SOME COMMENTS ON THE FUTURE OF VERY HIGH ENERGY ACCELERATORS

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Preface

This talk is intended as an introduction to a discussion session on the problems that are likely to arise in designing and building future very high energy accelerators. The questions I shall be discussing include,

- can the present accelerator designs and technologies be extrapolated and used for the next generation of machines ?
- will their size and cost require an interregional effort to build them ?
- can new ideas for accelerating particles be developed in time and what is expected of these new ideas ?
- how can these new ideas be stimulated ? how to increase the effort on accelerator research and development ?
- how to interest young physicists in accelerator research and development ?

No doubt other questions will arise in the discussion but perhaps these will serve as an introduction.

This is certainly not the first time that the future of high energy particle accelerators has been discussed. In fact, it has been a subject of lively discussion for over 20 years so perhaps I should start by recalling for you the history of these discussions and then go on to say how they have developed.

Interregional discussions on the future of high energy accelerators

As the size and cost of particle accelerators increased they outgrew their natural habitat which was originally the universities, and national laboratories were set up in which they could be built and operated centrally in most of the regions of the world where high energy particle physics was an active research subject. In some regions, notably Western Europe and Eastern Europe together with the Soviet Union, international laboratories were set up for this purpose.

It seemed reasonable to predict that ultimately, when the machines had reached even larger dimensions and even higher costs, it would be necessary to set up an interregional centre where a single machine complex could be built which would serve the whole world community of high energy particle physicists.

Of course, nobody knew when this would happen or even if could ever happen given the political divisions of the world but the trend seemed clear enough to justify serious disucssions on this topic.

The subject was first brought up at the time when the 30 GeV proton synchrotrons at CERN and Brookhaven were coming into operation. At the International Conference on High Energy Physics at Kiev in 1959, the IUPAP Commission set up a series of meetings to discuss interregional collaboration in the field of high energy particle accelerators. The participants at these meetings were scientists from the USA, the USSR and Europe.

Several meetings took place during the years following 1959 and a climax was reached at a meeting held in Vienna in 1964 to discuss how to build the next accelerator. The conclusion of that meeting was that machines less than 300 GeV energy could be built on a regional basis but for machines of higher energy, interregional action would be needed for their construction.

Soon afterwards, the USA launched a project to build a 200 GeV proton synchrotron which was later on upgraded to 400 GeV and built at Fermilab, and CERN launched a project for a 300 GeV proton synchrotron which was also upgraded to 400 GeV later on.

Not surprisingly, the meetings on interregional collaboration lapsed for a while and only restarted again on a tripartite basis in 1969.

When the Fermilab and CERN 400 GeV machines came into operation the problem of how to build the next machine again became a serious matter of discussion and at a meeting in New Orleans in 1975, a study group on future accelerators was set up. Even a name was given to the new machine at that time - the VBA or the very big accelerator.

In 1976, a proposal was made to the IUPAP Commission on Particle and Fields that it should sponsor an International Committee for Future Accelerators and in 1977 this Committee, called ICFA for short, was formally set up with participants from the USA, USSR, Western and Eastern Europe and Japan. By this time, projects for a next generation of machines were already being launched in the four regions of the world most active in this research,

- Isabelle (a 400 GeV proton-proton collider) in the USA,
- UNK (a 3 TeV proton machine) in the ~~USA~~, USSR
- LEP (a large electron-positron collider) in Western Europe,
- Tristan (an electron-positron collider which could be developed into an electron-proton collider) in Japan.

Other accelerators projects were launched in these regions later on.

One of the first initiatives of ICFA was to formulate a set of guidelines for the interregional use of these new front-line machines which were seen to offer complementary experimental facilities but were being built in different regions of the world. It was reasonable to suppose that experimental groups from the different regions would want to get access to these machines and the guidelines laid down the conditions under which groups from the other regions would get such access. These guidelines were agreed by all the major laboratories, including those who would be operating the new front-line machines.

