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# SOME PROBLEMS IN PLANNING FOR FUTURE HIGH ENERGY PROTON ACCELERATORS

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## I. Introduction

The purpose of this session on accelerators is, at least in part, I believe, to bring to this audience some portion of the subject matter of the first sessions of the international conference on high energy accelerators and instrumentation conducted at the CERN laboratories in Geneva in September of last year. I was asked to present to that conference some of the points which had been brought out in a series of informal meetings of a group of physicists at the Lawrence Radiation Laboratory at Berkeley about a year ago, and in a more intensive, one-week study by a group of visiting workers from several laboratories, organized last June by the Midwest Universities Research Association (MURA) at Madison. This latter study followed an exploration of feasible types of new accelerators conducted by other visitors at MURA during the preceding week. My task at Geneva was therefore that of a theorist, who has long been interested in accelerators, to report and to summarize the views of these groups of physicists whose knowledge of high energy experimental research was in many cases more intimate than my own. Today I will try to play that role again, at least to the extent of giving you some idea of the topics debated by these groups. However, I also want to emphasize that these problems deserve a great deal more serious study and hard work than they have yet received, and that the work of which I know to date merely points out some of the factors which must be weighed and considered, in the rapidly changing light of experience

in high energy research, in order to reach firmer conclusions on which to base future planning. In other words, I believe that most such discussions have raised more questions than they have answered, including the one you are about to hear.

By confining my remarks to proton accelerators I do not imply a conviction that new electron machines would be of little use. Instead, their uses are complementary, and in addition a practical energy limit for circular electron accelerator energy seems to be approached by the Cambridge electron accelerator, while the linear traveling wave type demands a specialized technology and the recommended designs seem to change very little from year to year. This last fact is in marked contrast to the situation with proton machines, where the variety of possible approaches is much richer. It is generally known, and was well documented at the CERN conference, that ample inventiveness has been applied throughout the world to produce a whole generation of new accelerator concepts and techniques, so that many of the limitations on present research may be eased or removed by future machines. These limitations include particle energies, beam currents, duty cycles, repetition rates, and other factors affecting experimental utility. It is obvious that a continuing search for new concepts and techniques should be encouraged. As an example, the suggestions about plasma and coherent acceleration techniques put forward by Veksler, Budker, and other Soviet physicists at Geneva in 1956 have at the very least led to the consideration of some interesting problems in relativistic plasma physics about which we shall hear from Dr. Rogers later in this session.

No one, I believe, would defend the view that existing machines plus those under construction will suffice to give us all needed information about the structure of matter. Even if this were so, the present rate of progress in making measurements leaves much to be desired, as is well

known to those who are familiar with the crowded experimental schedules at existing machines, the long runs required for many tasks, the rather small number of definitive experiments that can be conducted each year, and the great mass of unknown facts that we are all eager to learn. But the problems of planning are not merely those of reconciling available financial support with the rate of progress in carrying out a moderately well defined set of experimental objectives that can be set up as being of present interest. The real complexity of the task is probably clear to everyone, but perhaps I may be excused for enumerating some of these difficulties.

Not too many years ago it was possible for each innovator, and those associated with him, to simply convert his concepts into hardware and proceed to its experimental utilization. However, we have rapidly progressed to a stage at which determining the feasibility, design, and cost of a major accelerator are major efforts in themselves, measured in millions of dollars and hundreds of man years of effort by highly skilled people. A dominant factor is that the costs are so great as to require difficult decisions of national and even international policy. Because of the long lead-time of six to ten years required to justify, finance, design, and construct a big machine, each proposal implies or should imply the expression of a carefully considered conclusion as to the nature of high energy physics research in the relatively long-range future. There is much diversity of opinion among physicists on these matters, some of which is healthy and in fact inevitable in trying to predict the future. It is also natural that these opinions should reflect

not only the technical judgments involved but also personal optimism and conservatism, historical factors, regional desires, international competition, and other matters which cannot be excluded from consideration without altering human nature. As in many human affairs we are limited by our inability to see very far beyond the ends of our noses, or, in more scientific language, by the difficulty of extrapolating a function of unknown analytic properties into a new domain. In such an extrapolation the properties of the last known "points" assume special significance, and one should remember that the overall behavior of the function, as indicated by less recent past experience, may be very misleading. It is therefore important to assure that all available recent information shall be brought to bear on the question as to which of the many possible accelerators of the future will yield maximum returns of physical understanding for the investment involved. Many new insights are currently being won in the most advanced existing research work in this field.

