $\sim$ 100 GeV particles is that utilizing the logarithmic increase with energy of the collision losses. In gases, the rate of increase of the collision losses of ultra-relativistic particles ($\frac{E}{mc} = \gamma > 10$) is approximately, for ordinary pressures

$$\frac{d(dE/dx)}{d(\log \gamma)} \sim 0.15 \text{ MeV/g cm}^{-2}$$

out of total losses amounting to 2-3 MeV/g cm$^{-2}$. Measurement of losses should thus allow $\pi$ mesons to be distinguished from $K$ mesons of the same momentum ($\Delta \log \gamma = 1.15$) provided differences of $\sim 0.15$ MeV/g cm$^{-2}$ are clearly resolved. The density effect reduces the relativistic increase. In solids it is reduced to about one half, but is still present.
The importance of this effect for the future development of high-energy physics lies in the fact that, in a beam of given momentum the separation between two masses is proportional to \( \log \frac{m_1}{m_2} \), i.e., is \( \gamma \) independent, while differences between times of flight and differences between Čerenkov angles are proportional to \( \frac{m_1 - m_2}{\gamma^2} \) and sooner or later are bound to become impossibly small.

The relativistic rise can be observed in several ways. Yuan in Brookhaven\(^{(1)}\) detects the scintillation from Xenon. Richter in Stanford\(^{(11)}\) measures instead the secondary electrons emitted when the particle crosses thin metal foils. Both have obtained encouraging results. Any phenomenon related to the collision losses can show the relativistic rise; actually it could happen that some effect will turn out to be particularly sensitive to the relativistic rise and thus give us a more effective tool.

Another interesting property of this kind of detector is that the distribution of the collision losses, the Landau distribution is very different from a Gaussian distribution. As Purcell\(^{(12)}\) has pointed out, more information is then gathered by measuring \( \frac{dE}{dx} \), in many small thicknesses, than in a single large one.

It seems to me that the development of this kind of detector should be vigorously pursued.

3) Now let me examine what kinds of detector appear most appropriate for experimenting with the future machines.

As a general comment, let me first say that if the aim of high-energy physics is to build a detailed knowledge of what happens in the most characteristic high-energy phenomenon, the multiple production of particles, then it is necessary that the measurements be precise enough that the \( \pi \) meson mass be always well resolved. A rule of thumb for finding the resolution needed in the case of multiple production can be the following: A detailed knowledge of meson resonances seems to require the determination of the relative momenta of the particles involved within, say, 5 MeV/c. On the other hand, the average transverse momentum in multiple meson
phenomena is \( \approx \frac{1}{2} \) GeV/c. It follows that angles and momenta, in the lab, should be measured with a relative precision of at least 1%. It is with this requirement in view that I will discuss the detecting apparatus.

Scintillation counters will certainly remain useful, but simple experiments, involving few scintillators, will become even rarer than at present. Hodoscopes, especially with a computer "on line" are the systems to be considered. However, to reach the necessary resolutions, small solid angles will be mandatory and make the utilization of scintillator hodoscopes less promising, unless a way is found to reduce the dimensions and the complexities of photomultipliers.

Solid state detectors could play here an important rôle. They are essentially a semiconductor ion chamber and they can have very small transverse dimensions, and thicknesses of a few milimeters. They do not require photomultipliers and give outputs of a few millivolts, proportional to the energy absorbed; risetimes of \( \approx 10 \mu\text{sec} \) are already available\(^{13}\). The development of solid state detectors being in its infancy, this line of attack promises to be of great importance. Think, for example, of the possibilities of experimenting with the most energetic hyperons, within 10 m from the target. Superconducting magnets, DISC Čerenkov counters, and solid state detector hodoscopes seem a very good match.

Spark chambers continue to be useful for experimenting both with the 150 GeV machine and with the colliding beams. They can be small or large, deep or shallow, cubic, cylindrical, immersed in a magnetic field or with magnetic fields interposed; they can contain only thin foils, or very thick ones, or liquid \( \text{H}_2 \) targets. A spatial resolution of \( \approx 1/2 \) mm and a time resolution of \( \approx 1 \mu\text{sec} \) will, however, limit somewhat their use. The greater the energy of the particles, the greater the distance between the various chambers, and soon the time taken by the signal to travel back and forth will become larger than 1 \( \mu\text{sec} \). Also the number of chance coincidences limits the use of spark chambers to fluxes smaller than some 100,000 particles per sec.
Notwithstanding automatization the problem of scanning the millions of pictures that will be taken with spark chambers will always be with us, because the more efficient the scanning, the larger will become the number of pictures to be scanned; a situation we have already become familiar with for bubble chambers.

In order to eliminate the process of picture taking and scanning, sonic spark chambers are now being studied, where the position of the sparks is determined by the time that the sound produced by the spark in the gas takes to reach the sound detectors located around the chamber. In this case the digitization of the information is easy and "on line" computers could make a spark chambers as "simple" as an hodoscope. Unfortunately, the information carried by sound waves is coarser than that carried by light waves, and some details will be lost. The method is, however, promising, especially in the case of experiments where few particles are detected in each event.

The same comments apply to the grid spark chambers, where one of the electrodes consists of a grid of wires equally spaced. The spark co-ordinates are then those of the wires involved in the discharge. A resolution of ~ 1 mm has been achieved already at CERN by Krienen.

The already existing large liquid $H_2$ bubble chambers will be useful for experimenting with both the colliding beam apparatus and the 150 GeV machine, in the second instance provided that only the c.m. backward emitted secondaries are considered. In fact, the momentum of a 1 m long track in a 20 kGauss magnetic field can be measured with 1% precision up to momenta of ~10 GeV/c. In order to have the same resolution for the forward emitted secondaries, the length of the chamber should be increased in proportion to the square root of the momentum. I am told that an $H_2$ bubble chamber, 6 m long is feasible (the transverse dimension need not increase substantially because the transverse momenta of the secondaries remain constant). The most serious handicap in all cases is the difficulty of identifying neutral secondaries.
The photons from $\pi^0$ decay could be made visible by converting them inside gold plates placed inside the chamber. There is also the possibility of surrounding an $\mathrm{H}_2$ bubble chamber with a heavy liquid chamber, in which the neutral particles are much more easily converted.