The second initiative of ICFA was to set up Workshops to study the possiblities and limitations of future accelerators. Two workshops were organized, one at Fermilab in 1978 and a second at Les Diablerets near CERN in 1979. From these workshops emerged two machines, based on existing accelerator design ideas and technologies but with some imaginative extrapolations.

The first of these machines was a post LEP electron-positron collider for an energy of about 350 + 350 GeV and a luminosity of about 10^{33} cm^{-2} s^{-1}. Since it was considered that synchrotron radiation losses in a circular machine would be excessive at these energies, the machine was based on the idea of linear colliding beam accelerators. Two versions of such machines were considered; a superconducting R.F. cavity version and one with room-temperatue R.F. cavities and the main parameters were,

R.F. system	Superconducting	Room-temperature
Repetition frequency	1.4 10^4	10
Particles per bunch	5.6 10^{10}	10^{12}
R.F. voltage gradient	20 MV/m	100 MV/m
Length	2 x 17.5 km	2 x 3.5 km
Average mains power	414 MW	40 MW
Peak R.F. power	10 MW	10^6 MW

Both of these machine designs presented formidable problems. The average mains power of the superconducting version and the peak R.F. power of the room-temperature version give some measure of difficulties presented by the R.F. systems. The feeling was that the room-temperature version looked a more practical design. The Single Pass Collider at SLAC and work going on at Novosibirsk will throw more light on the practicality of many of the design features of these linear colliding beam machines.

The second machine was a post-UNK 20 TeV proton machine giving 10^{15} protons per pulse which was also a protron-antiprotron collider for 40 TeV energy in the centre of mass system with a luminosity of 10^{32} cm^{-2} s^{-1}. Assuming that magnets can be designed and produced to give 10 Tesla field the circumference of such a machine would be about 60 km or over twice the size of LEP.

The size and cost of these machines suggested that they would probably be candidates for the interregional accelerator complex which ICFA was set up to consider. The position of these two machines on the well known Livingston plot is shown in Figure 1.

About a year ago, as a result of the workshops just mentioned and in view of the problems that had arisen in trying to extrapolate existing (or near existing) accelerator designs and technologies, the need for radically new ways of accelerating particles became only too evident and ICFA began to interest itself in this question. Any new ideas which would reduce appreciably the size and the cost of future very high energy machines and preferable both together would clearly be very welcome. However, it was also clear that after 50 years of continuous development of accelerators and colliders, it would not be easy to come up with new ideas which would be better than the existing ones. In this respect, there is a very simple parameter which can be used to set a target for the new ideas. This is the MeV/m they can offer, calculated by dividing the maximum beam energy of the machine by its circumference or its length. Existing or projected proton machines already reach quite high values of this parameter, for example,

- Fermilab and CERN 400 GeV machines - 70 MeV/m,
- UNK and Tevatron (5 Tesla magnets) - 150 MeV/m,
- a 10 Tesla magnet machine - 300 Mev/m,

The circular electron-positron colliders, due to synchrotron radiation losses, reach much more modest figures,

- PEP and PETRA - 9 MeV/m,
- LEP at top energy - 5 MeV/m,

but the Single Pass Linear Collider using the SLED II modification is planned to reach 17 MeV/m and test cavities have reached 50 - 100 MeV/m which is approaching the gradients of the proton machines. A not unreasonable target for the new ideas would therefore be around 1 GeV/m.

In the nature of things it is difficult to say anything about the cost of accelerators based on radically new ideas and indeed it may be counterproductive to worry too much about this aspect of new ideas at the beginning. Nevertheless, it is worth noting that the 400 GeV proton machines, after decades of machine development, cost about 2 MSF/GeV which if applied to a 30 TeV machine would give a cost figure of 40,000 MSF. Even if built and funded on an interregional basis a machine costing this much looks prohibitively expensive but to reduce the cost by a factor of 10, which seems essential, will not be easy, especially when one remembers that alternating gradient focusing, one of the biggest cost reducing ideas of the past, only gave a reduction factor of 3 to 4 over constant gradient focusing for machines of the same energy. Indeed reducing the cost per GeV is a bigger challenge for the new ideas than increasing the MeV per m, which, no doubt, has inspired Fermilab recently to look at the idea of using very cheap mass produced magnets of 2 Tesla field to reduce the cost per GeV and a neighbouring desert as a possible site for a new machine.

