

DEPARTMENT OF SCIENTIFIC AND INDUSTRIAL RESEARCH
and
NATIONAL INSTITUTE FOR RESEARCH IN NUCLEAR SCIENCE
JOINT CONSULTATIVE PANEL FOR NUCLEAR RESEARCH

Slightly edited transcript of the meeting held at Imperial College,
London, on Wednesday, 6th March, 1963.

Present: Sir John Cockcroft (Chairman)
 Dr. J. B. Adams
 Dr. A. Ashmore
 Professor W. E. Burcham
 Professor E. H. S. Burhop
 Professor C. C. Butler
 Professor G. F. Chew
 Professor B. H. Flowers
 Professor J. C. Gunn
 Mr. G. Hubbard
 Sir Harrie Massey
 Professor P. T. Matthews
 Sir Harry Melville
 Professor A. W. Merrison
 Mr. L. B. Mullett
 Dr. T. G. Pickavance
 Professor C. F. Powell
 Professor A. Salam
 Dr. G. H. Stafford
 Mr. R. St. John Walker

 Mr. I. A. Learmouth } Joint Secretaries
 Dr. J. A. V. Willis }

Cockcroft: Perhaps I should begin by summarising the reasons why this meeting has been called. A few members informed the Advisory Council for Scientific Policy about the discussions which are going on in C.E.R.N. about the plans for a new high energy machine and the Advisory Council, after some discussion, decided first that it would want to have the case for this machine prepared, the estimates of the cost and the energy of the machine, the question of staff requirements and also (a point specially raised by Professor Flowers who is on the Advisory Council), that any repercussions on the university programmes should also be considered. One couldn't consider the future requirements of C.E.R.N. without taking into account future requirements of the home university programme. So this meeting has been arranged as a preliminary to preparing a case and it will be necessary I think at the conclusion of this meeting to set up a small group as we have done in the past, to prepare the case.

Melville: I should just like to reinforce what Sir John has said. I think this is a point of real scientific policy to the country.

The A.C.S.P. provides the best forum for a discussion of matters of considerable significance and they, being a body with no special interest in any one field of science, have got to see the case for very large expenditure, such as this would be if it came about. They would be in a position then to advise the Minister. These things need so large an outlay that I should imagine they would go to the Cabinet for decision.

Cockcroft: One other point that I should mention is that in parallel with this study which we are doing, they have asked for similar studies in the general support of universities, and also in the support of industrial research - so that they may have a balanced view of future requirements and may compare one with the other and not just pay more for requirements for this field of science without looking at the other fields. All this will have to come to a head I think sometime before June - I don't know whether it is going to come up at the C.E.R.N. Council.

Melville: It might come to a head but whether a decision is taken is quite another matter.

Cockcroft: Well I do not suppose a decision will be taken before the end of the year at the very earliest.

Walker: I should like to mention what has been said at C.E.R.N. about timing. Weisskopf outlined the position to the Finance Committee of C.E.R.N. The paper from the Scientific Policy Committee will be considered by the C.E.R.N. Council in June. It should be circulated in May. I asked that papers should be circulated as soon as possible. Weisskopf's hope is that the Council of C.E.R.N. will agree in principle in June to consider a supplementary programme to assess the new high energy project, costing about £ $\frac{1}{4}$ million in 1964 and that this supplementary programme will be fully agreed in December, 1963. Firm commitments for storage rings and for the synchrotron might then be sought at the end of 1964 and the end of 1965 respectively.

Adams: Sir John, how does the report you have just been describing fit in with the report which the C.E.R.N. Accelerator Study Group was going to produce later this year, considering the P.S. machine and the storage rings and also the physics case for the new step forward? Are we in some danger of just repeating this work in a precis form?

Cockcroft: No. C.E.R.N. are setting the pace and bringing the matter forward for discussion in June. We need some briefing for this. It's a matter of timing.

Melville: Is it possible to give the A.C.S.P. further information about the cost and timing so that we could have their comment before the June C.E.R.N. meeting? The other complication is, as I said at the beginning, that we know the position in the universities is very tight. To go ahead with a thing like this, even with preliminary design studies, and forget about the dire straits of some of our academics would be a bad thing. We have to bring other requirements of universities and also technology together in such a form that people can see what the pattern is going to be in the next two or three years. And that won't be done before June. On the other hand the A.C.S.P. should have information in more detail, not only as regards the cost of the machines but the possible ways in which we might collaborate with other countries because it is quite clear that some of the smaller countries are going to be cut out by the size of the bill. It is that sort of thing the A.C.S.P. would like to know and it might help to form some views which would be useful at the C.E.R.N. meeting in June. But they would be only preliminary views.

Powell: Our report should reflect the particular views of British physicists.

Burhop: There will be available in April a recommendation from the so-called Amaldi Committee to the C.E.R.N. Scientific Policy Committee, with regard to the energy of the main project, namely the accelerator, which will include the cost and an estimate of the annual budget.

Melville: It will be difficult for the U.K. delegation to say anything at the C.E.R.N. Council unless we have the observations of the A.C.S.P. in May. The Amaldi Committee papers will be the best estimates that anyone can make about the cost. What we need to know is what the British nuclear physicists think should be the right policy for nuclear physics in the light of these estimates. Then the A.C.S.P. would consider this, taking into account the other obvious demands on budgets. So there is still a lot to be done by a British nuclear physics committee which we need to appoint with some speed.

Cockcroft: Perhaps we should now go on to the various presentations. John Adams is going to speak about recent European discussions.

ADAMS

The note you have here was written some time ago, I think round December, and I think the important thing is to note the

changes in views since this was written. I don't think the four points of policy which are laid out on the front page of it have changed in principle at all, all that I think happened is that the energy of the future P.S. machine that was put down here as around a 100 GeV I think if anything has crept up since that time. At least people have discussed the energy range 100-300. I don't think there is any change of attitude about the storage rings. Similarly, point (c) about trying to keep the pyramid in balance and not just to spend all the money internationally I think, if anything, is strengthened now. The last point I think is a bit optimistic in the sense that there is naturally a tendency to say that the next machine should be a national one or a European one and the one after that should be a world machine and I think this will probably go on until the Governments give up. In other words I think the next machine after the one that one is discussing will always be the world machine.

Of the views expressed on the second page, most I think have sunk in by now. At the time we were discussing it in December, both Puppi, de Rose and Francis Perrin were rather anxious to keep the energy around 100 GeV for no really scientific reasons except that Puppi had a physics point. His idea was that one shouldn't really go too far in energy because of the difficulty of assimilating the machine in the sort of physics and he pointed out the difficulties that C.E.R.N. had with 25 GeV. I think this is rather a weakish argument because before 25 the energy that was available in Europe was very low indeed. The step to 25 was a very big one. 25-100 or even 300 may not be so difficult as Puppi thought. Bannier brought up the point that Sir Harry just mentioned and I think it is a very important one. Several of the smaller member states might well find themselves unable to support a project if it was too big and Bannier's guess was 100 maybe, but 300-500 really that's out of their class. It was only a guess.

I tried to press for collaboration with America and with U.S.S.R. in various ways, all unsuccessfully, I might say. And then there was a rapid review of what the countries were going to do themselves and there wasn't really anything unknown there. Perhaps I spoke out of turn a bit when I said I thought the U.K. might well be contemplating something bigger in 1967. I am sure they will be. Whether they get it or not is another question. Anyway I thought it was worthwhile showing we were still alive. (Laughter) Then the Scandinavian plans. I think these finally came down to a joint P.S. machine.

Then there was Amaldi's report which I expect many of you have read. It wasn't so much a report as a collection of facts which he tried to gather together really in order to answer the question, were we all really being very greedy in science and in pure research, and the only way he found he could answer that question was to first get some idea of what the world thought it should spend on pure research on the one hand and then try to see what Europe as a unit of the world was planning to spend. Well, he got this figure of 0.2% of the National income, then he worked out 0.2% of the total European national income; this is on pure research. And then he thought of all the most expensive new things that have been discussed in the newspapers and other places, such as the 300 GeV P.S., storage rings, space research at a much more accelerated pace than at present, new radio astronomy telescopes - all the things you might expect to spend a large amount of money on and he couldn't get it anything like 0.2% of the national income and he concluded from that that there was really no danger that pure research was asking for too much of the National cake. Now whether his figures are right I don't know; I looked through some of these quite carefully and the ones I looked through were not far out. Whether the 0.2% is the right figure I don't know. It is a figure actually put out by N.A.T.O. originally. It is a tenth of the total research of the country, in other words, 2% is I think the total research figure. (Melville: 0.2% checks roughly with our University expenditure).