These are thus far the most realistic proposals for solving the very important problem of neutral secondary identification.

Also the problem of the identification of the masses of charged particles remains without an answer; the number of $\delta$ rays that can be counted is always insufficient in ordinary $\mathrm{H}_2$ bubble chambers. At high energies, even the charged unstable particles become difficult to spot; the characteristic decay angle of an hyperon of 50 GeV shrinks in fact to $\sim 1^\circ$.

The advent of superconducting coils producing fields of 100 kGauss or more over a large volume will change the situation. Besides the obvious increase in resolution, a large enough field will allow the capture in the chamber of practically all secondaries below a certain momentum (a few GeV/c for a 2-3 m diameter chamber). On such long tracks the $\delta$ ray counting will permit accurate mass determinations.

4) We now come to a list of experiments which, at this moment, appear feasible and interesting.

a) Starting from the simplest, let us consider the measurements of total cross sections of strongly interacting particles. Counters will still be sufficient, with the help of identifying apparatus, e.g. Čerenkov counters.

It is likely that, at energies above 20-30 GeV all particles will produce flat, energy independent total cross sections, and the interest of these measurements will not be great. There is, however, the possibility that a more refined look will still be rewarding. The argument that discontinuities, the so-called bumps, are bound to disappear when the energy increases, is a sound one but has been proved wrong by the evidence accumulated thus far. In fact, whenever
accurate and narrowly spaced measurements of total cross sections have been made, significant bumps have been found - witness the last, at 3 GeV/c, in π - p collisions, recently found at Brookhaven\textsuperscript{16}).

It is thus worth considering a programme of total cross section measurements, and perhaps we could start right now with the beams already at our disposal. To be meaningful, these measurements must be carried out at energy intervals not greater than \( \sim \) 10 MeV in c.m. and have relative precisions not worse than a fraction of 1%. A rather long task.

b) Differential cross sections of elastic scattering of strongly interacting particles come second on the list. Some hint that they can be interpreted in terms of Regge poles makes them very popular today. Will the same be true a few years from now? Anyway, they should be measured, at small angles and at large angles.

Spark chambers, counter hodoscopes, single counters will be adequate for the task. It is a sure programme for both the 150 GeV machine and for colliding beams. Cross sections will become very small as soon as the four momentum transfer becomes larger than some \( (\text{GeV}/c)^2 \), and the limited intensity of the colliding beams will be badly felt for p-p collisions.

c) The study of inelastic interactions will be, in my opinion, the most interesting, as well as the most difficult one. Strong interaction means at these energies an inelastic process, where inevitably many secondary particles are created, and the detailed study of these events is hard. However, it begins to appear that these complicated phenomena have unexpected properties. In fact, at the lowest steps of the scale, - at Bevatron energies - a crop of particles and excited states has appeared as soon as the inelastic events have been analysed with adequate resolution and at the other end of the scale, at Cosmic Ray energies, there are indications that the secondaries are again produced in groups, - the so-called fire balls.
The detailed study of these phenomena appears thus to be a study of the grouping of mesons and baryons, i.e., a study of "mesonic matter".

Of very short life, but still with well defined properties, mesonic matter could constitute the third state of aggregation, next to molecular matter and nucleonic matter. We are very far from the simple model of Fermi, where only the obvious laws of chance were dominating!

The problems are formidable! Momenta must be measured better than within 1%, including those of the still elusive neutral secondaries. Lifetimes are ridiculously small. Still the main reward that will come from building larger machines will probably be the better understanding of mesonic matter. Here the colliding beams could be of paramount importance.

The first to be attacked will be the simplest channels; this is what we are still doing at 10-30 GeV. Slightly inelastic collisions, charge exchange collisions, two-body reactions. These measurements can conceivably be done with the tools described above. What is still missing is something equivalent to the H2 bubble chamber sensitive to neutral particles.

In connection with the high precision required by these measurements, the constructors of accelerators must be reminded that it would be nice if the internal proton beams, and the extracted ones too, could be kept mono-energetic, within at least \( \lesssim 0.1 \) GeV.

It appears that the necessity of stacking the beams will make it difficult for them to satisfy this condition in the storage rings.

d) The possibility of studying the reactions produced by neutrinos of energy up to at least 10 GeV constitutes one of the major attractions of the 150 GeV machine.
Van der Meer has proposed a simple way of describing the advantages and disadvantages of experimenting on neutrinos with high energy machines.

Let's make the following plausible assumptions:

i) that the overall length of the set-up of a given neutrino experiment is proportional to \( U \), the top machine energy. This is justified by the fact that the shielding is proportional to \( U \), as well as the mean free path before decay of the \( \pi \) mesons;

ii) that the \( \pi \) mesons coming out of the target are focused by a magnetic horn always with the same high efficiency. This is made possible by the constancy of the transverse momentum;

iii) that the \( \pi \) meson fluxes can be expressed by a universal function as proposed by Cocconi, Koester and Perkins.

It then follows that the number of neutrinos produced per interaction and crossing the unit surface of the detector and per unit interval of momentum is given by the "universal" curve of Figure 3, in the case of the CERN horn.

At 150 GeV the maximum flux of neutrinos is obtained at \(~4 \) GeV. A further gain in intensity is provided by the larger number of protons circulating in the machine (a factor of 6 per pulse in comparison with the present machine). The overall distance between target and detector will be \(~300 \) m. All this presupposes that the neutrino separation is obtained, as now, by absorbing all the other particles with, e.g., Fe, at 1 m per GeV; \(~150 \) m for a 150 GeV machine, a staggering wall!