Another question which has been raised at ICFA is when will these ideas be needed and how long will it take to develop them to the stage that they can be confidently used for a very high energy machine. Clearly the first time they could be used, if successful, is for the generation of machines after the ones now under construction, perhaps in ten years time. If it takes longer than this, then it will be the generation of machines after that, perhaps 20 years from now. These appear at first sight to be ample times in which to develop new ideas but past history shows that it may well take that time. For example, the Tevatron when it comes into operation next year will be the first big accelerator to use superconducting magnets but the first paper designs for such magnet systems were presented in 1961, 23 years ago, at one of the International Accelerator Conferences. A less happy example is collective field accelerator ideas first put forward in 1956 by Veksler, Budker and Fainberg which are still seeking a practical application. On the other hand, alternating gradient focusing was taken up within a year of its invention and used in the CERN and Brookhaven 30 GeV proton machines but that did not require the development of any new technology. Perhaps the conclusion one can draw from past experience is that it may well take 10 or 20 years to develop new ideas to the stage they can be used for a new very high energy machine particularly if they involve developing new technologies on a mass production scale. ICFA concluded that it is important to stimulate new ideas now and to increase the present rather low level of this kind of accelerator research as soon as possible.

Most of the discussions during the last six months in ICFA have revolved round the problem of how to increase accelerator research and how best to organize it. At the present time, most of the major laboratories are very busy with new accelerator projects. Indeed, the list is very impressive and worth recording,

- Brookhaven - Isabelle or an alternative,
- CERN - LEP (and the SPS p-pbar),
- Cornell - Study of a 50 + 50 GeV e^+e^- collider
- DESY - HERA ep collider,
- FNAL - Tevatron I and II,
- KEK - Tristan e^+e^- collider (and ep collider),
- Novosibirsk - Linear e^+e^- colliders,
- Serpukov - UNK, p and p-pbar,
- SLAC - Single Pass e^+e^- collider 50 + 50 GeV.

Not surprisingly the staff of these laboratories are fully engaged on these projects.

In the past, there were national laboratories in the different countries with lower energy machines and strong accelerator groups but nowadays, nearly all these laboratories are building accelerators for other purposes than high energy physics, such as spallation sources, synchrotron light machines, etc.

Going further back in the past, university groups were once very active in accelerator research but when the machines moved to national and international laboratories most of them lost interest in this work. At least in Europe, there is very little teaching of accelerator physics in the universities and consequently very few young physicists interested in this fascinating subject. To make matters worse, there are now very few vacancies for young physicists interested in accelerator research in the major laboratories due to the cutting back of their budgets during the present economic recession.

Given this far from satisfactory situation, ICFA proposed to bring together the limited efforts that are available in the different regions and to set up an interregional programme of advanced accelerator research. Hopefully, this would stimulate the work and encourage new groups to enter this field. The present idea is to set up some kind of interregional body to advance accelerator research by way of an association of the major laboratories.

However the work is organized, the main ingrediens for success are an enthusiasm to press on with this work, a recognition that it is now urgent to increase the effort, a modest amount of money (about 1% of annual budgets of the major laboratories whose future depends on the construction and operation of very high energy accelerators) and, of course, a plentiful supply of new ideas which can be developed into practical designs for very high energy machines which will be cheaper and smaller than machines based on extrapolations of current ideas and technologies. Above all there is the need to interest more young physicists in accelerator research and to find some way of offering them a career.

I hope that these comments are sufficent to set of a lively discussion on this topic.