So, anyway, this was comforting to many people because I think when one is proposing new expensive pieces of apparatus for laboratories we always have a sneaking feeling we are absorbing too much of the tax-payers' money and occasionally it is very good to see that one is not really out of proportion at all. Then there were some costs for the machines that had been discussed; I don't think there was anything there.

The other point was the various discussions I had in Russia and in America on visits last year. Of course this is with the official side, not with the physicists. I have noticed both in Russia and in America quite a disparity between what you can call the official view point and the view of let us say the most senior and often quite weighty physicists. In the States for example the A.E.C. view as expressed by Glen Seaborg and Lee Hayworth and others in the A.E.C. seems to be a little different from the view for example of John Blewitt of the Brookhaven Group and of the Berkeley Group. This is probably not surprising. In Russia it seems to be also the same sort of thing, you have people like

Veksler who are certainly at variance in views with Emeleanov. I think Emeleanov for example is quite keen on collaboration even to the extent of collaborative facilities which is clearly not very welcome to some of the physicists in Russia who see that they would have to travel long distances to use the machines and have other reasons for not wanting them. I discussed with several of these people the "Troikatron" project and with the general feeling that it would be extremely difficult to get any agreement to build a common accelerator (and of course I needn't remind you here the idea was to build it in Europe, without being too Machiavellian) because I think that both America and Europe at this stage could afford to build one of these each. They may not want to build them but I think that they could afford to build them, one each. So inevitably the "Troikatron" as it is called goes to the next energy range which is a 1,000 GeV. There was quite a bit of discussion I think originated by I. Rabi about a joint U.S.A./European project. What is felt about this in America at the moment I don't really know. When I was there which was in November/December last year, this was in the ascendant and Rabi was quite keen on this idea on the basis that I sketched down here (in the paper). But, clearly, it suffers the drawback that there isn't a balance between storage rings and a new P.S. machine even at 100 GeV energy and although in the long term one could say that one builds facilities jointly and therefore one could achieve the balance after a number of years, it would be after a decade or so not in the next few years. So there's this unbalance problem and I think all these things will not be resorted to unless Europe as a whole says that it won't or can't afford or doesn't want to build the new facilities, storage rings and a P.S. machine, and America does ditto. I mean I think you only get this type of collaboration on building facilities and jointly owning them if the individual continents have in fact given up. What the stage is after the world machine I don't know. I suppose that will be the end.

Cockcroft: There is just one point I should like to make on this 0.2%. It's a matter of accounting. In the 0.2% we charge half our total university expenditure. Different countries may have different ways of reckoning.

Adams: I am sure that the figures are not correct to a factor of 2. The fact that we did not come anywhere near 0.2% seemed to me significant. (In a general discussion it was stated that the Gross National Product in the U.K. is £24,000 million, and that

university research costs £30 million, and pure research in Government establishments, the A.E.A. etc., although hard to estimate accurately, is about £40 million. If the total expenditure on pure research is thus £30 million + £40 million this is 0.29% of the G.N.P. The figures are based upon the A.C.S.P. Annual Report for 1961-62).

Massey: Let me get the situation quite clear. In the U.K. we spend more than 0.2% on pure research?

Melville: There is no serious argument about the figure of £30 million p.a. in universities. There is some question what fraction of the additional £40 million which is spent by the Government in the civil field is fundamental.

Massey: The argument of Amaldi doesn't help us at all?

Adams: It helps for setting the sights of Europe as a whole. Many C.E.R.N. countries spend relatively less on nuclear physics at home than we do, so on the average there is quite a lot of lee-way.

Pickavance: In the restricted field of Amaldi's enquiry namely high energy physics with accelerators above the pion threshold, there was no striking differences between the U.K., France, Germany and Italy who were each spending domestically several times their contribution to C.E.R.N. Germany was the lowest, about $2\frac{1}{2}$ times, the others 3 to $3\frac{1}{2}$. The smaller countries of C.E.R.N. were way down. Some spent nothing at all. So as far as high energy physics in U.K., France, Germany and Italy are concerned there's no great disparity in terms of Gross National Product because the C.E.R.N. contributions are based on that.

Massey: This may be a double-edged weapon.

Pickavance: I don't think it is very useful one way or the other because of different accounting procedures.

Chew: I think there is another potential criticism which is not immediately financial but has to do with the fact that if you continue to set up very big experimental facilities it automatically draws into this field a high proportion of scientific talent. The other areas of science are beginning to complain about this, the fact that the students who come up automatically get dragged into high energy physics because of the big facilities.

Cockcroft: Pickavance is going to talk about that.

Adams: I do agree with that point.

Massey: Yes, I think this is an important point. You have to

consider whether or not certain other branches of physics, nuclear physics particularly as distinct from high energy physics, may not be getting the benefit. It is not necessarily just the high energy side which is getting a very large number of physicists.

Adams: I think also one shouldn't fall into the trap of equating all sciences, giving the same factor of merit to all research. It just happens that in high energy physics we have got ourselves into a situation where we could really make a world contribution, in Europe and nationally due to C.E.R.N. and the efforts which have been made in this country. Now it's not right to throw that away in order to bring up some other research subject, so that you end up sort of mediocre in all. So I think one has to be a little careful when cutting the cake.

Cockcroft: Well this is what the A.C.S.P. are trying to do. To look at the other demands and see how much we can squeeze out of the Government as a whole.

Powell: There's nothing sacrosanct about 0.2%?

Melville: No. It's bound to go up because the expenditure on research is rising much faster than the G.N.P.

Massey: There is one point I should also like to mention in relation to this question of manpower and resources. It seems to me you have to be a bit careful about plugging this pyramid idea because you can go on plugging this to enlarge practically any branch of physics as important background training for high-energy physics. I think one has to be a bit careful that one doesn't get oneself in the position of enlarging the amount of money which goes to all branches of nuclear physics, with the justification that this is necessary in order to maintain a level of high energy physics. There is almost a qualitative difference between low energy nuclear physics and high energy physics. One has to be careful one isn't overestimating the expenditure required in connection with high energy physics by having a pyramid too broadly based at the bottom.

Pickavance: Are you saying you get into trouble if the base of the pyramid includes nuclear structure work? I am sure this is right. It's best when talking about high energy physics in Europe to restrict the subject to high energy physics. This has been done in the States for years.

Massey: This is quite important. Our total national expenditure in the nuclear physics plus high energy physics field, is quite big; we must be careful I think to keep the two to some extent separable.

Pickavance: I think we have to consider them separately, though they are linked in certain ways, but not the same.

Burcham: As far as one can guess, at this particular point, the next steps in nuclear structure are going to be quite expensive. A nuclear structure machine might take several million pounds. That is not negligible.

Pickavance: It's not negligible - it's different.

Burcham: I don't think it's all that different. It should be taken into account.

Massey: I would think one has to be very careful about regarding this as comparable to high energy physics. I think they are now getting very far apart.

Pickavance: I think the point is that you could usefully do one without doing the other at all.

Burcham: I disagree with this statement. It may be true in the financial sense, but not in a physical sense.

Massey: I think it is in a physical sense because nuclear structure is a subject that depends rather more on the kind of physics which you use in dealing with say solid state. You are dealing with large numbers of particles, interactions of this kind, and it is not concerned with investigation of basic fundamental laws. It isn't the application of known laws at all, and in that way it is quite different from a subject where you are dealing with application of known laws. You don't know them all of course, I admit, but even if you did know them in detail it would be a branch of physics comparable with say solid state or rather more that kind of physics. If high energy reaches a stage where it is again a matter of dealing with complicated situations, with lots of particles involved and so on, and is really applying laws, then I would say essentially, it has lost its driving force. I think there is a vital difference between these two and we would make a mistake by not making a distinction between them.

Burcham: I could answer that Mr. Chairman but I don't think this is the moment. However, I entirely disagree with what Massey says.

Cockcroft: In any case the Advisory Council are asking for a long-term view on expenditure on nuclear structure work as well as on high-energy physics.

Flowers: There is one important point here. I think I am on Burcham's side. The important thing is that one cannot afford high energy physics machines in universities all over the place,

which are training people. So the question is how are you going to train a high energy physicist of the future. Here I think Massey is quite wrong. Nuclear physics is still the closest thing there is to high energy physics. A very good training ground. And although I agree with him in a philosophical sense techniques used are so similar that to drag in solid state physics as a comparison is ludicrous. The techniques are not similar at all.

Massey: I think there is danger of thinking physics is all technique. It is physics. I think we ought to classify on that basis.