Gold would reduce this thickness by a factor of two, but this does not look to be the simplest solution. Magnetized iron shields have been suggested, but not seriously enough. Let's hope that within ten years from now, an elegant solution of this mastodontic problem will be found.
e) Considerations similar to those made for the neutrino experiment, as far as the scaling of the experimental set-up is concerned, can be made for the realization of an intense, well separated $\mu$ meson beam. A 50 GeV $\mu$ meson channel will turn out to be $\sim 1000$ m long and allow $\mu p$ scattering measurements up to four momentum transfer of $\sim 7$ GeV/c$^2$. At present, the largest four momentum examined is still below 1 GeV/c.

f) Another class of possible experiments is that in which the electromagnetic form factor of the unstable particles is studied by observing how they are scattered by the electrons of stationary targets. The maximum four momentum transfers which can be obtained with a 50 GeV secondary beam are:

$0.2$ GeV/c for $\pi$ and $\mu$ mesons, $0.1$ GeV/c for $K$ mesons and $0.05$ GeV/c for antiprotons.

Precisions of $\sim 1\%$ in the differential cross section measurements at these momenta seem feasible using the techniques already known.

g) Finally, it must not be forgotten that the gamma ray flux created by a 150 GeV machine will make possible a host of photo reactions at energies thus far unexplored. Among them there is a curiosity: the reaction:

$$\gamma_1 + \gamma_2 \rightarrow e^+ + e^-,$$

the well known electron pair production, but this time from real photons, one from the accelerator and the other from a well collimated light source. A colliding photon beam experiment!

The maximum cross sections of 0.3 barns, occur, for head-on collisions$^{13}$, at $E_{\gamma_1} E_{\gamma_2} = 2 m^2 = 5.10^{11}$ ev$^2$. With 100 GeV photons from the accelerator, ultraviolet photons of $\sim 2500$ $\AA$ are needed. In a pipe, 10 m away from the target, 1 m long and illuminated by a flux of $10^3$ joules of this ultraviolet light emitted in 1 $\mu$sec (Laser?), an equally short burst of 100 GeV photons from the 150 GeV machine (zero degree emission) should produce one pair of electrons.
every 10 pulses. It appears that the observation of this reaction will not teach us anything that we do not know already. Still it would be nice to see these two 50 GeV electrons come out from the most perfect vacuum!

5) I shall close my talk with a sort of an anticlimax. We are so busy discussing big machines that we tend to forget that nature provides us with a steady beam of protons with energies much larger than our $10^{11}$, $10^{12}$ ev. Would it not be possible to apply the modern techniques to the investigation of Cosmic Rays and have a glimpse of what happens at energies not yet reached by present (and future) accelerators? Actually, invaluable work has already been done by studying with photographic plates, jets produced by Cosmic Rays of energy up to $10^{15}$ ev (Remember that the constant transverse momentum law was found by cosmic ray physicists!).

Also, the group of Dobrotin in Moscow operating with counters, cloud chambers and ion chambers, succeeded during the last years in gathering interesting information on proton interactions above $10^{11}$ ev.

Still there is room for improvement. A massive attack with large spark chambers, magnets and scintillator hodoscopes is very well worth considering. Near sea level, the negative Cosmic Rays, once freed of electrons, consist purely of $\mu$ mesons, of which about 50 fall every hour on a square meter and within one steradian, with energy above 200 GeV. At the altitude of Mont Blanc, the flux of protons with energy $> 10^{12}$ ev is $1 \, m^{-2} \, h^{-1} \, ster^{-1}$. Deep underground, the cosmic ray background is due to $\mu$ mesons coming from the zenith at a rate of a few $m^{-2}h^{-1}$. On the other hand, there, the neutrinos coming up from the nadir, after crossing all the earth, are $~300 \, m^{-2} \, h^{-1} \, ster^{-1}$, with energy above 50 GeV. A detector of the secondaries that these neutrinos produce in the earth beneath, if of adequate surface (say 100 m$^2$) could establish whether the cross section of neutrinos above some GeV is that predicted assuming a point interaction only ($\sim 10^{-37}cm^2$), or instead is that predicted assuming the existence of an intermediate boson ($\sim 10^{-35}cm^2$). In the first
case about one count per month would be registered, in the second, several dozens²⁰.

Perhaps, while building the new accelerators it would also be worthwhile spending some percent of the money available on a frontal attack of Cosmic Rays."

⁰
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THE CHAIRMAN:

"We are grateful to Cocconi for his very interesting contribution which in a certain sense is very optimistic about the possibility of doing experiments with a very high energy machine. What he has presented to us is extraordinarily interesting."

WINTER:

"I hoped that Cocconi would comment on beam transport at very high energies, since it is hard to imagine the scaling up of bending magnets and quadrupoles by a factor 10."

COCCONI:

"There is no need to scale up. Essentially the same magnets as now can be used at 150 GeV. The problem is one of length and I do not think efficiency in collecting particles will change at all. We should remember we are already in the asymptotic region. This would not be true if we compared 0.6 GeV to 25 GeV, because the angular distribution is not then determined by the constancy of the transverse momentum."

MORRISON:

"Cocconi is saying that total cross-sections may contain some surprises. The proton-proton cross-section is normally assumed to be constant: it is constant at machine energies and cosmic-ray data give the same absorption cross-section as at machine energies. There is, however, one very important difference. In cosmic rays, no one has ever observed a clean elastic scatter. This is because in cosmic-ray work there is a bias against measuring events with small multiplicities or small deviations in the incident tracks. If you take CERN data on elastic scattering and extrapolate them, this means that at 1000 GeV there are probably still 5 millibarns cross-sections.

If you add this 5 millibarns cross-section into what the cosmic-ray physicists observe, this must mean that the total cross-section must in fact increase with energy.

In this connection, it should be remembered that cosmic-ray physicists do not measure protons on protons from hydrogen but protons on such substances as silver or bromine."

SCHOCH:

"Cocconi has pointed out that experimental arrangements become essentially longer when one gets to 150 GeV. For electronic experiments does this mean at the same time that one can digest only a basically smaller
flux, because the time of flight within the equipment becomes already quite noticeable?"