Factors Affecting the Desirability of Ultra-High Laboratory Energies

I will first mention some of the factors affecting the desirability of ultra-high laboratory energies. I will not include colliding beam arrangements aimed at achieving high energies in the center of mass since these will be discussed in the following paper by Prof. O'Neill.

About three years ago some thought was given at Berkeley to the problems of constructing an alternating gradient synchrotron in the energy range of one hundred to two hundred billion electron volts. Workers in other laboratories have also considered this question. Our

tentative conclusion at that time was that the task was feasible but that it would be rather long and very expensive, and that it was premature to contemplate such a large project in the absence of data from machines in the 25 to 30 billion volt range. Our tentative estimates were based on a straightforward extrapolation of the Brookhaven and CERN designs. During the first week of the study session at Madison that I mentioned earlier, another look was taken at this machine design problem by Prof. Sands of the California Institute of Technology. He made some numerical estimates about a more radical design consisting of two tangent rings, the smaller accelerating protons to 10 billion vilts, which one then transferred to the larger ring in which they reach 300 billion volts. His work stimulated some of those present during the following week of study on uses of new machines to examine the utility of ultra-high energies.

Experiments in this energy range appear to involve somewhat heroic measures. It was pointed out that one might measure the curvatures of particles having energies below 150 billion volts in a large bubble chamber or in two chambers separated by a bending magnet; the displacement would be about a millimeter in traversing one meter of ten kilogauss field, and the angular displacement is a milliradian. For reasonable precision one would have to measure bubble positions to about $10^{-3}$ cm, relative to the bubbles of ultra-high energy tracks in the same frame to minimize distortion. The energies of secondary particles having velocities comparable to that of the center of mass ($\gamma \sim 12$) are easier to measure. Time of flight identification or

separation would require distances of several meters even for such particles, assuming optomistically that differences of $10^{-11}$ seconds could be measured. Some hope was expressed that higher energy particles might be identified by their characteristic interactions.

Three classes of experiments were suggested: (1) a search for new particles, (2) a statistical study of known particles, and (3) an examination of correlations among secondary particles in various interactions. During discussions at the CERN conference it was brought out that the cosmic ray data at around $10^{12}$ electron volts indicates an elasticity, or failure to produce secondaries, of greater than 50%, a tendency toward relatively small transverse momenta, and a tendency for forward and backward peaking of secondaries in the center of mass, but that none of the details are established beyond debate, because of poor statistics. If one regards the nucleons as cores plus clouds around them, then most cosmic ray events seen thus far may be cloud-cloud or cloud-core collisions, while the more interesting core-core collisions may be poorly represented. It is therefore hard to conclude whether or not the cosmic ray data should be taken to mean that there is no specific justification, aside from the need for better statistics, for accelerator work at these energies. The uncertainty is like that of a man asked to estimate his scanning efficiency for picking out bald headed men from a crowd without being told whether they are wearing hats!

To conclude the summary of the MURA study on this topic, it was pointed out that a single ultra-high energy accelerator would have some advantages over colliding beams, including higher interaction densities,

lower backgrounds, and better secondary beams, but also some disadvantages including a poorer duty cycle and difficulties of identification. It was acknowledged that it would be more expensive than adding a colliding beam capability to a single beam machine. From all of this I conclude that this is the wrong time to seriously consider ultra-high energy machines, especially since new information at 25 billion volts will soon be available.