Flowers: Yes, but you still have to train people to do high energy physics.

Adams: I think also one should be a bit careful about saying you are training people for high energy physics - I mean surely universities train physicists who should be able to do different types of physics. You have to give them first researches to do and this is conditioned by what you can do in the university.

Merrison: The arguments about the pyramid are not so much concerned with training people but with making sure that whatever the physics atmosphere in the country is, it is sufficiently lively to make sure there is a flow of people coming backwards and forwards. We have seen all sorts of people coming into Liverpool and being trained in high energy physics, from various backgrounds. It is much more important what a man is than what his background has been.

Massey: The thing I don't want to see is vested interest in low energy physics. I think it fair to say that one machine was built for low energy physics because the people in the department did not want to change to high energy; this is an attitude that seems to me to be wrong.

Burcham: I claim that some low energy physics is quite closely related to high energy physics.

Massey: Yes, but this is low energy physics of a different sort. I wouldn't say for a moment that the Cavendish Laboratory in its early days didn't discover a fundamental basis. It's not that that I'm arguing about.

Burcham: Isotopic spin, weak interactions - all these are mixed up with high energy physics.

Massey: Quite, and I would say if you study the interactions between nucleons and go as far as few nucleon interactions perhaps: I would say these are fundamental. But I am not sure that we can

afford such a large facility for some of the low-energy work. I am not saying that polarisation phenomena in nucleon nucleon collisions for example is not fundamental. I am not taking energy as my criterion.

Cockcroft: I think that for our present purposes we need to say something in the report about future university equipment other than that already envisaged in the D.S.I.R. plans. We have a few machines provided for the next few years but it is a question of whether there is any more expensive equipment coming along.

Perhaps we should move on to hear what Professor Salam has to talk about.

SALAM

When I was asked to speak on the usefulness to physics of very high energy machines, I thought I should be speaking in a very placid atmosphere. (Laughter).

I think it is by and large true that in the past whenever a theorist has been asked to provide a justification for a higher energy accelerator, he has had to recourse the same type of hopefulness as Mr. Micawber*. He has usually said, "I hope something new will turn up". I do not know what the theorists who were consulted at the beginning of the Bevatron said but I remember the study group, of which I was part, when C.E.R.N.'s inception was near. The list of experiments Prentki, D'Espagnat and I were responsible for was the most ludicrous list in the light of what has actually been found to be important.

But today the situation is different. The subject is absolutely bursting out of its seams. Even a theorist can now provide some reasonable, well-defined, concrete ideas - things which the present generation of accelerators are just incapable of doing, things which must be done and studied if ever the fundamental laws of physics are to be discovered. We are not taking a leap into the complete dark in pressing strongly for a higher energy accelerator. But before I say what I think are the concrete reasons for newer accelerators, let me, with the greatest approval, quote a magnificent remark made by Van Hove at the recent C.E.R.N. Conference.

*In parenthesis I may remind you of the famous phrase of Churchill who said Treasury people are inverted Micawbers "wating for something to turn down".

This remark sums up beautifully the general philosophy of what we are asking for. Van Hove said our interest in High Energy Physics is on the Frontier of the Science called Physics. In terms of distance, the two frontiers of physics lie respectively at distances of the order of 10^{-14} cms on one side and 10^{+27} cms at the other. The frontier at 10^{-14} cms is the frontier of High Energy Physics, this is the smallest distance we can probe to using our highest energy particles. 10^{-27} is the distance of the most far off galaxy we know of. We can state laws of physics which hold for intermediate distances. These laws have the power of prediction. But for distances shorter than 10^{-14} cms and for distances larger than 10^{+27} cms we do not know anything. In so far as High Energy Physics is engaged in answering the challenge of one of the two frontiers, mankind will for ever engage in this. To leave it off just at the moment when the race - the tempo - is getting hottest would be a tragedy in terms of human history for which future generations will never forgive us. And particularly for a country which produced Newton, Maxwell, Rutherford and Dirac, this can not even be contemplated.

Now what are the specific items whose glimpse - no more than a peep - the present generation of accelerators has shown us, and for whose real elucidation we must wait for the next generation of accelerators. To my mind, there are two major topics:-

- (1) Asymptotic behaviour of cross-sections.
- (2) The interrelationship - the unity - of strong, electromagnetic and weak interactions.

I do not have to elaborate very much on the asymptotic behaviour of cross-sections. Up until and including the time when Pomeranchuk's theorem was stated this was a barren subject. After all how could one feel excited about a flat curve. But a new life came into it with Regge ideas. I know these ideas are under an eclipse today. But let us make no mistake about it. In a modified form, these do represent a real breakthrough. There will soon be a more decent version produced - a version which will explain all the scattering of primaries and secondaries till 30 BeV. And then there will be a cry for Higher Energy both for primary and secondary particles to verify the modified Regge theories. In fact my worry is, what are we going to do till 1972 when the next machine is being promised?

The second topic is to my mind the still deeper of the two. I am going to assume that the present generation of accelerators working flat out will have finally, before 9 years are out, fixed

the symmetry properties of the strong, the e.m. and the weak interactions individually. For strong interactions we shall perhaps have found that the unitary symmetry is right - we shall or shall not have found all the higher multiplets. We shall perhaps have also found that electromagnetic interaction does not break down. We shall perhaps know exactly the status of the Universal Fermi Interactions and the secret of the Anomalous β decays of the strange particles. We shall perhaps have solved the μ mystery one way or another - either the particle is nothing but a heavy electron or that it has a strong interaction of some sort, and joins the strongly interacting gang.

But even if all this gets determined, the basic question remains; is there any fundamental correlation between the three types of interactions besides their role in breaking each other's symmetry? We have just begun to see a new synthesis appearing among these interactions. This synthesis is the fundamental role of the vector particles in mediating these interactions:

	Strong interactions mediated by	ρ, ω, K^*
E. M.	"	" γ
Weak	"	" perhaps W^+, W^0

The common feature of all three interactions is the determination of the vector form factor:

To find the strong Form Factor.	Perhaps the present machines can do this.
" " " E.M. " "	" " " "
" " " Weak " "	No present machine can even begin to attempt this.

The importance of the Weak Form Factor can not be overstressed. The E.M. Form Factor has had a basic role in modifying our concepts of the nucleon structure. It needed a colossal effort on part of Hofstadter - a fully justifiable effort. The Weak Form Factor and its comparison with E.M. and strong Form Factors will open up the synthesis between the three types of interactions.

Let us make no mistake about it. Even if nothing startlingly new turns up to justify the building up of a 300 BeV accelerator, there is enough which is foreseeable that must be done before a meaningful probing of the Frontier starts. Do not forget the history of the Lamb Shift - no one would have believed the outrageous theory of renormalization if experiment had not confirmed it. The same thing happened at the time of parity theory. If experiment had not confirmed the breakdown of parity, who would have believed in the

work of Yang and Lee? The correct theory is seldom accepted right away. Usually it is too outrageous; only experiment ultimately can decide it. Even if nothing qualitatively new is discovered, through a newer accelerator, just to decide finally the ultimate form of the fundamental Laws of Inanimate Nature, once for all is a task which deserves the highest priority.

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Powell: I agree very much with the important point which Salam mentioned that we cannot see at the present time any very tangible advantage coming up. It seems to me a very important point, because I heard it recently said by an eminent authority that the only thing which has come out of Rutherford's experiments is the fact that the cost of electricity is now greater than it would otherwise have been. This kind of approach has to be met. In making a case one has to meet this kind of suggestion and point out for example the enormous consequences from the Rutherford experiments on our whole system of ideas. It is quite clear that we cannot see an industry at the moment which will start from this subject, but we can certainly anticipate a completely revolutionary effect upon our basic philosophy and that affects every aspect of our thinking and in the long run has an enormous bearing on whole kinds of new industries of which we can't yet anticipate so I think it is a long term investment but the advantages are likely to be priceless in the long run. I think this is a point that ought to be clearly and firmly made, in the various presentations of the case.

Merrison: The important point surely is that this is a question you can't ask, any more than you could have asked Plank what experiments on heat radiation would lead to. But it could be possible to revolutionise what we know about the world and how things inter-act.

Powell: You say it is a question that can't be asked but in fact it is always asked, what practical advantages flow in the near future from this activity.

Walker: I think the question people on the financial side ask is for what purpose do you require this, rather than is there a practical application. It is accepted that fundamental research is not linked to practical applications.

Cockcroft: You have to make intelligible to the laymen what the purpose is.

Salam: It is the frontier.