COCOCCI:

"There is a fundamental difference between spark chambers and counters for instance. When one operates with counters, one can delay by means of delay lines the information from a counter until it is made contemporaneous with that of another counter. Accordingly, the resolving time does not suffer.

In a spark chamber, one has to let the particles go, then find out whether a particle is good and then trigger. The delay cannot be eliminated in any way. Accordingly, a spark chamber suffers from the lengthening of experiments, whereas counter experiments do not."

SCHOCH:

"Does it not remain true that you must not have more than one particle in a Čerenkov counter at any time?"

COCOCCI:

"No. The information is isochronous in the sense that each particle gives pulses which are transmitted with the same velocity as the particles. So you can separate particles very well, even if the counter is very long."

MICHAELI:

With reference to the solid-state counters mentioned by Professor Cocconi, has any work been done on the relativistic rise in those counters? This could be promising because of the low energy required for the formation of ion pairs. They should be able to supply very good statistics."

COCOCCI:

"I only know of a report by Yuan, where he produced a good energy loss in a solid-state counter. However, he did not study the relativistic increase."
PEYROU:

"If the bubble chamber is going to do the programme assigned to it by Cocconi, with whom I agree, in looking at special particles in multiple production, the main thing will be the precision of momenta in angle measurements. A stage is reached when it does not help to increase the size of the chamber any more, because of Coulomb scattering errors, and the most essential thing then becomes the field. The important thing will be to build a chamber of reasonably gigantic size – between 2 and 4 m – with the highest possible magnetic field. Assuming a field of 100 kGauss, the hydrogen chamber will still be much more precise than the heavy liquid chambers, because it should give a precision of 200-100 MeV for 50 GeV secondaries, whereas a heavy liquid chamber would be 5 times less precise. Nothing could be done about this, unless the field of the heavy liquid chamber was made 5 times larger.

I agree with those who say that the bubble chamber is a very stupid instrument. I hope, however, that in ten years' time it may be a little less stupid. You might then be able to register the photos on TV screens and scan them immediately with a gadget fitted with pattern recognition which will print only what is necessary and leave the rest out."

VON DARDEL:

"The neutrino experiment would be one of the few cases in which striking developments would result from the energy range available with a new machine, provided the vector boson exists. Given the spectrum calculated by van der Meer for the higher energy machine and given reasonable extrapolation for the neutrino detector, one finds that vector bosons should be produced at rates about 1000 times higher than with the present machine."
Maximum Momentum of π-mesons from 150 GeV Protons

Fig. 1
FLUXES PRODUCED BY $10^{41}$ PROTONS OF 150 GeV INTERACTING IN A TARGET

$\frac{d^2 N}{dp d\alpha}$ (ster GeV/c $10^{11}$ interactions$^{-1}$)

- $\pi^+$ or $\pi^-$ at $\sim 0^\circ$
- Photons at $\sim 0^\circ$
- Neutrinos at $\sim 0^\circ$

$\pi$ from 30 GeV protons, $0^\circ$
Neutrino flux density in CERN detector
a) as expected
b) with "ideal focusing"

Fig. 3
The CHAIRMAN invited Dr. Kowarski to give a paper on planning for data processing in a new domain of high-energy physics.

Dr. KOWARSKI:

"Yesterday we heard communications on various aspects - theoretical, engineering, experimental - of what amounts to a new venture in an entirely new domain of high-energy physics. I would like to add here a brief plea that in planning for this venture the data-processing aspect should not be forgotten.

It used to be, in the past. In the early days of high-energy physics events were rare or difficult to obtain; the processing of raw data could go on in between the catches and, since it raised no efficiency problems, nobody paid much attention to its methods and equipment. There were, of course, pioneering exceptions. Alvarez recognized the need of development in data-processing methods as soon as he realized the importance of the big bubble chamber, say in 1955 or 1956. Similar initiatives were taken elsewhere, maybe not as forcefully. But after even that, the new chambers and other detectors were developed rather more eagerly than the new data processing equipment capable of keeping pace with their output.

When the output reached the stage of requiring automatic scanning and measuring ("automatic" means non-dependent or less dependent on human intervention and therefore faster) it turned out that no automatic system was ready. In the last year or so it has become clear that there has been a major miscalculation and that data-processing methods have been allowed to fall behind the needs. Here are two examples of tangible manifestations of this state of things:

(a) The Hough-Powell digitizer, which is at present the most advanced among the partly automatic measuring methods for bubble chamber pictures, is not yet ready for daily use in physics. The hardware is ready, but some of the programming aspects are still in the course of development; the situation is exactly the same in all major centres where HPD's have been built - CERN, Brookhaven and Berkeley. There has been enough personnel and other provisions for efforts of classical kinds - mechanical, electronic; but not enough for programming, that is for a specifically data-processing effort.
CERN is now performing, in collaboration with M.I.T., a spark chamber experiment in which something between \( \frac{1}{2} \) million and 1 million of pictures will be taken, and is now groping for the possible ways of measuring them. Three ways are being envisaged - the old human way of digitizing projectors; the programmed-spot device which was recently developed by Deutsch at M.I.T. and uses a special computer which also came originally, from M.I.T., and finally, we shall use the HPD on pictures, from the same experiment. In both automatic methods the hardware exists, and the adaptation work to be done before the production can start is again that of programming.

Other examples could be cited: they are all parts of one acute problem due to the fact that the place taken by data processing in the experimental chain was not sufficiently recognized. If a similar lag occurs in the new stage of high-energy physics, the one we are discussing now, the consequences will be far more serious, because the new accelerators will be far more costly than even our present PS, and therefore the urge of extract the largest possible number of megabits (or terabits?) of significant physical information per hour of their useful life will be even more imperative.

I have looked at the papers prepared for this meeting and, to my biased eye, it appears definitely that the place given to data processing questions in this ensemble of discussions and considerations is inadequate. It is useful here to keep in mind that a high-energy physics experiment can be envisaged nowadays as having three aspects: the production of a suitable beam of particles, the detection of the physical event, and the processing of the resulting data. All three aspects are essential; all three have to be attended to and prepared. And just as continuous research and development is necessary in any living high-energy physics laboratory on methods of beam production and of detection, the same kind of effort is necessary on both the hardware and the software aspects of data processing.