Factors Affecting the Utility of Higher Currents

I will now turn to an examination of some considerations affecting the utility of higher currents. The Berkeley group addressed itself primarily to the question of studying the need for higher proton currents in the energy range 10 to 25 billion volts, and the largest effort of the visitors at Madison during the second week was also devoted to this question, although a number of other problems were also given attention there. On this topic similar discussions were held by both groups, and I will not try to separate the origins in summarizing the points that were made. High currents in this energy range could be produced by high repetition rate synchrotrons, by various types of FFAG machines, or by linear accelerators.

It is becoming clear to many high energy physicists, both theoretical and experimental, that for a long time to come there will be a need for much more detailed data on the interactions of the known strange particles. According to our present theoretical understanding the interactions between any pair of the more than twenty strongly coupled particles are, in principle, of equal interest. In a more general way, many theorists feel that the masses of the particles are somehow a result of their interactions, and that we should not expect to understand the masses and the interactions separately. The dispersion-theoretic approach to these problems is meeting with increasing success

and acceptance; more precise experimental information than has been needed heretofore, and over wider energy ranges, will be required to exploit this approach. This point of view emphasizes the importance of trying to apply the suggestion of Chew and Low for conducting experiments in which particles other than nucleons may be regarded as virtually present in nucleon targets; it is clear from the start that this type of experiment will require higher intensities than are presently available.

The energy range up to, say, 12 or 15 Bev may be singled out as including, with a reasonable margin, the production thresholds of all the known strange particles. There is a great deal to be done in this energy range, much of which can be accomplished in beams of from 3 to 300 millimicroamperes average current; these figures bracket the best present and anticipated future performances of existing proton machines and those under construction. (For orientation, the Cosmotron and Bevatron now operate at about 2 and 6 millimicroamperes, respectively; the Argonne machine would produce 40 millimicroamperes at $10^{12}$ particles per pulse, and the Princeton-Pennsylvania accelerator would produce 300 millimicroamperes with $10^{11}$ particles per pulse at the high repetition rate of about 20 pulses per second. This latter current may be approached at Argonne, and also at Berkeley after modernizing the Bevatron injection system. In contrast, the big alternating gradient machines at Brookhaven and Geneva will probably not exceed a few millimicroamperes; the present performance at CERN is one-half a millimicroampere.) The work to be done includes nucleon-nucleon, pion-nucleon, K-nucleon, antinucleon-nucleon, and perhaps some hyperon-nucleon scattering, as well as the determination of production cross sections for various particles as functions of angle and energy. [Lack of this latter information is perhaps the greatest handicap in planning for

desirable secondary beams from future machines, and it is greatly to be hoped that some of it will be obtained early in the operation of the CERN and Brookhaven AGS machines.] The total and elastic differential cross sections just mentioned are easier to measure than the polarizations, which may therefore require higher currents, together with measurements on antihyperons as well as a number of processes involving virtual targets as proposed by Chew and Low. It is possible to enumerate many other types of experiments which also require higher intensities. An intense beam of antiprotons would be valuable for the study of antihyperons, since the cross section for making them in this way is probably about $10^4$ greater than by the use of pion or proton beams. Static properties of various baryons, such as the anomalous magnetic moments of antinucleons and hyperons, could be measured. Many processes involving weak interactions, such as decay processes involving small branching ratios, require high intensities. An interesting example is the possibility of measuring neutrino scattering, about which Professor T. D. Lee will speak in this room tomorrow morning. High energy muon beams, which could be obtained by passing a pion beam through several nuclear mean free paths of absorber, would have some interesting applications if they were quite intense. This enumeration is intended to be suggestive but not exhaustive. Higher currents have other obvious advantages. One could tolerate the greater attenuations inherent in better purification by beam separators. It is already known that through beam sharing and multiple target operation more can be done than if the full current on a single target is required for every experiment. It seems safe to conclude from all these remarks that there is a definite need for intensities at least as high as the

0.3 microampere I have mentioned above, which corresponds to what may be possible at the Argonne and Berkeley machines (after modernizing the injection system at the Bevatron) and for even much higher currents if we can learn how to cope with the many problems such higher currents will create by the time they could be produced.