Melville: Is it not another problem? - of convincing other scientists - biologists for example - who clamour for more. Biology

is not progressing as quickly as nuclear physics because they are not ready for spending the money and I think it important to realise that it's not just a Treasury or political matter. Other scientists have to be convinced. If they consider themselves starved of facilities in other branches of physics, chemistry, biology, whatever it is, then they are not going to accept these arguments without a good deal of questioning so I don't think it quite so bad as translating these abstruse ideas into language which political people and financial people understand - it is for other scientists to understand. If they are convinced then you have gained the point. The A.C.S.P. are a body of scientists who if convinced this is right will strongly advise Ministers, and Ministers cannot afford to ignore their advice.

Massey: I am sure this is an extremely important point. Perhaps I move in as many non-nuclear circles as most people, and I sense very strongly a great growth in the last six months or so of a certain reaction against nuclear physics. They think nuclear physics is getting too much. This is what I hear in non-nuclear circles. And I think it is a very important matter, and this is increasingly true in anything that requires large facilities, that you've got to convince quite a large fraction of people in other branches of science. I feel that this is one of the reasons why the argument that you give must not be one which applies also to the subject which the other people are dealing with, and I think you could make a special case for high energy physics on the frontier basis as compared with many other things. This is where I think you might be able to get this over to a reasonable scientist. If you try to argue on any other basis you find that it's very much more difficult and this is where I think we should be very careful as far as tactics is concerned.

Salam: Therefore we should drop the word nuclear, is that your suggestion?

Massey: I think we should emphasise the high energy side.

Salam: And I think the particle aspect should be emphasised much more.

Massey: There may have been misunderstanding of what I said before. What I said was not necessarily that low energy nuclear physics was not fundamental but that many parts of it were not fundamental, and that we must be careful that we are not applying the fundamental argument to the non-fundamental part. This is I think very serious and important and many non-nuclear physicists are aware of this.

Adams: I agree very much with the argument that the real opposition in Britain is the other scientists and not the Treasury. I am sure if there was a unanimity of views amongst scientists this would be accepted for all the good reasons that Sir Harry mentioned. Then I think our real problem here is to convince laymen that it is worth this sort of amount of money, and I think your sort of arguments based on the understanding of forces is much more easy to understand than some of the ones that were proposed at C.E.R.N. at the last meeting. I think it has got to be put in a fundamental way of that kind, and not as some detailed statement that you want to do neutrino physics and then stop there. On the question of what has come out in the past in elementary particle physics, you can use the argument (again perhaps a double-edged one) that there is the whole electronic industry and the nucleonic industry. If you look at the capital involved in those industries, you find that the cost of all high energy and elementary particle research is a fraction of the capital involved in these industries - if you want that type of argument. I personally think it is the wrong one. Something has come out of elementary particle physics which is practical, has in fact revolutionised those industries quite apart from ideas but I think at the moment we should emphasise the ideas.

Powell: I didn't want to emphasise this question. I was pulled up very abruptly by some conversations where it was clear to me that eminent people really didn't understand the role of high energy physics and its place in the development of our understanding of nature and the consequences which flow from this and it seemed to me that in any case that was made there should be an element of discussion of this because I think it can be properly met without great elaboration along the lines that Salam has indicated.

Cockcroft: The presentation should be such as to convince chemists and so on. And the other physicists.

Ashmore: My experience in a physics department with a wide range of interests is that physicists outside nuclear physics are very interested in elementary particles, and also in nuclear structure, so I don't think they will be at all difficult to convince of the value of carrying on with this work.

Massey: The real trouble at the moment is that now other branches of physics and other sciences are beginning to be attuned to the idea of spending much more money, they've got projects in hand, costing more money. The pressure therefore is greater. Scientists in other fields are beginning to think that they could use much more money more effectively, and naturally they think, "Why shouldn't we get it?"

Merrison: I think this question of sharing out the money is obviously the basic motive of these people.

Chew: Not necessarily, I think. I heard the argument presented that high energy physics has been given an unfair amount of prestige and therefore the students coming out go into that field.

Merrison: I do agree with you. Yes, that's another argument entirely, I think the most solid point talked about this morning is this question of how you distribute the manpower; the financing, I suppose, is in a certain sense secondary. But talking about the financing it is said that if the money goes this way it will not go that way, this is the most worrying thing. I would have thought there was very little evidence to show that if one project gets a lot of money, other projects do not get it. It's a much better argument to say, "Look he's got all that money to do this, why can't I have a little money to do that?" - Much more forceful.

Massey: I agree entirely with this viewpoint. If you go ahead and get the new projects established and raise general level of expenditure, that gets everybody interested in science. But this unfortunately is not the view that most people take. You have only got to look at minutes of discussions of this procedure to see a certain envy expressed about other subjects of which you can guess the nature and this again is the same attitude involved and I think you have to take this as one of the facts of life, even though I agree that the true situation is different.

Flowers: Is this really right? I'm just thinking about the solid state industry and research for that matter in this country. It is in a clearly deplorable state compared with the United States. Isn't this largely due to the enormous amount of effort put into Atomic Energy - in this country just after the war which stopped people at that time from going into solid state.

Stafford: The danger of opposition is that you eventually don't do anything at all.

Adams: It's difficult to get the other fellow to see it that way. But most of the trouble is not that there are other projects ready but the attitude is "in a year's time or two year's time we are going to need more money and if it now goes to nuclear physics we will not get it". It's not as if you can write down a list of projects in other branches of science at this moment. It is a sort of fear that money won't be there when it's needed.

Melville: That's another point. I think the worry is "suppose the expenditure in high energy physics goes from £10 million per annum

to £50 million per annum" - which is quite a possible figure if you put down the list of requirements both international and national. These people look at it in this way: at the moment the expenditure by industry on research is about £200 million a year and will go up. That's on their own account; they've got to make profits to spend it, so it is research to benefit the economy and make it possible to increase expenditure in academic and other fields. The worry is whether, for example, the Government might have to decide between nuclear physics and expenditure of considerable sums of money by private industry to make the economy a bit more healthy. This is the difficulty, and you see the sums are becoming comparable whereas before, even at £10 million a year it wasn't really a significant point. And if faced with that challenge then scientists both pure and applied have got to decide whether to put their money into speculative projects like this which may have big dividends in the future or to go for a shorter term policy to try and do what the general economic thought in the country conforms to. I think that's the worry at the moment.

Burhop: I think that what Sir Harry has just said illustrates the importance of as soon as possible putting on paper exactly what is being considered because the figure you mentioned of £50 million a year is so far out from any sort of figure which anybody was thinking about.

Melville: Are you sure?

Burhop: We have recently done a study of what would be the likely expenditure of a 300 GeV accelerator, nine years after its completion. This would be in 1980-1981. Now you may think this is impossible to foresee but this is made with most elaborate and almost fanciful allowances. There are 15 beams of different kinds and it has been worked out in some detail, but the highest figure which is arrived at is 400 million S/F per year. If Britain contributes $\frac{1}{4}$ of this, that's about £8 million a year. So I think that these figures are very large figures, but until we get it on paper what the actual figures are, people may be a little put off because you guess a figure which is several times the actual one.

Melville: On the other hand, people can easily point to what happened to C.E.R.N. estimates before. I have to go to the Public Accounts Committee in the next 2 or 3 weeks, and someone will no doubt say this is all very well, you start off with a modest demand and look what happens. The only way I can answer is to say the P.S. as such came out just about right but all these fellows started having new ideas about bubble chambers and that broke the price and

you can't hold back science. That's all I can say to them.

Massey: Another point I would like to make quickly and that is one other thing one has got to counter in the views of people outside nuclear physics, is the idea that anything very big like this is done for prestige reasons only.

Merrison: One thing I would like to say criticising Sir Harry's point in a sense is that I don't think you have really got the choice between speculative and useful research, I think that you have to make some sort of decision as to how much of your effort is going into speculative or useful research, it's not a case of deciding between one and the other.

Melville: It's not just a case of people asking for the money. We hear that if in the engineering faculties of universities you had some of the bright physicists that now go into nuclear physics, the effect on British engineering would be pretty big.

Merrison: I'm afraid you wouldn't have them. We read what Lord Hailsham said and we know very well that our people go and do bright things abroad if we won't let them do bright things here.

Melville: They might do bright things in engineering.

Powell: I understand Sir Harry's point very well and I think that the people complain about the relative prestige attached to high energy nuclear physics and solid state physics for example, and the allocation of manpower. But they don't really take account of the fact that although an effect would be produced by an allocation of funds, the very brightest of the young people are attracted by the most demanding and fundamental subject and they won't go into engineering. They will in fact go to places where they can assume this study.