Here, cited at random, are a few examples of questions on which ideas should be clarified for planning purposes. As time goes on, some of these questions will be solved, but some will remain, and others of the same kind will appear.

- Should the selection of significant events be incorporated in the detector, or left to the subsequent analysis? For example, if in a spark chamber experiment the triggering is very elaborate, there are fewer pictures to scan, and pictures are more similar to each other. If not, a faster and a more sophisticated automatic processing system is necessary.

- Should data processing methods be of universal and flexible kind, or should they rather be developed in connection with each specific kind of experiments? This was the case of Deutsch's successful system.
- Can particular problems be solved by particular programmes alone, or is there a need for numerous specialized scanning and measuring devices. In other words, what balance should be struck between the numbers of programmers and of electronic or mechanical engineers.

- What should be the ratio between programmers and other active contributors to an experiment? (Roberts used to mention a 1:1 ratio; we are certainly very far from it in our present work).

- When on-line computation is necessary, should every major experiment have its own computer (advantage: independence and flexibility), or should there be on-line sharing in a big central computer (advantage: substantially lower cost per operation, availability of a very large memory).

- How important is it to keep a full record of the event, such as a photograph? Or would it be enough to keep a reduced record, for example on a magnetic tape? Television cameras rather than photographic cameras can then be used, and one such system is being developed at Princeton.

- How much development of data-processing hardware, programming, time-sharing routines should be undertaken within the high-energy centre itself.

Planning of future HE physics activities must include thinking on all these questions and on many others of the same kind. Allocations of money and manpower should be foreseen accordingly. This was not done in the past and I wanted to call your attention to this past mistake and to the necessity of avoiding it in the future."

JOHNSON:

The problems raised by Kowarski have not been completely forgotten in our manpower estimates. It has been assumed that about 100 people would be working in this particular field. Some people may think this figure is too low, whereas others regard it as too high.

CHAIRMAN:

Kowarski has reminded us that gigantic sums would be needed for experimental equipment as well as for the accelerators themselves. A lot of thought and ingenuity would have to be devoted to the design and development of this experimental apparatus.
The Chairman invited Dr. Hine to give his paper on "Technical Considerations relevant to planning a European High Energy Programme".

DR. HINE

Introduction

Long-term planning of facilities for scientific research is notoriously difficult, but these days is also unavoidable. One must use all ideas and methods of planning available, to try and make plans which have accurate forecasts of those things which can be forecast, and use the best guesses for the rest, and leave these as unfixed as possible.

One can use:

- Detailed predictions from current knowledge of physics and machines
- Past experience about what have been successful types of policy
- Any general rules of thumb or economic boundary conditions which can be found.

An example of how things have gone in the past can be found by looking at the history of CERN. A good deal of conscious planning of the CERN facilities was made in 1953-54, and a number of reports exist, including a paper by Citron and myself on the PS given at Varenna in 1954; this summer I gave, again at Varenna, a talk comparing predictions with reality, to see how well things had worked out in practice.

For both machines, there are three subjects of which predictions and plans had to be made:

a) Machine performance, cost, manpower, time scale
b) Experimental facilities, beam intensities, ways of using the accelerator
c) Experiments to be done, both for their physics and for their technique. Forecasts here had to be made, partly in the justification of the projects, and partly because they influenced the machine design and the facilities planned.

Comparing 1963 with 1954, we find that under a), essentially everything has worked out according to plan, except that the PS machine intensity, which was forecast very conservatively, has turned out to be about 100 times better than given at the start. Even so, we still think that the maximum current possible will be about the same as in our original design studies. Nowadays a design will have a much smaller safety factor on intensity. Costs and manpower were within 20% of the estimates. Under b), most of our errors on the PS come from somewhat underestimating the scale of experimental equipment, and the total space and staff this would
need, and its cost, if the machine were to be fully exploited. Thus we should have made the experimental halls bigger, and made clearer provision for extra experimental areas, and for generator buildings and laboratories for experimental groups. We foresaw the need and use of quadrupole and bending magnets, though not in sufficient numbers. We did not make bad estimates, mainly based on cosmic-ray data, for secondary beam intensities, for then known particles ($\pi$, $p$, $n$, $\gamma$). $p$ and $K$ were essentially unknown at the time.

We did not forecast the sharp forward peaking in centre of mass associated with most real reactions, leading to fluxes near $0^0 5-10$ times higher. For future machines, the Cocconi-Perkins formula should give even better forecasts. Under c), our ideas on detail of experiments and even of experimental techniques for the PS were hopelessly wrong: bubble chambers, spark chambers, gas Čerenkov counters, high speed transistor electronics, fast computing techniques, separated beams, none of these was thought of, and nor was the scale and complexity of present day equipment. The experiments and topics of physics for which the machine is suitable have been through several revolutions since 1954, and all the then outstanding problems have been solved, or have lost first place in importance.

For the SC the technical side was better foreseen, involving a smaller extrapolation, but the entire basis of the experimental programme is quite different, having been switched to weak interactions and similar problems shortly before the start-up of the machine.

Despite our general ignorance on details, this has not really mattered: we were sufficiently nearly right on the general parameters and properties of our machines, and left room in most cases for expansion, though at some cost, where we did underestimate. The universal generosity of Nature, who has so far always made useful work for any good accelerator, has looked after the subjects for research, and the technical ingenuity of physicists has provided the means for observation and measurements.