These problems are by no means inconsequential. Among them is the shielding problem. People tend to think of this as merely a matter of how many tens of feet of earth or concrete must be used in constructing a house or tunnel in which the machine can be imprisoned. I recently came upon a calculation made at the Argonne Laboratory which illustrates the complexity of the shielding problem. There the linac injector beam will be introduced into the accelerator enclosure through a pipe whose area is four square inches, which passes through a 25 foot solid concrete wall, emerging about 15 feet from a possible target site in the big machine. To estimate the neutron flux produced at such a target coming back through this hole it was assumed that one percent of the $10^{13}$ particles per pulse would interact in this target, each producing 5 neutrons of which half are isotropic. The pipe's solid angle then admits $3.5 \times 10^5$ neutrons per pulse. Continuing from the accelerator backwards along the linac to the control room wall 160 feet further away one finds 800 neutrons per square centimeter per pulse. Twenty inches of concrete at this point will stop half of these; by assuming that three feet of steel and one foot of copper of the main

magnet shield the source of neutrons, the resulting estimated flux in the control room is reduced to one third of the tolerable flux. However, if the linac pipe happens to point at a straight section there would be a serious problem. Zig-zag tunnels carrying power and control wiring cause similar difficulties, and it must be remembered that this machine is assumed to produce one third of a microampere rather than the ten microamperes or more of which an FFAG machine is thought to be capable!

Another problem associated with operation at higher intensities is that of heat dissipation in targets. A ten microampere beam will lose about two kilowatts in traversing a nuclear mean free path. Targets used for the production of secondary beams must be very small in order to make possible the design of separating systems at these high momenta; this heat loss may be approaching a practical limit even for thin, refractory targets. An even more serious matter is the protection of machine, and especially such sensitive substances as coil insulation, from radiation damage due to lost or scattered beam. These and many other questions are bound to plague those who try to make plans for very high current machines.

My own conclusion is that inadequate study has been given as yet to a proper determination of feasible intensities, considering the system as a whole including experimental needs, technical difficulties, beam handling equipment, costs, and detection and analyzing apparatus. I have not touched on these last items as yet, and I do not feel myself to be at all expert in such prognostications. It seems clear, from the number of large bubble chambers being planned for use at such laboratories as Argonne, Brookhaven, CERN, Harwell, and in the Soviet

Union, that these instruments will be dominant in high energy physics for a number of years. They have certainly played an important role in the thinking of the study groups I have mentioned. It is much harder to predict to what extent other techniques will find general use, particularly in a period about ten years from now. As a sideline observer of luminescent chamber developments I have not yet understood how it is hoped to handle data of a complexity similar to that from bubble chambers which may be delivered, in principle, at microsecond intervals, and yet the existing state of data handling in high energy physics might have been regarded as fantastic only a decade ago. (Some of you who heard Dr. Bradner this morning may perhaps feel that it is fantastic now!) At the very least one can say that the capacity to handle experimental data is intimately related to the intensity question. I must reiterate that the question of intensities required for colliding beams has not been considered here. It is well known that presently discussed figures involve extrapolations beyond existing currents by a factor of a thousand. The proper way to prudently approach this impressive figure is a separate problem.

It is very difficult to predict the relative future importance of extending such measurements as have been discussed into the 25 billion volt region as compared with the importance of new, more interesting reactions that may occur at these higher energies. In any event the

arguments for high intensity will also apply there. In one respect the situation may be worse. It has been pointed out that the cross section for any reaction proceeding through a particular angular momentum state is proportional, among other things, to the square of the center of mass deBroglie wavelength which is inversely proportional to the laboratory energy for relativistic beams. Some of the production cross sections may turn out to be so low at 25 Bev as to greatly complicate or in some cases to prevent the design of usable separated secondary beams from the new AGS machines. One might feel it wise to wait and see what life is like at this energy before drawing conclusions.