Cockcroft: Well we have two more contributions to come, and we should now hear Professor Chew.

CHEW

I'm going to give a point of view which is at the same time very conservative, but in another sense as you'll see it's perhaps rather radical. I share with Salam his complete enthusiasm about the general programme but I have rather different ideas about the concrete prospects that may come out of a step-up in energy by a factor of 4 to 10 which is the thing we are talking about. By the way, I should say for those of you who have attended the Royal Society meeting, that if you try to understand what I'm going to say in the context of what Weisskopf and Van Hove said, you may have great difficulty! My point of view is just orthogonal to the points

of view that they expressed.

To take the conservative attitude, let me point out that in the past when major steps in energy were achieved, one did have a characteristic energy of some kind that one was trying to surpass. The first nuclear accelerators were trying to get over the threshold for nuclear disintegration, of the order of millions of volts. The second big step was to get over the threshold for producing the lightest strongly-interacting particles, which was the pi-meson. The existence of these two energies was known in advance in a general way: it was known that some threshold was being surpassed, and the same thing was true when the Bevatron was built: it was known of course that the mass of the nucleon was going to be surpassed, and that therefore one would be able to produce baryons for the first time. Now, after the Bevatron we do not have the same situation; we do not see any more characteristic energies which lie above us. There may be some, but we have no general arguments as to where they may be except for one which comes when the weak interactions begin to saturate the unitary condition, if that ever happens. Unfortunately, that energy is at about 10^6 in the laboratory and we are not going to get anywhere close to it in the present situation, so that it's not fair to invoke that energy in the discussion we are having now. Now of course it may very well be that there are some characteristic energies between 30 GeV and 10^6 GeV which we just don't know about. If we're lucky, and one of those energies happens to lie in the next decade in the energy scale, then we may discover some characteristically new phenomena, but I think it's not quite fair to point to past experience, where characteristically new things have appeared at every stage and say that it's very likely to happen again, because always in the past we have had at least a glimpse of the threshold that we were looking forward to, and that is not the case in the present situation.

With respect to weak interactions, the most promising characteristic energy that one can think of would be the mass of a vector boson which, if we're lucky might be achievable in this next stage, but there's no reason to think that the mass of a vector boson is anything in particular, except that it's more than a GeV or so, and it may very well be discovered by experiments with existing accelerators if the experiments are pushed to a sufficient sophistication. So this particular point is completely cloudy.

Neutrino experiments in general of course will benefit from increase in energy, there's no doubt about that. But I would prefer to say nothing more about neutrino experiments, and to vest

my own appeal for a step up in energy entirely on what has been understood already about the strong interactions, and what the immediate future seems to promise. In describing this, as I have already indicated, I am presenting a point of view which is not shared by the majority of theorists. It's a rather conservative point of view I believe from the experimental standpoint, but it's perhaps an overly bold point of view from the theoretical standpoint, because it rests on the notion that in fact for strong interactions, there is only one characteristic energy, which is of the order of a billion volts: that all the particles that we're going to see will end up having masses of the order of a few billion volts; and that the majority of this spectrum is going to be investigated with existing accelerators. Within the next six or seven years this very complicated spectrum, I would expect, would be fairly well filled out. There would be some particles whose quantum numbers make them a little bit difficult to get at with the intensity of the secondary beams which we currently have, and no doubt a step up in energy will make such particles more accessible, but I do not see the discovery of new particles in the strongly interacting business as an essential feature of the new accelerator we are talking about.

Similarly, it has been very clearly established that one billion volts characterises not only the energy scale but also the momentum transfer scale. Nearly all the reactions that have been seen either with accelerators or in cosmic rays have shown that one billion volts momentum transfer is all you get. If you go very much beyond that, then the cross-sections drop very drastically, and so on this basis again, the step-up in energy is not going to be particularly significant.

Now, that's a gloomy picture. Why then would I be enthusiastic about going up in energy to 100, 300 or 1,000 billion volts? It is because of the point that Salam mentioned, that we see the glimmering of a theoretical tool in the Regge type of asymptotic expansion, which promises - even if it has to be considerably modified from the form in which it is at present - to give a systematic way of digging into the fundamental laws. The Regge idea if I can over-simplify it a little bit - says that when you go to high energies, you should be able to represent the reaction amplitudes which you see in terms of a fairly small number of poles, and that these poles are closely connected to the particles that have been already observed and are all the time being more and more studied in the BeV region. In order to have only a few poles contribute,

however, you must go to energies which are substantially higher than a billion volts. Let's assume for the sake of argument that 1 GeV is your characteristic energy. Then you get the situation that when you are well above a billion volts, your scattering amplitudes can be represented as, let's say one pole plus another pole, plus another pole: each one of these poles contributing with a strength which goes with a power of the energy, and the power of the energy depends on the position of the pole in the complex plans and that in turn depends on the masses of the particles with which the poles are connected. The picture so far, from the experiments which have been done indicates that these poles are separated by distances which correspond to powers of energy which are of the order of $\frac{1}{2}$ to 1. This again is in line with the view that in the strong interactions picture there really aren't any small or large dimensionless constants at all - everything is of the order of unity, and this 1 GeV is simply the energy scale on which everything is based.

It is reasonable then that the separations of the powers which occurs in the Regge expansion should be of the order of unity, and experimentally they seem to be. The total cross sections which have been studied with the existing accelerators seem to have power dependences which correspond to individual separations of this order of magnitude.

Now, that means that to separate out the top few Regge poles, you must go to sufficiently high energy that all but a few have dropped down to the negligible stage. This is very similar in the inverse sense to phase shift expansion at low energy. If you go to sufficiently low energy you can make everything but your S-wave negligible. As you come up in energy the p-wave comes in; the d-wave and so on. Working with the Regge expansion you are doing just the opposite sort of thing at high energies. If you go to extremely high energies you make the top-level pole dominant. As you come down, you begin to see the second one. Come down some more and you have the third or more come in. Now because the characteristic energy is supposed to be a billion volts, you can estimate the relative strength of these different poles by just taking your energy, - and it turns out to be that the laboratory energy is quite a reasonable quantity to use in this respect - take your laboratory energy in billions of volts and raise it to the power which is associated with the particular Regge pole, and that gives you a reasonable estimate of the contributions. That means, you see, that at 30 billion volts (let's say that the spacing of the first two contributions is of the order of half a unit of power) then the

second pole will be about the square root of 30 in importance relative to the first pole - something of the order of 20%. This is a fairly typical type of result. If you want to separate out the third pole however, if you want to look at the contribution of the first two poles, then the contribution of the third pole is down by an appropriate amount and then your experiments have to be enormously accurate in order to be able to pick it out.

Well, 30 billion volts, from this kind of argument is just on the borderline of where you can begin to do something because of the fact that $1/30$ is just beginning to be a reasonably small number, and it is true that some total cross-section experiments can be profitably analysed by the Regge approach, but not very many other kinds of experiments seem to be clearly analysable yet because of the fact that the energy is not sufficiently high. If one simply asks the question - "how high an energy do you want to get an appreciable increase in analysability over what one has at present?", one might say that you would like to see these Regge power behaviours clearly manifesting themselves. The spacing of the trajectories on the average is something like half a unit of power. Suppose you want then to get a factor of 2 variation over the whole energy range in which the expansion can be used, then you would say that you want a total energy variation of a factor of 4. The existing accelerators are just on the lower limit of where you can begin to use these expansions: so a factor of 4 increase over 30 GeV is guaranteed to give a good test to the whole idea of the Regge expansion. Of course if you have a factor of 10 in the energy that's still better. The higher the energy the better off you are from this point of view, but I would say that you will certainly get something very valuable if you go at least a factor of 4. And I don't see any rational way of pinning down a characteristic energy. It's simply a question of what the practical possibilities are.

That's all I have to say. In a certain sense it's conservative. You may discover some enormously different and unexpected phenomena by this jump in energy, but even if you don't, you're guaranteed of getting something very potent from the Regge expansion.

Cockcroft: Any observations?

Salam: I agree with what Professor Chew has said, but he has not commented on the point which I made about neutrinos and the aspect of synthesis which I was looking for between strong, weak and electromagnetic interactions. May I take it that you agree with

that point?

Chew: Well, I agree that that would be terribly exciting, but I find it difficult to believe that the step in energy we are talking about would be enough to unify these fields, because I can't see what the energy is that will do this.