Colliding Beam Machines

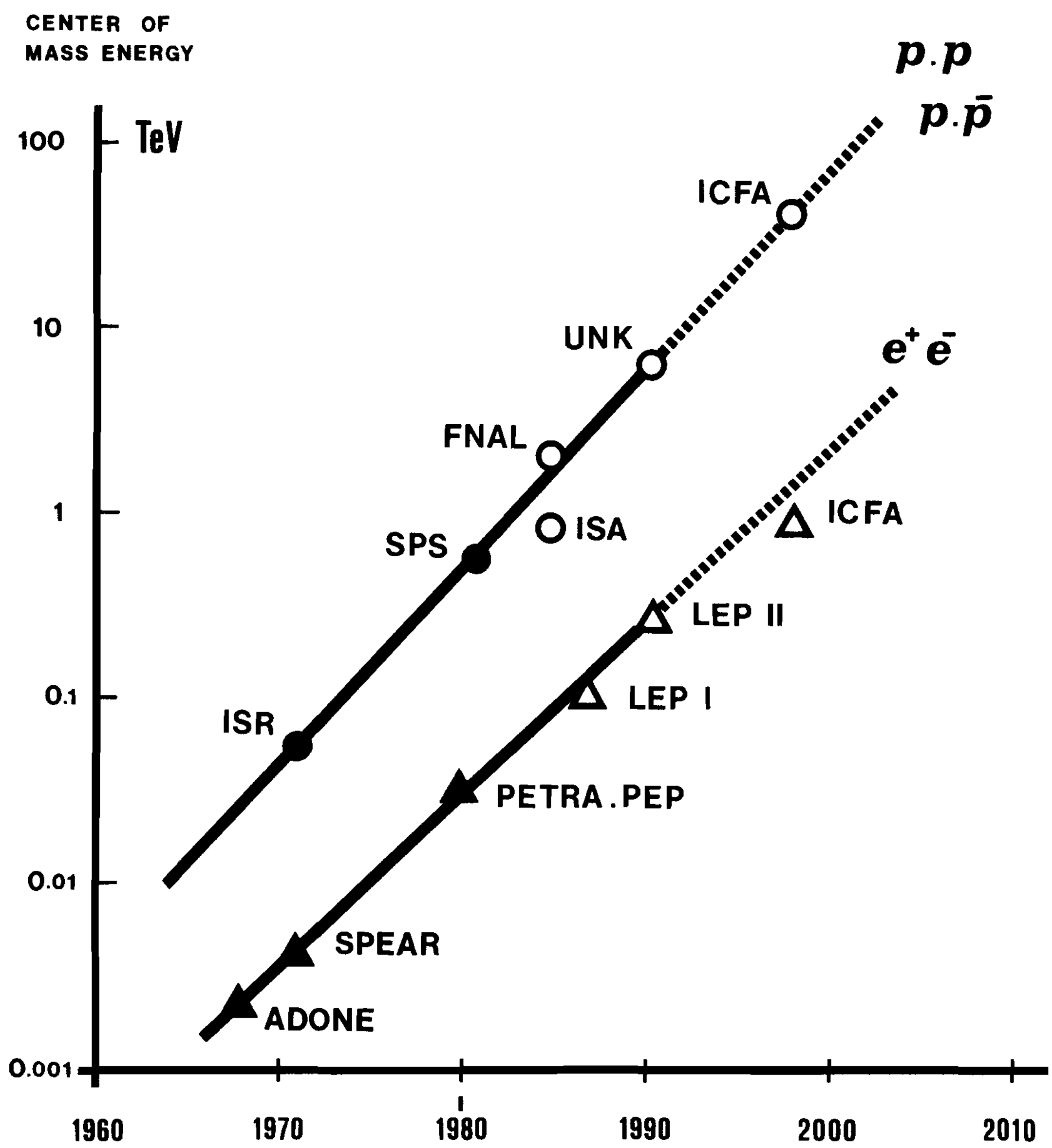

Fig. 1

DISCUSSION

Motz It is perhaps appropriate that an academic from Oxford should speak about this, but the situation in Britain is desperate. There are scores of talented young men who simply cannot get jobs and they know that with the freezing of jobs at universities at this moment there is very little chance that they will get into employment if they do engage in post-doctoral research in the fields that we are interested in. We need some organizational frame which a) provides research support for young people at universities, and b) sketches out possible future careers for such people. As you say in 10 years' time many of the people now in the field will reach retirement age and there will be room for young people. The cost of scholarships and fellowships would be chicken feed in comparison to the other costs of high energy physics.