Today, in trying to forecast for 1972, we can see the same situation: Johansen can talk in great detail about the machines which will be operating by then, but has to make general assumptions and aim always at flexibility in discussing experimental area design; Van Hove can give general indications on theory's needs for new data, but we can hope that a lot of his currently most important problems will be solved, or made to look quite different, somehow or other before a new accelerator comes along; Cocconi can tell us how difficult it will be for experiments to go beyond about 100 GeV in machine energy, if we are restricted to the foreseeable extension of current techniques. This, however incomplete, is still the best available evidence for making our plans, and we have to use it, and extrapolate from it in the most reasonable way, using any other general arguments and rules to guide us which we can find.
Limiting Parameters and General Rules

a) Time scales
b) Sizes: equipment and manpower
c) Money: needs
d) Energy, intensity, types of particles and machines.

The following remarks may in some cases seem trivial: if they are accepted, though, one can often arrive at interesting conclusions in fields where no detailed technical guidance is available.

ad a) There are several characteristic times of importance:

i) Useful life of accelerator 10-15 years: $T_1$
ii) Construction time (including planning) 6-10 years: $T_2$
iii) Time to establish a new experimental technique as routine 3-6 years: $T_3$
iv) Major changes in the centres of interest of physics, or new ideas for technique, occur every few years or sooner: $T_4$

The paradox of planning is that since $T_2 > T_3 > T_4$ an accelerator which is intended to help physics really break new ground must be designed without knowledge even of what experimental techniques will be used with it, and the latter will be developed in ignorance of what experiments or topics of physics they will be used for.

This is clearly brought out in my remarks earlier on about our experience with the PS.

Taken with the comparatively short time (say 2 years) for competent physicists to get used to a new accelerator, and its long useful lifetime, these facts seem to indicate that the value of a good accelerator will not depend on whether it has very good and experienced physicists in its first year or two of life, or whether it starts to work a year or two ahead or behind a similar competing machine. Conversely, a noticeably better performance of accelerator will continually give a laboratory an edge over its neighbours in the same fields of research.

Too little and too late does however seem to be a bad policy, unless the machine is deliberately intended mainly for routine research or teaching.

ad b) Experimental equipment usually deals with particles of similar energy to those being accelerated, and uses similar technology, at least after a year or two of running. Very roughly then, one can expect a constant ratio between size and complexity of equipment and of the accelerator.
most big beams are about one machine diameter long. Most ordinary experiments cost 1-2% of the capital cost of the accelerator they are made with.

Sizes and number of experimental groups both increase rather slowly with size of accelerator; the total of experimental staff seems to lie between 1 and 2 times the size of the machine group, and both go about as $\sqrt{\text{size of machine}}$ or less.

Both machine groups and experimental staff need more backing services and off-line equipment for data handling per man as the laboratory gets larger; hence the overall staff may be about proportional to machine size. Since the machine group is rather larger than the construction staff, the final laboratory size seems to grow by $\sim 3$ or more after the end of construction. Another relevant quantity is the maximum rate of growth which can be reasonably handled. During a construction period of a well organized project, 30-50% per annum is quite feasible; later on, with smaller groups, leaving the business as they go along, maximum rates of 15-20% per annum seem more normal.

This falling rate of natural expansion implies that the manpower build-up of a new laboratory with a large machine is very crudely linear in time, over the first 10-15 years anyway.

ad c) Money needs:

For a given technological level, annual cost is about proportional to the total staff numbers over a rather wide range of work and size of laboratory; excluding buildings, CERN's money needs per man have hardly changed from 1955 to 1963.

With the assumptions on staff build-up in b), this leads to a budget growth of a CERN-like laboratory as sketched.

\[\text{Cost/yr} \quad \sim \frac{1}{2} \text{Machine cost/Year}\]

\[\text{Operation} \rightarrow\]
The time scale seems to be rather slowly varying: 5-7 years will cover most machines for 3-150 GeV under current conditions.

If, as may even be desirable for machines which are lower down the pyramid, the fullest exploitation is not aimed at, the build-up of money after the start of the machine may be slower. Construction should always be fast.

ad d) Under energy and intensity there are three types of question on which we need any general guidance we can get:

i) Comparison of extreme cases: very high energy vs. very high intensity
ii) Comparison between similar large machines (e.g. 150 GeV vs. 120 GeV)
iii) Specification of smaller machines well down in the pyramid: how to choose parameters.

Here we are making forecasts and value judgements at the same time, which is a danger squared!

ad i) Van Hove says that one is more likely to break really new ground by the use of higher energy machines, and that the high-intensity machines are most necessary for refinement and consolidation of theory.

There has been a rather sad history in the past of high-intensity projects having been beaten in their own field by rather later high-energy machines because of the much greater overall power of the latter. This might continue: Symon shows that the MURA 10 GeV machine with $10^{14}$ p/sec will only have a good field for secondaries of less than 3 GeV, compared to, say, a 100 GeV FS, costing about the same, and this is rather a small area of work. High-intensity machines are thus a little like storage rings (which are high-energy machines par excellence), though not so badly specialized and with more obvious uses. A reasonable programme must include both, taken on the world scale, but the high-energy machine seems to offer more value for money.

ad ii) On the question of comparing one machine with another, small differences can play a significant role: the Bevatron ought to have 1-2 GeV more to make antiprotons well, 300 MeV cyclotrons were a failure for pion physics, the AGS may well have the edge on the CPS for the intermediate boson. These are three cases where 20-30% energy can give one machine a private field of research over a competitor. This difference gives a factor of ~3 intensity over the upper half of the pion spectrum in favour of the higher energy machine.
Even a factor 2 in intensity does not seem to be as important a deciding element. Along with intensity, the efficiency of secondary beam production and the ease of using the machine must also be considered.

Bearing in mind the relatively small importance of a year or two's delay, it looks as if there is a case for building say 150 GeV rather than 100 GeV, even if it were to take more time.

Assuming it is decided to make, e.g. a 4 GeV PS for a third layer type of machine, how should its intensity be fixed? The generosity of Nature, and the typical time for something new to occur in a particular field, mean that these machines will continue to do good work, but it is important that the intensity should not be seriously below that of a well-established competitor. A conclusion could be that it will be better to build fewer, better small machines even at the price of a certain concentration of effort.

