A POSSIBLE HIGH INTENSITY, HIGH ENERGY ACCELERATOR

M. Schwartz
Nevis Laboratories, Columbia University

I would like to outline what I consider to be a major and necessary revision in the report of the Ramsey Panel, and to indicate the program which I feel should be followed in planning for future high-energy facilities. I would like to point out in particular that there exists an alternative which does not appear to have been considered by the Ramsey Panel and which I feel deserves further study. I appreciate, of course, that further study of any alternative may lead to a delay of the program as a whole. However, I feel that the cost of any high-energy program is sufficiently high both in money and time that a year or two of delay is warrantable if the best interests of high-energy physics are served in the end. Let me emphasize that what is being planned for here is likely to be the last major program in high-energy physics within at least the next quarter century, and that a program based on expediency without further study is certain to hurt the nation both in quality and cost.

If I were asked to give my opinion as to what I wanted most in the way of a new facility for high-energy physics, in agreement with the Ramsey Panel I would give the following answer. I would like the most intense, highest-energy machine that can be built within the limitations of the present state of the art, the available money and ten years. Unfortunately, there is no simple equation or set of boundary conditions given, or unique solution. However, assuming the same size of program as a whole as that which the Ramsey Panel apparently used, namely of the order of one billion dollars in ten years, I believe that a far better program, leading to a much more useful facility, is possible and at least needs further study and exploration.

Through discussions with representative accelerator experts from Brookhaven, Berkeley, and MURA which took place on June 1 and 2, 1963, the possibility became apparent that the entire beam from an FFAG-type machine could be injected into a high-energy AGS. This point, which was apparently unknown to the Ramsey Panel, represents to me the single most exciting prospect on the horizon of high-energy physics and may serve to open up worlds of exploration which were undreamed of but a short time ago.

The machine that I talk about is an accelerator with energy of the order of 100 Bev and intensity of 10^{14} to 10^{15} protons per second.

I will begin by extolling the almost obvious virtues of such a machine from the point of view of physics. Then I will discuss the question of its practicality from the technical point of view and from the point of view of cost. Finally, I will indicate my feeling as to how a national program should be implemented within the framework of existing laboratories and organizations.

I. The Physics

We stand right now at a tremendous threshold from the point of view of the weak interactions. Indeed, if the CERN group has really discovered the intermediate vector boson, as appears likely, then we may soon find ourselves in an era whose productivity can be comparable to the days after the pion was first discovered. But this may only happen if we show foresight and intelligence now. The costs of equipment and accelerators being what they are, it is necessary to think some ten years ahead to the questions that are not likely to be explored in the intervening period.

To my mind, any really serious study of the high-energy weak interactions, including the production and decay of the bosons, requires that the experiments be carried out in a liquid-hydrogen or deuterium bubble chamber. There is just no substitute for the simplicity inherent in the hydrogen or the resolution inherent in the bubble chamber. This choice of instrument places, however, a clear requirement on choice of accelerator. In order to do these experiments properly it is necessary to have intensities of the order of 10^{14} protons per second at high energy and these protons must be delivered in bursts no less than a second apart.

Up until now the MURA FFAG machine has been the only one suggested which comes close to filling the bill. That has been the crucial argument in favor of its being constructed. But the proposed MURA machine falls short in one extremely important respect, namely, energy. Because of the high ratio of initial proton energy to final neutrino energy, the 12.5-Bev machine produces neutrinos which are very largely below the threshold for many of the most interesting processes. Indeed, to produce bosons in hydrogen, it is necessary to have neutrino energies greater than about 2 Bev if the boson has a mass of about 1 Bev. Furthermore, an increase of a factor of 2 in average energy in this region could mean an increase of a factor of 100 in the boson production rate! It is quite reasonable to think of 1000 bosons per day being produced in a hydrogen chamber if one had a 100-Bev machine at the proposed MURA intensities.

While there did not appear to be any way of getting high intensity at high energy, except at the frightful cost of 10 million dollars per Bev characteristic of the FFAG machine, I was thrilled at the thought of having even 12.5 Bev. Now that it may be feasible to get additional energy at the characteristic price of the AGS, namely of the order of one to two million dollars per Bev, it seems criminal to limit ourselves to 12.5 Bev without further study. It would be like starting to build the Rochester cyclotron after the pion was discovered, when we could be devoting the same effort to building the CERN cyclotron.