Palmer I would like to speak up for the proposal that came from the United States : a body which would fund R and D in all countries, whether in universities or national labs, and speak against the proposal of trying to fund this research by some kind of collaboration between the national labs. There is a very natural, and quite proper, reluctance on the part of the directors of national labs to support activities which will not have an effect on that national lab for another 10 years. The objective of the U.S. proposal was somehow to break the grip that the lab directors, one might say, have on the funding of these far-out ideas. I do not understand why the Europeans and the Russians were so against the idea of funding real R and D which could go back to the universities and which would provide employment for the people there.

Soergel I think European laboratory directors did not vote against the possibility that universities should work on developing new ideas for acceleration; they did not vote against the idea that this work is supported by the large centres. They were concerned about creating new agencies. It was more this administrative point of view and not the basic principle. Indeed I think that European directors are quite in favour that this kind of work should go on, and quite positive towards having direct relations between the labs and the universities which would like to do this kind of work. Let me add some other points. I think that the really important requirement is that in the universities, some of the faculty are willing to put their interest into this kind of work. Those interested in high-energy physics are doing development work for detectors, or analysis work, and the development of accelerators has dropped to the point Sir John has described. I think this can be only reversed if university faculty members are willing to transfer their own interest towards the development of accelerating technologies, and I'm certain that DESY will support this kind of work in German universities and perhaps elsewhere. I am also quite sure that CERN would support this kind of work if European universities would get involved in development work for new acceleration. That is how I understood the discussion which went on in Paris at the conference. So these universities should gain more because young people alone can't do it; I believe the faculty also can gain as much from such work as from development of detectors.

Mulvey I agree with Soergel. The objection that was seen by people from Europe, in particular, was to the setting up of a new international agency with its own budgeting, we felt that you would just get bogged down in that exercise. Moreover, we did not see that, at least in Europe, it would bring any new funds into the exercise, it would only impede us in the ways in which we might try to use those existing funds. The proposal which resulted from the discussions puts the prime responsibility into the regions, and I believe that is where it really lies first. We must, within our own regions, make the necessary effort to get the funds and attract the interest for research on new accelerator technologies. If in that context we can point to the willingness of the laboratories and organizations in the different regions to participate coherently in some part of the research on the development of new techniques for particle acceleration, I think that it helps us obtain support within our regions and represents a first step to the more formal collaboration our U.S. colleagues would like eventually to see. But we believe one has to do this in steps and do things that we think are realistic first, rather than trying to leap immediately into a new super-international organization.

Thresher Let me just say one thing about the situation as I see it. What we need are proposals. Certainly within the UK, the mechanism exists to fund research in accelerator development. What we have not had, are people coming forward with ideas to do that work. There is absolutely no problem getting the funding, we are not talking about a large sum of money compared with the sums of money that go into the development of detectors and equipment and so on. I think we come back to the comment that was made right at the very beginning that if you are going to push forward ideas to do development work, you actually do need a few young people to push them along, and somehow I think we have got to break this barrier. I think it does not matter whether these ideas come from national labs or they come from universities. What we want are the proposals, and I think when we have got that we will go ahead.

Amaldi I can make a comment on one of the problems we face, as seen by one who has been involved in administration, over the past two years, of the fellowship and associate programme at CERN. We have seen this problem very clearly: we do not get good candidates for fellowships with our accelerator division. Not only for working on the present accelerators, but even when we could offer them some interesting work on new developments. And the rate of fellowship applications from member states, and for research - associates from member states and also from non-member states, is going down. In the last year we have done a lot to try to improve this situation, and a positive thing that we can say today is that we shall set up a group of people at CERN who will try to stimulate a greater interest in this work. The Director General of CERN has already agreed that a few posts, say 4 or 5 per year could be available for Fellows to work on accelerator R & D. If you know professors who want their students to do some research on a topic related to something which is in progress at CERN, we may be able to support them there as fellows or associates and certainly leave them more freedom than we normally do to go back and forth so that there is a link between CERN and the universities.