For the smaller local machines it looks worthwhile to consider seriously using other particles, or having special ideas, such as storage rings or unique experimental facilities. In this way a small laboratory can have some "private" but interesting lines of research, in which it is not continuously facing comparison with larger accelerators and the fierce competition of the major institutes. These new ideas can also be the starting point for later generations of larger machines or equipment.

Hypothetical European Expenditure on High-Energy Physics 1960-1975

The generalizations about expenditure and growth of laboratories made in the previous sections can be combined with data from the report by Amaldi to the recent Frascati Accelerator Meeting, and from statistics made available to the Bannier Working Party early in 1962, to suggest the likely future growth of high-energy physics expenditure in Europe, and how new accelerator projects could be fitted in.

Figure 1 shows the data on laboratory costs available to me at this moment, and my guesses (they are no more than that) for the missing information. The known figures confirm the simple triangular growth curve with a plateau suggested above, and the same shape has been assumed for DESY and NINA (Liverpool Electron Synchrotron), fitted to the known total costs and time scales.

The steady growth of expenditure after the start of operation has been the rule in all healthy accelerator laboratories: despite forecasts for certain laboratories which suggest it will not occur, I have assumed this growth in all cases. The growth rate shortly after the start of operation I have assumed to be about 10% per annum for the national laboratories. This is too low for the optimum build-up to a state of high exploitation such as CERN should be aiming at, but it is perhaps even an optimistic estimate at the moment.
The resulting total expenditure grows roughly linearly, at about 10% per annum in the near future. This should be taken as the minimum acceptable; in fact, it is very low compared with the past or possible future growth for the main American laboratories. A more forceful policy for growth would involve ~350 M Fr. per year in 1967 instead of 300 M Fr., and ~470 M Fr. by 1972 instead of 400 M Fr. (Costs for the future are given at 1962 prices, so inflation must be added.)

A new accelerator programme for international construction might be equivalent to a 200 GeV accelerator, for which the estimated expenditure curve (determined from the proposed construction programme for the different components and buildings) is shown in Figure 2. It amounts to some 220 M Fr. per year in 1970, i.e. about one half of the total budget for existing large laboratories (3 GeV or over) at that date. The expansion of expenditure after 1972 can be questioned, since it implies very high expenditure on experiments and equipment.

Expenditure on smaller laboratories, and physics departments not owning accelerators, must also be considered. In 1962 this amounted to about 100 M Fr. (for Europe) if Amaldi's figure of 3.5 times CERN for the total European high-energy physics expenditure and my estimates for the main laboratories are correct. The least that should be aimed at for the future is to keep the ratio between large laboratory and small laboratory expenditure the same. This is also shown in Figure 2. Including the cost of a 200 GeV machine, this leads to about 750 M Fr. per annum in 1970, i.e. an average growth rate of only 12% per annum over this period for the total European high-energy physics expenditure, falling to 7% per annum by 1974.

On the supply side, a curve is given in Figure 2 which indicates what might be a reasonable expenditure for large basic research (high-energy physics, space physics, etc.) in Europe: it is 50% of 0.2% of the total national income in 1962, rising to 30% of 0.3% in 1970 and of 0.4% in 1978. These higher figures correspond to a growth of expenditure on basic research of about 8% per annum, which is not unreasonable even if the number of scientists in basic research were to remain constant, since they still will need more technicians and more expensive equipment per man as techniques develop.

It is clear that the suggested high-energy physics programme takes, if anything, a diminishing fraction of this estimate of reasonable resources.

Figure 2 also gives the US Ten Year Projection for total expenditure by the Federal Government on high-energy physics made in 1960, and the actual expenditure in 1963. The latter is a little less than the projection because of delay in starting the Stanford Linac. Operating expenses in 1963 are slightly higher than predicted. The projection includes further local and regional accelerators and also a very large proton synchrotron.
A number of questions arise at once:

- What are the present day official budget forecasts for the existing large laboratories, and how do they in total compare with my estimate?

- Are these official estimates in fact justifiable? Would it be better to concentrate resources on intensive exploitation of these laboratories on a European basis rather than talk of more national or regional accelerators?

- Is the amount I have assumed for Support (100 M Fr. in 1962 - 200 M Fr. in 1972) adequate for university departments working with large laboratories and on their own with small machines? If so, will it also cover several more medium sized accelerators? Such accelerators, equivalent to two CERN PS machines in total, would cost about 100-120 M Fr. per year by 1968-1970, leaving only the same amount of money as now for the universities directly. Figure 2 also shows the effect of adding this amount for regional machines and another 100 M Fr. for local support for a 200 GeV laboratory.

- If the comparison with Gross National Product has any meaning or value as an argument, how should it be presented, and can the curve I give be justified? The annual expenditure for high-energy physics by 1970 shown on my curve amounts to 0.5 per mil of the Gross National Product, which is half the figure in the US Ten Year Projection, so it is not unreasonable from that point of view, and even supports my suspicion that I may have been underestimating.
Fig 1: Budgets for existing large high energy laboratories 1954-1972.

CERN, Nimrod, Saturne: Actual budgets
DESY, NINA (Liverpool): Total costs 100 M, 50 M Sw. Fr.
Fig 2: Hypothetical European expenditure on high energy physics 1960-1975

- Reasonable Big Basic Research for Europe
- USA 10 Year Projection 1960-1970
- Actual HEP in USA 1963
- CERN 200 GeV
- 5% p.a.
- 10% p.a.
- 15% p.a.
DE RAAD

I have looked at the problem of experimental areas for a very large machine. I think this would be different from the PS in two respects.

1. External targets would play a much more important part than internal targets. In a 300 GeV accelerator the forward collimation of the secondary beams would be such that it would be very hard to work from internal targets. It would accordingly be much better to base everything on external targets, which could do practically all that is expected of internal targets and have the advantage of accessibility from all sides and flexibility. For instance, it would be easier to adapt an external beam to whatever new experimental devices or techniques might be invented.

With external targets there would be less danger of contamination by induced activity and the contamination of external targets would be easier to handle.