Salam: No, I can't fix the energy. It's a question of producing neutrinos at all to do experiments with neutrino scattering.

(Electron experiments?) Yes, electrons produce scattering - produce the form factor or Hofstadter factor and if you want neutrino scattering you end with the same for the weak interaction form factor. If you really want to plot on one side of the board the form factor from strong interactions, which presumably the present machines will provide, and the form factor from electro-magnetism.

Chew:? Which you believe will probably be the same, don't you?

Salam: No.

Chew: Yes, but we need to measure all those. Where is the prospect of unifying the weak and strong interactions in the face of

(discussion inaudible)

Salam: Well, this is not a theoretical discussion!

Chew: It is true you will measure some more form factors, I agree with that.

Salam: It is an absolutely vital thing. For the first time some inter-relation between the three types of interactions.

Powell: I don't know whether experiments of this kind would resolve the question of a synthesis, but I suppose both parties would agree that the form factors are an essential step towards a synthesis.

Massey: It seems to me that the fact that we can't foresee is the very justification for what we're doing. It shows that it's fundamental. If it were not fundamental we could easily arrange a whole series of experiments to measure this and that. If it is fundamental we don't know what we're going to find out.

Merrison: If I may go back to the early part of your speech, you talked about knowing characteristic energies. In fact of course the energies of the 400 MeV cyclotrons were fixed before the discovery of the pi-meson and it turned out that the most useful thing they could do was to make pi-mesons and there was a whole range of physics begun there. (Voice - they thought they were making muons).

Merrison: They discovered a strongly-interacting particle produced copiously by cyclotrons. If it hadn't existed, then the original reason would have been nonsense. And then with the Bevatron.

Admittedly then there was this argument that the protons had such-and-such a mass, but if you look back on the Bevatron's history, the really important energy has been the threshold for producing strange particles, and again the existence of strange particles was not known before the energy of the Bevatron was settled. So I think one shouldn't place too much emphasis on this question of characteristic energy. We are much more sophisticated about our ignorance these days, but I don't think its any more valuable.

Salam: I think the justification for a machine like this is the following. Here we have for example electro-magnetism, which seems to be very successful. But there's still a justification to build a Panofsky machine in order to verify it at a very much higher energy. Supposing electromagnetism comes out right, then that's the end of it: that's electromagnetism set on one side and there's no more to be learned about it. Even for that, if we have nothing else to do, we want to know how the neutrino form factors behave, how the Regge poles fit into this form extraction characteristic, but if no surprises come, even then a machine of this type is fully justified, just as Panofsky's was justified, to finish off one particular set of rules. Then we won't come searching again for money!

Burhop: There is one pointer to the machine energy which has already come out at C.E.R.N. At the last meeting in January, Gregory was very insistent that the French wanted a machine quickly and if C.E.R.N. were not going to build one they would go ahead themselves and set up a study-group for a 60 GeV machine and so on. His idea was that you could get a machine quickly simply by scaling up the parameters of the C.E.R.N. P.S., but it appeared very early in the study that I am referring to that this was a fallacy. Owing to the fact that as you go up in energy in this process you have to inject at lower magnetic fields, the aperture of the machine has to be greater in order to contain the wanderings about the orbit, and in fact it is cheaper to use a different philosophy, namely to inject at a high energy and perhaps the best way to do this is to have an intermediate proton synchrotron of perhaps 3 GeV. This enables you to get away with a very much smaller aperture and a much less expensive magnet, and also the R.F. programme is very much easier, because you haven't got to have such a wide range of modulation. It turns out that the cross-over energy at which it's more economical to use high-energy injection occurs somewhere between 100 and 120 GeV, so that the machine of the future will undoubtedly have high-energy injection, and 100 GeV is just a nasty energy where you get the worst of both worlds, because when you go to high energy

injection, not only do you reduce expense but you greatly increase the space-charge limit and so it's possible to conceive of a machine which will give you about 10^{13} particles per second at 120 GeV, say at only a little more cost than a scaled-up C.E.R.N. machine from which one would only get one-tenth as many particles and this does give some guide from a technical point of view to the lower limit of machine energy.

Salam: I would like to ask about the paper by Johnsen comparing 120, 150 and 300 GeV machines with the C.E.R.N. P.S. If I read it correctly the comparison between 120 and 150 GeV came out strongly in favour of 150.

Burhop: That's in comparison with a scaled-up P.S.

Pickavance: I think its also true, however, that if you took the 120 GeV scaled-up P.S. and made that have a booster it would be a better machine, and still be cheaper than 150 GeV. What you are really saying is that the expense does not go up too much.

Burhop: At 100 GeV it would probably be cheaper to build a scaled-up P.S.

Adams: I don't know what this all proves. For a bigger machine you do technical things in injection but this we knew five years ago. I don't see the significance of changing technology to fix an energy.

Pickavance: In the old days one used to say for a very big machine of say 300 BeV or more, you would have to build an extremely high energy linac for injection, but the other approach is to do something simple, to extrapolate the C.P.S. to high energy with a simple linac. What the C.E.R.N. people have now concluded is that it pays you to use a booster at rather lower energies than has been supposed.

Gunn: I think that a very vital point has come out here which is that the cost plot is flattening out so that in going from 150 to 300 GeV the cost does not increase by a factor of two. I think that is an important fresh factor in considering the energy of the machine.

Pickavance: If I can just quote from the paper of the C.E.R.N. accelerator research group which Abdus mentioned the very provisional cost estimates which Johnsen and his group worked out would show that the 300 BeV machine would cost in total up to the end of the construction time £120 million; the 150 BeV would be £76 million, and the 120 BeV not using a booster would be £73 million. The last two figures show that if you have to

compare the 150 BeV machine with a booster with the 120 BeV machine without, you obviously choose the 150, but there are other possible permutations and combinations.

Stafford: Kjell Johnsen also gave the rates of spending on the 150 and 300 GeV machines. They were roughly the same, only for the 300 GeV machine the spending would go on for longer. So from the point of view of the rate of contributions by Treasuries, there is not much difference. But it would take longer for the experimenters to get to work.

Merrison: The time taken to build the machine seems to be a very flat function of energy; Kjell Johnsen was talking in Rome of a period only 2 years longer to build the 300 GeV machine, which I think is a very significant point. And the amount of first-class scientific manpower you need to build the machine is a fairly flat function of energy too.

Salam: Because of the resistance to this project which we have heard about from Professor Massey, one should fix the energy at the higher value, so that we don't soon have to go again for another machine of higher energy.

Stafford: This is very important. We are taking a step which is going to take so long and cost so much money, that we must make sure that it is big enough.

Chew: Maybe the length of a human life-time gives the scale. How long will a person work on a project of this kind?

Adams: We used to worry about this problem at C.E.R.N. People stayed on the C.E.R.N. P.S., but my worry with the new machine would be that it might not have the interest to attract the sort of staff to whom to entrust that amount of money - especially if it were a 120 GeV scaled-up C.E.R.N. P.S. Alternating gradient and the non-linear theory are no longer novel and interesting.

Mullett: But the interests of accelerator physicists must not be given preference to the needs of high energy physics. If this thing has to be done, it has to be done. We have to train a better generation of engineers. By the time it was finished we should have some wonderful engineers.

Hubbard: Could I put another point to Dr. Adams. When the same sort of discussions were taking place on the C.E.R.N. P.S. it was not a radical and interesting machine to the same extent. Every time you must hope that when you build the machine some new advance will allow you to make it more than just a scaled-up

version of the last. But you must not know about it at this formative stage, because if you do, the machine will be too late to be the first to use the new advance.

Stafford: There is another point of view. A machine of this sort is not just a ring with beams coming out of it, but includes all the beam transport and techniques, associated with the experiments. These make a challenge big enough for anyone.

Cockcroft: I think we should now take the last contribution about manpower.

PICKAVANCE

I was going to make just a few remarks about some of the manpower aspects. The manpower problem is of course a very important one and rather involved. Of necessity I am talking about quantity and not quality, and so I've no answer to John Adams' point, except the one that Mullett has given him.

I started by looking at the manpower we have at present engaged in this country in the field, and I defined the field as being on or in close association with accelerators above the pion threshold. So I'm eliminating from this particular survey the nuclear structure machines, the Harwell synchro-cyclotron for example, and the 50 MeV machine of N.I.R.N.S. And so we are talking about several machines, the C.E.R.N. P.S. and S.C., NIMROD, because already this involves a large number of experimental physicists who are preparing experiments, the Birmingham P.S., the Liverpool synchro-cyclotron, the Glasgow electron synchrotron and, in the later stages of course, we have to consider the needs, and the facilities provided by the new NINA electron machine. The numbers that I am going to give are extremely rough but they do exclude U.K. staff at C.E.R.N.