Von Dardel   I would like to draw attention to another remark made by Sir John, namely that over the last decade a number of the smaller laboratories have been losing their role as accelerator laboratories. I think that this is a very unfortunate development and I believe that in a way these smaller accelerator laboratories are perhaps an even better source for new ideas than the universities. They have a lot of previous experience, they have workshops and they have the experts which often the universities do not have, and I would like to say that the smaller laboratories could be fertile ground for the new ideas.

Winterberg   As an outsider I would like to suggest that you may have a public relations problem. Go into any pub or restaurant and ask how many people have heard about CERN. Maybe if you are lucky one will raise his hand. Space research people also ask for billions of dollars but they make an enormous public relations campaign to get the funds. I think in order for you to get the money, you must start organizing a very large public campaign to get more support for this kind of work, because otherwise you will never get the billions of dollars you would like to have for the accelerators.

Tajima   As someone from the plasma physics community I would like to say I believe there are a large number of plasma physicists in the United States who would be interested in the problems of particle acceleration by these new methods, and I see no reason why this would not be so in Europe also. There are a number of large, national laboratories where these advanced technologies are being developed and where there might be considerable interest in your problems.

Joshi   Perhaps some of you are not aware of even what modest scale experiments to test these so-called new ideas might cost, and let me take as an example the idea of the beat wave accelerator. I suspect a proof of principle experiment which will lead to demonstration of the fact that you can get gradients of something like 10 GeV per metre, so we are really talking about at least a quantum jump in the fields, and to show that we can get particles up to say, a half a GeV or so, are likely to need a steady level of funding over something like three years, of up to $400-500 K a year, which is not actually a lot of money, but is not peanuts either.

Morpurgo As Sir John has said, there is not much teaching of these topics in universities anymore and that is natural because there are no machines in universities either. So I believe, as Amaldi has said, we need an 'accelerator school', lasting at least several weeks in one of the large laborators. The student fees should be paid and at the end those who are best might be selected to continue in research.

Johnsen The CERN directorate has asked me to look into the possibility of organising something like an accelerator school, and we shall hold discussions with member states institutions on how this should be done. I would also like to come back to one of the earlier points. I have recently spent some time on the other side of the Atlantic and would like to comment on what I think is behind what the Americans have brought up. Namely, a few years ago, a panel chaired by Mory Tigner considered the needs for accelerator research in America. What came out was that there was a need for more research than was taking place at that moment. The sums involved were not very large, definately so small that there would be no difficulty for the laboratory directors to divert that kind of money if they decided to do so. This was several years ago and no, or very little money, that I know of, has been diverted into this research and I think that is the reason for the views expressed by Bob Palmer earlier. I do not quite agree that we are lacking ideas. I think there are ideas but we are lacking encouragement for people getting into this field, and I would like to bring up another of the points made before. Some of us are so old that we think a little about what we do, and would we recommend to our sons or daughters to enter this field? Honestly, when you hear that CERN has a full freeze on recruitment into the accelerator field at this moment, and you hear that they will build LEP without any recruitment, and you hear that HERA will be built without any recruitment at DESY, why should our sons and daughters go into accelerator physics? So. we should not forget that aspect of our problems and I think that that is a little behind what Ugo Amaldi was saying. He was discouraged that he had not had enough response, but you cannot expect it. It has a fairly simple solution, appoint young staff already now in anticipation of these who will leave in the future. We do need some of the best young people now to spend a few years on accelerator research, both basic and medium term.