Finally, it would be cheaper and easier to shield external targets than internal ones.

Accordingly arrangements might be such that experimental physicists might never need to see the machine.

2. The general shielding problem would also be different from that of the PS. Although a ten metre thick baryte wall might be sufficient to stop most particles, a thickness of about 100 metres would be required to stop muons. To overcome this difficulty arrangements could be made for the muons to go downwards into the earth.

HINE

I agree the trend will be towards external targets. However, we should not forget the advantage of internal targets from the point of view of physics. Because of possibilities of multiple traversal, the physical volume of an internal target could be much smaller than that of an efficient external target. This may have significant value in designing refined optical systems. If you start from a point source, you can always focus better, make separations better, etc.

It is obvious that the difficulties on the experimental side will be going up faster than linearly. Therefore, if you add the cost of a full-scale ejection system to the cost of the machine, my feeling that staff and cost go up and up is reinforced.
DE RAAD

I do not agree about target sizes. Owing to the fact that the beam shrinks more and more at higher energies because of adiabatic damping, you can focus your beam down to targets of a few tenths of a millimetre. Accordingly, the optics can be as good with external as with internal targets.

COCCONI

You could still have some surprises about intensity in colliding beam machines, as improvements in vacuum technology could result in a major breakthrough.

HINE

I was referring mainly to machines of conventional type for which designers expect to get within a factor of 2 of the space charge limit.

JOHNSON

I agree we may be conservative about the intensity in colliding beam machines. In this case the interaction rate goes as the square of the machine intensity. In storage rings we have things like phase/space densities which we may be able to increase at the sacrifice of total intensity. At the moment, however, the estimates given should be used as the basis for the discussion.

WEISSKOPF

I am very glad the discussion is reverting to storage rings. We should all be aware that this open window on 50 GeV in the centre of mass is very important for science and for the attitude of physicists towards high-energy physics. If they were not built, the atmosphere in high-energy physics would be too conservative.

COCCONI

I am sorry I forgot to underline the advantages of storage rings which I had written down in my paper. One of them is the possibility of finding out what "mesonic matter" means. The storage rings would be very good for investigating this kind of problem, because the interaction cross-section concerned would be something like 30 millibarns. The number of interactions would be very high and, instead of looking into the geometry and the correlation between some of the particles, it would be possible to try to understand the full interaction.
AMALDI

I entirely agree with Weisskopf that we must not forget the storage rings which are very much a CERN idea.

I also agree with Hine that the consequences of the pyramid should not be taken too seriously. If CERN built a 150 GeV machine, it would be wrong to think that each university should have its own machine. It is good to build a small machine only if it is then possible to perform certain types of experiments which cannot be done with other machines. It would not be a good idea, for instance, to build several AG synchrotrons of a few GeV. Moreover, if a university builds a small machine, great care should be taken about the organization of the work, because the machine must not be run by one university alone.

With regard to the figures quoted by Hine, which agree with the ones I discussed at Frascati a month ago, I must stress that the data are only good enough to convince ourselves that we are not crazy. However, no government would yet accept the notion that fundamental research can be talked about in terms of a percentage of the national income. Accordingly, the figures should be presented in the right way, so that we can convince officials that we have a rational case.

CASSELS

I rather disagree with Amaldi about the basis of the pyramid. We must take this problem quite seriously if Europe is not to divide into two camps, i.e. those countries which are rich enough to have their own domestic programmes and those which are not and consequently have some difficulty in making proper use of central facilities.

Some scorn was poured on the idea of mass-produced AG synchrotrons. I agree that if you have a clever idea for a national or regional accelerator, you will have a more interesting machine. You should not forget, however, how much the present state of high-energy physics owes to a very definite, stereotyped and standard machine, viz. the 300-600 MeV cyclotron. These all-purpose accelerators provide a useful training ground for high-energy physicists who spend some part of their career at more glamorous places.

FARLEY

I agree with Cassels. We are in danger of neglecting the role of small (10 GeV) machines at the lower level of the pyramid. Looking back historically, a characteristic energy is needed for the study of various phenomena. In chemistry you need the thermal energy provided by a large number of Bunsen burners. In spectroscopy and X-ray physics, spectrosopes and X-ray tubes are very important. 10 MeV Van de Graaf's produced in
large numbers make a very great contribution to the study of nuclear physics. Cassels has mentioned the part played by cyclotrons. Now the logical development is to have a sprinkling of rather efficient high-intensity machines of a few GeV.

HINE

I agree that the approach advocated by Cassels and Farley is correct in theory. Each decade should plug in accelerators ten times as good. The difficulty is that the expense would mount at a terrible rate.

BERTHELOT

Can somebody tell us about the possibility of storing anti-particles in storage rings?

SCHOCH

It seems possible to store $\bar{p}$ at the rate of $10^{-7}$ or $10^{-8}$ of the number of $p$ stored. Accordingly the reaction rate would be reduced by about that factor and the minimum detectable cross-section would be reduced correspondingly. Therefore, probably only experiments such as diffraction scattering at very small angles would be possible, as well as inelastic experiments, still at feasible counting rates, but of course a long way below the rate for proton-proton collisions.

SALVINI

Until recently I was doubtful about storage rings. Now there seem to be a number of reasons for starting work in the energy region of the storage rings. In my opinion they are the following:

1. There are nice technical problems to solve and, as Cocconi has pointed out, some problems are going to be increasingly difficult to understand with rising energies in the laboratory system, whereas in the storage rings the geometry may be better.

2. You should also bear in mind what I call the asymptotic consideration. Looking at the proposed time schedules in Europe and the United States, there should be a fairly long period during which storage rings would remain alone in their own field. We could be quite confident that in all the phases of physics we do at high energies, the storage ring will come at the right moment to give the asymptotic behaviour of something.
Accordingly, in view of their relatively low cost, we should give them very serious consideration and even start technical development work on them, as if we had already decided to build them.

THE CHAIRMAN

We are very grateful to Kowarski and Hine for their contributions.

The meeting rose at 11.30 a.m.