Desirable Properties of Future Accelerators

Both of the groups I mentioned devoted some time to a discussion of properties that might be desired in a future accelerator to facilitate experimental work. It is not a difficult task for experienced experimentalists to define a supposedly ideal arrangement of beams, but accelerator designers may have a difficult time meeting all such specifications. In any case some of them will probably turn out to be wrong. On this subject, and in fact throughout these remarks, the experts and planners will find nothing new to them in what I have to report, but perhaps others may be interested to know what they are thinking about.

It is generally agreed that a flexible arrangement of multiple internal target and secondary beam facilities should be designed and set up as an integral part of the accelerator, together with their extensive shielding and the separators, focusing and analyzing magnets that will be required. It may be important to bring out secondary particles of both signs at small angles to the primary beam; considerable

advantages in studying phenomena as a function of energy will accrue if this can be done in a field-free region. Disposal of unwanted secondary particles downstream from their targets must be given careful attention. It is vital to intercept the full beam on a target of very small size. High luminosities may be obtained in this way by exploiting multiple traversals of the targets, particularly in machines with large momentum compaction which reduces the inward displacement corresponding to a given energy loss. This high luminosity from small targets facilitates the separation of particles with the same momentum but different mass. Many physicists who have not been following developments at the large accelerators might be surprised at the level of sophistication already achieved in the design and use of separators and, more generally, of beam handling devices. This subject was discussed this morning by Dr. Courant, and is well documented in the Proceedings of the CERN Conference which have recently been published. Large numbers of beams have been set up at the Bevatron and Cosmotron for various special purposes, and plans for a variety of beams at each of the major accelerators under construction are well advanced, each involving tens of quadrupoles, tens of bending magnets, several velocity spectrometers, tens of megawatts, and flight paths of up to two hundred feet or more.

External primary beams are being planned or are in use for many of these accelerators. They are inferior as sources of separated secondary beams but will have definite advantages for certain experiments. The availability of large solid angles and the full angular range is vital in studying primary interactions. Lack of interference with the machine and the possibility of stringing out several experiments provide

flexibility. Some r.f. bunching schemes become simpler in external beams. Absolute measurements can be made more readily, and shorter lived particles may be more accessible for study. It is desirable that such beams should be variable in energy and should have high optical quality. They must also be highly stable in position from pulse to pulse. Some of these specifications may be hard to meet. It has been noted that long straight sections of 15 to 30 feet may serve as effective substitutes for an external beam in some situations.

The importance of flexibility of beam duty cycles has been emphasized, since the needs of bubble chambers and of electronic detectors are different; even within each class there is a need for flexibility since counter control of chambers and time of flight work with electronic equipment may impose special requirements. It appears at first sight that FFAG accelerators hold an important advantage in this respect, since it should be possible in principle to vary the duty cycle and repetition rate of deflecting a stored beam onto a target almost continuously over a very wide range. However, the same capability can probably be obtained with a pulsed accelerator feeding a storage ring. The r.f. structure of an accelerated beam is important for some time-of-flight experiments; in one arrangement considered for an experiment at the Bevatron a peak-to-valley beam ratio of $10^3$ was required, but measurements showed that the actual ratio was smaller. On the other hand, even a moderately pronounced r.f. structure has an obvious bad effect on counter duty cycles, which can be avoided by slowly dribbling a coasting stacked beam onto a target. Magnet ripple may interfere with this process. All of these comments on duty cycles apply to both internal and external beams.

Conclusion

In concluding, I hope that I have been able to bring to those of you who have not been involved in day-to-day activity at a high energy accelerator an appreciation of some of the many factors which combine to complicate the task of planning for future high energy proton accelerators. I would be surprised if I have said anything new, and I have presented no final conclusions, but I do have the conviction that with the accumulation of new information and with continued study the picture will become clearer, and I am sure that this kind of study is important, both to prevent expensive errors and to make significant advances in our understanding of the nature of matter.