Martin I think that one has to face up to the basic problem of why should young people go into the field of accelerator physics, when in fact there are not any jobs. It is clear that the number of accelerator centres has been reduced rather drastically over the last 20 years, because there used to be a lot of universities in the business and now there are very few. If you want to continue high-energy physics at very, very high energies, you go to one or two world centres, and that is it. Now, the whole development of accelerators to a very high level has been supported by high energy physics and people who support this understand that development and new ideas are constantly needed. These ideas have also inspired new approaches to research in lots of other areas, in solid-state science, and many, many other areas where there have been applications. But in these other fields they do not understand that if you build an accelerator for a facility, like a neutron source, that you must continue to develop that facility, you cannot just operate it. The synchrotron radiation facilities are extremely important for many areas of science; the neutron sources are very important; there are awfully good ideas in medical accelerators but they have not been fully exploited because the medical people will not pay for the development of accelerators. Their usual practice is to say that some company develops an instrument and they will pay for the clinical trials but not for the development of that instrument. My favourite subject is the energy-related applications of accelerators, in fusion, and I think that this is going to be very important but it is not clear that there will be support for this or not. So how do you resolve these problems? I looked at this a few years ago, and said that the future of accelerator research has got to be in the commercial area. I saw two possibilities: one is the application of medical accelerators, and the other is energy related applications in heavy-ion fusion or in plasma physics. So what I would suggest to this group is that one should consider first not whether you can build 20 TeV or 100 TeV machines, but whether you can build a 200 MeV machine for medical application, with this new principle. It does not matter that it is going to cost a little bit more; it is going to be some exciting development if it works, and if the model works at 200 MeV, we will then take it to 20 TeV. You will then have all the answers about the credibility of all these kinds of things, and the low energy models might have useful applications and interest a lot of people. And if you get into the business of my little proton machines for medical applications that will go into every hospital, then universities can start teaching accelerator physics; and if you ever make a heavy-ion fusion machine go, well this is the nucleus of the future technology of the world.

Adams I remember that is what Budke used to do at Novosibirsk!

Martin That is right!

Reiser I fully concur with Ron Martin and I would just like to add a few comments from the university prospective. What you are talking about is R and D - Research and Development - and at universities, if you are interested in attracting young people to do a Ph.D thesis, say in physics or electrical engineering, you cannot attract people with D, that is, development in technology, or development in detectors, but you can attract them with exciting new ideas, R - for research. So I am suggesting the important thing, in the interest of the later development work in the big laboratories and in the interest of teaching these topics, is to get research going in the universities. Don't judge these ideas only on the basis 'will they lead to a 20 TeV or 100 TeV accelerator' but on whether they are good topics for research, can stimulate interest and get young physicists into the field. Whether it is beat wave or any other idea, it will also develop interaction between accelerator physics and plasma physics and laser physics etc. And as Martin said, even if these ideas don't lead to high energies they might be commercially applicable. If we can get young people again entering the field, they will be the basis of future developments in the big laboratories.

Mulvey Returning to the remark made by Johnsen, the laboratory directors are clearly very powerful people but I think they are not completely isolated, or at least they should not be. If they are then there is something wrong with the way we form policy, and I think that we need to work harder in the community and to persuade our physicist colleagues that some significant proportion of the resources of high energy physics should be applied in research on these new techniques, in the big laboratories and also with their help in the universities. We cannot just blame the lab directors because they do not listen to us, we are obviously not arguing strongly enough with them. I think that there is some indication that we make a little bit of progress with CERN and a lot of us will hope that we will make some more progress, because it is not enough so far. But I wonder having said that if we could hear a remark from one or two of the other laboratory directors? (Unfortunately, Professor Schopper was unable to be present at this discussion). What is the position in some of the laboratories in France or in Italy? Do you see some prospect of putting some resources into these "test-of-principle" experiments? But let me first define our aims, which, at this meeting, are not to talk about the next round of accelerators; that is to say we are not talking about the accelerators which will be developed as extensions of present techniques even up to the end of this century. What we are trying to do is to foster the ideas that in time take us to a different energy region early in the next century. So it is no good if the lab directors say it has to be something which he can foresee using in 5 years' time.