The number I think which is basically important is the number which we can predict of the experimental physicists using these facilities at Ph.D. level and above. I think at the moment the number of people in Britain in this category is around 120. For comparison - my figures are even rougher - I think in the low energy part below the pion threshold the corresponding number is 190, which is quite substantial.

The number of applied physicists and engineers, whom I'm going to say rather little about, associated with the machines in this country at this moment (the proportion changes rather rapidly as NIMROD comes in) on the operation, development and construction

of the machines is about 60 and on high energy physics facilities, such as data reduction, bubble chambers and the like about another 60, so that the total numbers, counting all the professional engineers and experienced applied physicists of similar capability in their field to the post-Ph.D. experimentalists - the numbers then are about the same in these two fields.

Now let us see what we may expect to be the production of the experimentalists who are the basically important people for whom the machines are being built, in relation to the time-scale of the C.E.R.N. proposals. I start by looking at a survey which Merrison made as part of a detailed questionnaire around the universities some time ago, the results of which showed that over the years 1955-1960 an average of just over 170 Ph.D's per year were turned out in the whole of experimental physics. Of these it turned out that 10% had got their degrees in high energy physics, about 16% in low energy physics, and about 7% in cosmic rays, so that the so-called "nuclear physics group" of subjects added up to 33% of these Ph.D's. Now, a rather careful survey was made in the U.S.A. at about the same time, and published in "Physics Today". This is for the years 1956-1960 which showed that there too about 10% of the Ph.D. output consisted of people who had got their degrees in experiments on high-energy machines, and the total production of Ph.D's in the U.S.A. at that time in physics, not experimental physics, was about 500 per annum, so the numbers are rather surprisingly over that period, more or less in the ratio of the populations of the two countries. In projections into the future in more recent U.S. surveys, these numbers diverge rather considerably. The Americans are expecting to turn out many more Ph.D's in the future than I think we can hope to do. But just at that moment, about 1960, we were in step.

Now the fate of these physicists investigated by Merrison showed that up to 1960 about 27% of them stayed in their own or other universities, for a while anyway, and that a few others - we don't know the number - stayed in the subject; that is took jobs in C.E.R.N. or N.I.R.N.S. etc. So starting from this, I tried to guess, not taking it too seriously, and putting in rather round numbers, what could be expected to happen in the future, on certain starting assumptions.

The first assumption is that over the next ten years, assuming that there are facilities to attract students to work on them, in high energy physics we should expect, conservatively I think, to increase by about 10% per annum the number of Ph.D's gained. This is in accordance with the planned university expansion and not

assuming an increasing proportion going into this particular field. Over the last 4-5 years I believe the D.S.I.R. grants for research students have increased at a considerably higher rate than this, and the D.S.I.R. have found that the overall quantity of the students has been maintained.

The second assumption is that we might expect to continue to devote 33% of our young physicists to the group of subjects, high energy and low energy physics and cosmic rays, but that rather more in future would go into high energy physics; if indeed there are facilities for them to work on, and I have assumed 15% instead of 10%. Incidentally, this is also the assumption in the forward U.S. plan. The reason for feeling that there will be defections from other fields is that for example many of the people interested in high-energy interactions namely, cosmic rays, are already beginning to turn their attention to experiments on machines, and I suspect that a few people who graduated as it were in low energy physics would tend to move over to high energy physics. This is certainly happening with our own machine at the Rutherford Laboratory, and with the A.E.R.F. synchro-cyclotron, which I regarded as a low energy machine.

The third assumption (and this is very speculative) is that one might expect a Ph.D. if he stays in the subject to be productive in research on the average for about 20 years.

Fourthly, the number who stay in the subject, corresponding to Merrison's 27% - 30%, I have assumed might rise to 40%.

From this basis of course it's simple arithmetic to predict the numbers who will be available for, or who will be wanting research facilities in high energy physics over the coming 5 or 10 years. The numbers, including the residue of the present 120 turn out to be about 180 or 185 by October, 1967, and I've made a very rough guess of what we might need at that time or what facilities will be available at that time from the programmes that we know about, because nothing new that we start can be operating by then, and I conclude when one remembers that the electron machine in the North will be operating by then, that the C.E.R.N. C.P.S. will consume more people, and here I have assumed the predictions in the Bannier report, and that NIMROD will have more people on it, than at the beginning; and I conclude that the numbers will be about in balance, that we shall have facilities for and will need about 180 people on this basis.

The next five years are the important ones because if we start a new international machine it's during that time that it

will be coming into early operation. With the same assumption again about the wastage and so on we would come out with a figure of about 280 or 290 total experimentalists in this field. If one makes a guess as to what NIMROD and NINA and the C.E.R.N. P.S. soak up in additional people at that time, it wouldn't be very many; the machines would be overcrowded by that time but on the other hand experiments become more elaborate as time goes on and need more people to work on them, and furthermore, there are a couple of machines in this country which I would guess would certainly be shut down by that time if not by the earlier date - the Glasgow electron synchrotron and the Birmingham proton synchrotron, which would release some people looking for facilities. Some of those people would undoubtedly find an outlet on the 4 GeV electron synchrotron which is being built for them among other people, and some of them, particularly at Glasgow, would go into the low energy field on the electron linac. And so with the guesses that I put in there, I convinced myself that we might put in about 160 people for the programme that we know about at the moment if no more machines are built. This would leave 120 or more free for the new machine that C.E.R.N. are talking about, and for a possibly enlarged national programme if it was decided to have such a thing. If the experience in other C.E.R.N. countries were the same, and certainly the large countries' plans for university expansion are similar to ours, and the percentage increase in people graduating is similar to ours, this would mean there would be 400 or 500 people, if indeed they had been trained during that time, looking for facilities to work on. This is certainly more than on the estimates C.E.R.N. have been able to make would be capable of being accommodated on the machine by that time (I'm talking about 1972) say $1\frac{1}{2}$ years after the machine came into operation if it's the smaller one or about the time it would come into operation if it were 300 GeV.

If we make this guess even more conservative and stick to the present situation where only 30% of the people who get Ph.D's continue to work in the field and the others get jobs in other fields of activity you still reach a result of 80 or so people generated in this country beyond what the present programme could conceivably support, and these would certainly be enough to start off the work when multiplied by the factor of 4 to convert U.K. into Europe for the new C.E.R.N. machine. The C.E.R.N. people themselves have made some estimates of what they would require there, by extrapolating their experience with the present machine, and these amount to saying that by 1970 or 1972, depending on the size of the machine, they might need about 100 to 130 Ph.D's in the fields of high energy physics and data handling.

Cockcroft: Any comments?

Adams: Of course this all rather depends on high energy physics having the attractions that it has today. These arise both from the fact that many of the physics professors come from the high energy physics or nuclear physics side, and from the fact that the subject in itself is still extremely lively, important and fundamental and carries a lot of prestige. But in the years we are considering supposing biophysics blossoms out very quickly. It would I suppose take about half a human lifetime at least for the full effect to be felt after professors had changed and so on, but it may well be that the influence from the research students' point of view might begin to swing more quickly to another subject. Such a change in the position of high energy physics would almost certainly be downwards.

Pickavance: Yes. But we start with the known fact, not just from British experience but from other countries working in this field too, that about one-third of the experimental physicists are going into these related fields. We have only allowed for half of this number to work in elementary particle physics.

Another important assumption in all these figures is that there will be enough facilities available in the present C.E.R.N. machine and the national and university machines to provide for all these physicists to be trained. There would be no sense in going in for the big new machine unless we did train something like these numbers of young physicists to use it. We would have to work the domestic facilities very hard indeed, and might have to pump more money into them to get more groups working on them. Just to give an example, we know now of 21 different groups, some large, some small, in 13 different universities who are at this moment engaged in high energy physics. Most of them have made plans for working with Nimrod. These 21 groups would have to have something between 5 and 8 research students per group. It adds up to a great number of people, and at present we have only two high energy machines in the country, plus C.E.R.N., which at present is not very good for students except in the film analysis groups. Looking at this situation makes me feel that your suggestions that the U.K. might go in for another machine at some time during the period we are considering is probably a realistic one.