To continue my physics arguments, there is another field which has just opened up and which would be advanced enormously through construction of the proposed accelerator. That is the field of muon scattering. At high momentum transfers it becomes increasingly difficult to explore sensibly the electromagnetic structure of the proton by means of electrons. This is because electrons radiate as they scatter and because it is hard to avoid large backgrounds from photopion production and other unwanted processes. With muons this is no problem. It is possible to think of exploring protons to distances as short as 10^{-15} cm by means of these scatterings, with a considerably diminished background. And who knows what these experiments may turn up insofar as the structure of the muon is concerned?

In the field of the strong interactions the utility of this machine is obvious. As a source of anti-particles, strange particles and pions, it is superior to a low-intensity 200-Bev machine up to quite high energies, probably at least 50 Bev. Experiments such as hyperon-nucleon scattering, which have been intensity limited with present machines, become feasible. High-momentum-transfer collisions would be analyzable with much more precision than has heretofore been possible.

It seems obvious to me that from the point of view of physics, this is the machine to build!

II. Is This Machine Practical?

After on the order of a month of discussion with a variety of machine experts, no serious questions as to the feasibility of this machine have been raised. The detailed questions of optimum injector energy and optimum aperture as a function of price and space-charge limit need to be studied. The naive space-charge limit formula yields an injection energy of 5 Bev to a conventional AGS. However, image-current effects will probably require either raising this energy or increasing the aperture of the AGS. Other problems will have to be faced, and will certainly add somewhat to the cost of the machine. However, we must not lose sight of the fact that beyond a certain point, we buy energy at the price of the AGS and not at the price of the FFAG. There seems to be very little doubt at the moment that a 100-Bev high-intensity machine will cost quite a bit less than the combination of 12.5-Bev FFAG and 200-Bev conventional AGS.

None of the questions being raised on the practicability of this machine can be answered in full without further study. But the prospect seems good enough to warrant unquestionably this further study.

III. How To Proceed From Here

If modern-day machines took only two years to build and cost only five million dollars, the way to proceed would be clear. One could authorize the 12.5-Bev FFAG, complete it, and then think of how to use it as an injector. In addition, one could authorize the 200-Bev machine, complete it, and subsequently add a new injector if the original design permitted it.

No one can get too excited about a three or four year delay. And five million dollars isn't terribly much money.

But when machines begin to cost hundreds of millions of dollars and a decade to construct, we had better re-examine our approach. A machine which looks great today could be obsolete before it is finished. In addition, a saving of 10% on the cost of a machine begins to look quite important. Given this situation, what appears to be a good conservative policy now can easily be a costly mistake in the years to come.

The Ramsey Panel suggests that 400 million dollars in new construction funds be authorized during the next three years. It stands to reason then that the following questions be asked:

a) From the point of view of physics would a 100-Bev high-intensity machine make more sense than the two machines presently planned?

b) How long would it take to come up with a workable design and cost estimate for such a machine and who should do this design?

c) How is it to fit into a national program? Namely, what comes after it and who is to build what machine?

These questions are all difficult ones to answer. It seems to me that I have answered the first of these already, by pointing out the rich possibilities such a machine would have. I will now consider the other questions.

The general feeling among machine builders I have spoken to is that, if the machine is feasible, a proposal could be made ready for the FY 1967 or FY 1968 budget. A serious feasibility study might take of the order of a year. Detailed cost estimates and some preliminary design might take a year or two more. All of this falls within the Ramsey time schedule for the step to higher energy.

Who should design this machine is not at all clear. My own inclination is that the MURA group should carry forth this design because they have the most experience with the FFAG principle. Furthermore, if this machine is feasible and practical, then there is no sense in building the 12.5-Bev machine without further study as to a suitable injection energy and aperture at the AGS. In addition, the MURA group is in the best position of all to initiate such a design study immediately.

Insofar as the over-all program is concerned, I would like to see this machine followed closely by a 1000-Bev machine at high intensity. This step is truly an enormous one and it would seem to make sense for Berkeley and Brookhaven to collaborate on its implementation.

In conclusion, my program is as follows:

a) Do not authorize the 12.5-Bev machine in FY 1965.

b) Instead, ask MURA or some other equally competent organization to initiate a study on the feasibility and design of a 100-Bev high-intensity facility.

c) Authorize such a facility, if practical, in FY 1967.

d) Ask Berkeley and Brookhaven to design jointly a 1000-Bev machine, preferably with high intensity.

One final point -- it may easily be that six months of intensive study will serve to throw this idea into the wastepaper basket. If this happens, so be it. On the other hand, were the idea to be rejected without the study it warrants, it would be in my opinion a colossal and costly blunder. I feel strongly that the Ramsey Panel should reconvene to recommend a course of action in the light of the points made in the above proposal.