Lehmann It is difficult to say. In high energy physics at Saclay for instance, we have a tradition since 30 years now, not to work in the field of accelerators. That was because the laboratory was created in a time when national machines were obsolete and we had to concentrate on CERN. So in that context no tradition exists and if some young physicists should like to come into this field it would be creating something from scratch. On the other hand, I notice that in other departments in Saclay, and the university nearby in France, there are certainly groups of physicists interested in plasma physics and in the other kinds of physics which have been discussed these few last days, and I am quite certain that a very large majority of them are completely unaware that accelerator physicists consider using these techniques either for future machines or just for development. It is right that we are trying to develop interest in these fields in several labs, and I think that if the conference proceedings could be ready in a clear and well presented form by the end of the year, or soon after, this would give a very good tool to interest those plasma physicists, and others, and to convince them that there is something interesting for all of us in these topics. I think that the question to know where should the funds come from, or where should the staff come from, are subsidiary questions.

Soergel I would like to add one more remark to what I said before. First of all, I believe that it is a problem that we must get the ideas and the interest in accelerator physics through to our young people, the students. Certainly what we could do from the high-energy community, from the large centres, is to help to develop that by for instance, lecturing in universities. Not just a kind of school which was proposed -- obviously a school is a very good idea -- but it is certainly not enough. I do not think that you get the young, undergraduate student attending these schools, but you need in universities a course on accelerator physics as one of the regular courses of the curriculum. I have some experience in that from Heidelberg. We had Schmelzer there as the professor for applied physics and we had a regular course in accelerator physics, and a regular student seminar, which was very much liked by the students and which has helped a lot in developing the heavy-ion accelerator. Many of the ideas which went into this accelerator were originally tried out in tutorial classes in the university long before the project was approved and long before it was known that it would be finally applied. It maybe that the universities do not have the professors which are willing or experienced to do that, and here maybe the large centres could give help and offer courses, regular courses in accelerator physics. Now, let me give you one example how it can work when a professor is interested in this kind of thing. We have a beautiful example of collaboration of this type with the University of Wuppertal where Professor Peal is interested in superconducting cavities. He has a very active group in the university with students very keen to try out new ideas with these nice techniques. At DESY we have a strong collaboration with him to develop structures which can be installed in Petra. You do not know if it will ever be applied in the future, maybe it will, but certainly we have a good collaboration, it stimulates both sides and DESY is helping to finance this project in Wuppertal. That is the kind of collaboration I think could be used as a model.

Adams Could I just comment on what you said about Peal and Wuppertal. The 2 major laboratories like CERN and DESY have got an important role to play. In fact when Peal really got interested and started to be able to contribute to the field in a big way was when we set up the little group at CERN under Picasso and the difficulty is that if you do not have a small nucleus in the big lab, it is a little difficult to encourage the universities to collaborate. Furthermore, you can offer them technological possibilities which they do not have in the universities. So there is some interaction, some role which the central lab I think has to play in this, if we want to bring in the universities in a more positive way.

Soergel Yes, and we have to help them do some work in their own laboratories, perhaps through finance or in other ways.

Nielsen I have some experience in giving lectures in accelerator physics at the University of Aarhus for a period of 10 years. There are two ways to go. The normal one is that you take a very few lectures and ten pages from Segrè or something like this and show big colour slides of big accelerators. That is nothing. If you really want a university to bring this up on a proper academic level, my experience is that it first ought to have a double lecture per week for a whole year, starting with fundamental things and ending up with accelerators on a really serious level. Finally I found out even this was too little, and I ought to have had one and a half years, one double lecture per week. And then another thing, it is very important that the students can use this for their final examinations, that they can get credit for it. Also you must have some people at university who teach it, but a contact with the centres can be an extremely important help.

Nation If there is a good job market open to Ph.D students when they finish there would be no difficulty in obtaining students. The problem, as I see it as an outsider, is the set of jobs that are available for accelerator physicists in high-energy physics is just too small to attract students. You must take a far broader attitude of what constitutes accelerator physics. You must bring in a much larger set of people if you are to attract students. They will go where there are the jobs.