Massey: I think there is an important point there. I don't really believe that we have consciously trained people to work at C.E.R.N. We are now talking about training people to work at this big accelerator. This is involving a change of policy, because we are not doing this as far as C.E.R.N. is concerned. In fact one of the

problems is how we use C.E.R.N. from a university point of view. This is a thing we shall have to give a good deal of attention to: how we would use this facility if we were given it. The difficulties are ones which are definitely not scientific. They involve all sorts of personal domestic complications and so forth. I would have thought that with Nimrod coming on there would be a large tendency for the next few years for the high energy groups to be national in outlook rather than international.

Pickavance: Certainly no-one has sat down to work out how to train people who in future will join in and make good use of C.E.R.N. This gets you back to the arguments about the pyramid. But at present graduate students are being produced by people who make use of C.E.R.N., not necessarily by taking the students there.

Massey: I agree with this, I am just not sure that if one carries on in the same way one will get the people available to use the new machines. There is a different outlook required.

Butler: The way high energy physics instrumentation is going, it's going to become more centralised. At the moment we've got roughly 50 professional physicists in this country concerned in work which comes from C.E.R.N. - some of it in Imperial College a few yards away from in here. In 10 year's time this may not be the case, because data processing may be done - and rightly - by extremely elaborate facilities in the machine laboratory. So that will change the pattern.

Pickavance: I agree, but it's not quite so disastrous as if there were only one machine laboratory.

Cockcroft: To go on to another subject. When this report is being prepared by the working party, I think something should be included about the position of nuclear physics in the universities.

Massey: I think the position is that a forward look on many of the aspects of this is available. The nuclear physics sub-committee of D.S.I.R. set up a working group to examine the future requirements in the field of nuclear structure; a report will be available about this. This I think covers the major additional features on top of those associated with the development and operation of Nimrod and so on.

Cockcroft: So that could be fed into the report?

Massey: Yes.

Cockcroft: Are there any other general points which people would like to make? We shall have to set up a panel to prepare this

report now. This discussion will provide a very useful basis for it.

Adams: I wonder whether anyone has any views about the combined programme of storage rings and the larger proton synchrotrons.

Cockcroft: I personally have not heard the latest case for storage rings. We knew that the C.E.R.N. physicists were originally very unenthusiastic. Is there strong support now, from the physicists?

Pickavance: I think the attitude has probably changed quite a lot. When these arguments first started the question being asked was a ridiculous one - would they rather have storage rings or the new synchrotron. But storage rings become much more exciting if they are additional and not alternative to the new accelerator. The two are complementary.

Burhop: Apart from colliding beam experiments, a storage ring could be used to give the beam current for a particular experiment. For example if you had 10^{14} particles ~~per second~~ circulating in the ring, you might only require 10^9 for a particular bubble chamber experiment and ⁶⁰⁰⁰ ~~per~~ the ring by one pulse every minute or so. Also for example for neutrino experiments you could fill up the storage ring to its full capacity in say half an hour and then suddenly take everything out and produce pions. You could then do a neutrino experiment in a hydrogen bubble chamber. My impression is that the majority of people on the Amaldi committee would rather like to recommend a programme of going ahead straight away with storage rings and postponing a little the decision on the high energy accelerator. This makes very nice sense in that the storage ring programme as well as being completed by 1969 would involve an expenditure of about 60 million swiss francs per year for the next few years and then you can imagine a 300 GeV accelerator coming on top of that. The difficulties of this course are two-fold. First there is the suspicion of the physicists that they would be swindled in some way - that the Government would think that the storage rings satisfied all their requirements and in fact that the accelerator would never be built. The only way to get agreement on this would be to have a package deal involving the accelerator as well as the storage rings. The other reason is a national one involving France: that the French say that they are badly off for facilities because Saturne is coming out of operation or at least has passed its best days, and they badly need some intermediate facility. Their time scale for building a 60 BeV machine is much the same as that for the new European accelerator,

namely decision by 1964 and I am afraid that any decision to postpone the construction of that by a few years, would mean that the French would go ahead with their 60 GeV accelerator and then might not have enough money to come in on the big machine.

Hubbard: The big machine would undoubtedly need a design study period which could not possibly be only one year. I think what we are probably faced with is a request from C.E.R.N. at the end of this year for a supplementary programme for planning, (3 to 4 million francs over next year) covering both projects. At the end of 1964 a positive decision might be taken on storage rings. At that stage you would not be able to put to the Governments a firm proposal on the big accelerator. Even if you were ready on engineering design, I very much doubt if you would be ready on Convention design - an even more tricky branch of the art, and you would then have another year during which negotiations over a new Convention would take place, and I think probably you would also have another year of design work to do. So at the end of 1964 your decision would be yes or no on storage rings and to go forward to one further stage of design on the big accelerator, but it could surely not be till the end of 1965 that you would take the irrevocable decision on the accelerator.

Adams: Will storage rings not require a new Convention?

Hubbard: As you know there is provision in the present C.E.R.N. Convention for supplementary programmes. I think the C.E.R.N. Council are agreed that storage rings if built would be a supplementary programme.

Salam: How would you counter the suspicion of physicists that they would be swindled by the storage rings?

Gunn: I have a certain fear there too. These storage rings presumably cost some fairly large sums, and it would be a pity I feel to start off with a thing that we have not even talked of here. It has been very secondary to all of us here. We have talked principally of the work which would come from going up to the 150 GeV machine. There would be a certain loss in our finishing up with a statement which put these storage rings first. We might be stopped there. It's an easy way out to say we don't have to make a choice. If we were making choices I should put storage rings very much second, to a 150 GeV machine.

Adams: I should be surprised too if the Governments did not feel this as the thin end of a wedge - first a study, then storage rings, then the big machine. Governments would surely want to

have the programme submitted as a whole.

With regard to the Convention, I'm not sure that the new accelerator would require a new Convention.

Hubbard: I feel sure that it would. It would need a new site. The existing C.E.R.N. Convention says the laboratory will be at Geneva.

Walker: Whether it could be done on the present Convention would depend a lot on the actual wishes of Governments. It would be desirable for our own administrative reasons to have one Council dealing with all the programmes - if this were possible, but there are a number of countries who will regard the siting of the machine as an important factor in deciding whether they will back it.

Adams: Do we know the latest American decisions on storage rings and on the Berkeley studies?

Chew: My impression from attending a meeting in December was that the storage ring idea was very much down, and that the thinking was in terms of backing the kind of programme we have been talking about.

Burhop: Ramsey and Gell-Mann came along to the Amaldi Committee last week and seemed very much inclined to adopt the C.E.R.N. programme! They had not realised the point about high-energy injection, which makes the Berkeley idea of a 100 GeV machine rather an unsuitable choice. The information we have here suggests that Berkeley have now put up their sights to a 200 - 300 GeV machine. At the same time last week there started in Brookhaven a study on storage rings, and apparently they felt that their sour attitude to storage rings had been a mistake in the past. And in this general scaling up, the Brookhaven study project which originally was for a 600 GeV machine has been pushed up to 1,000 GeV.

Massey: We talked this morning about Britain spending 0.2% or so on research. The thing which I don't understand is the relative amount of effort which is being put into pure science in the U.S. as compared with this country. It is said that in this country we spend relatively more money on nuclear physics than in the U.S. If so, where do we waste it?

Flowers: It is the same fraction of the Gross National Product, not the same fraction per head.

Massey: So the U.S. spend three times as much per head on nuclear physics as we.

Stafford: There is the other point that our spending started much later, so we have not yet reaped the benefits. At C.E.R.N. we have started to reap the benefits, but at the National Institute we haven't properly started yet.

Massey: To get the facts clear, the U.S. Gross National Product is 8 times larger than ours, their population 3 times larger? (Yes).

Cockcroft: I think we should close this discussion now and set up a panel to produce the report. Then perhaps we should meet again. We could perhaps take the same sort of people who produced the last major study of this kind.

(The following were nominated:

Professor Flowers (convenor)
Dr. Adams
Professor Butler
Professor Cassels
Sir Harrie Massey
Professor Merrison
Dr. Pickavance
Professor Powell
Professor Salam
Professor Wilkinson)

There is not too much time; we have to produce something as soon as possible.

Adams: I would be inclined to append the C.E.R.N. document to the report. Then you would not need to write out all the same points again.

Cockcroft: I think we could agree on that. Then we have to decide who will be the convenor. Flowers is on the A.C.S.P. Would you like to take it on?

Flowers: I wouldn't like to, but I will if you wish.

Cockcroft: I am sure that all of us will want to see this report. Then we may have another meeting, but in any case we will send it round.

Powell: May I just mention that Wilkinson and I prepared a document along these lines some time ago. It might be useful as a contribution. Many of the points dealt with today were mentioned.

Cockcroft: I am sure it will be very relevant.