Ultrahigh-Energy Accelerators

Eventually physicists will push into the domain of superintensity as well as that of superenergy.

Robert R. Wilson

The discovery of the particles of physics and the study of their interactions comes as a culmination to the great developments in quantum mechanics and nuclear physics. This new field, sometimes called high-energy nuclear physics, is characterized by the elegance of both the experiments and the theory. Evolving from the Cambridge tradition of making simple measurements that are directly interpretable, the drive to examine the innermost structure of the nucleus and then of its constituent particles has resulted in the construction of nuclear accelerators of ever-increasing size and complexity. With each order-of-magnitude increase in energy, a new domain of physical experience has been made available. Nuclear structure, nucleon interaction, meson production, strange particles, and antiparticles are but some of the landmarks of this progression. The movement is still developing apace: two 30-Gev proton synchrotrons have just been completed and are beginning to yield most interesting results; a 10- to 20-Gev electron linear accelerator is in an advanced stage of planning; and a 60- to 70-Gev proton synchrotron is under construction.

We are, however, far from a basic understanding of the particles of physics, and this in spite of the rich detail already revealed concerning their properties by excellent experimental and theoretical work. The momentum of the activity already under way, the understanding that this knowledge adds to the stature and dignity of men, the justifiable pride of all concerned in a truly international accomplishment—these considerations and an intense desire and drive to finish what has been started will impel us to continue to work toward an eventual satisfactory understanding of the particles. There is no reason at all for optimism that such an end is near in any sense. Already there has been considerable discussion about the next stage of development. Will it be toward higher intensities of particles at presently attainable energies, or will it be toward the construction of ultrahigh energy accelerators—greater than 100 Gev, perhaps even in the range between 300 and 1000 Gev? Will progress in both directions be necessary—or in neither?

The cost and effort involved in any such construction will be significantly large. Consequently it seemed appropriate to have a discussion in which the broadest points of view were represented and in which the desirability of developing superenergy, from the point of view of the theory of particles, could be considered at the same time that the experimental practicality of constructing and using ultrahigh-energy machines was discussed.

Physicists from all over the world were assembled at the 10th conference on high-energy physics, held at Rochester, N.Y., in August 1960; some 30 or so of these physicists were invited to participate in a completely informal and unofficial session that was given over to intensive discussion of this problem.

We began our discussion by agreeing to keep in mind, but not to mention to each other, that the center-of-mass available energy of a system of two particles in which one is at rest varies as $(2\,ME)^{\frac{1}{2}}$; thus, a beam of 300-Gev protons corresponds to two 12-Gev proton beams opposing one another, and 1000 Gev is equivalent to two clashing 22-Gev beams or to about 44 Gev altogether in the center of mass—as compared to the presently available 7.5 Gev given by the 30-Gev machines.

It was natural to examine cosmic-ray evidence for a clue to new and exciting phenomena, inasmuch as such energies are still not large for the physicist working with cosmic rays. Although nothing of particular interest to the field of nucleon structure is indicated as yet, nevertheless it was felt that this represents inconclusive evidence because the number of events that have been examined has been very small and because the techniques for extracting information from cosmic-ray jets are not such that one could expect to be aware of physical processes that might be considered significant. The cross sections for interesting electromagnetic phenomena are known to be much too small to be determined from cosmic rays.

Ultrahigh Energy Particles

Turning from this not-very-productive exchange, we tried a more positive approach—consideration of what might be desirable about beams of ultrahigh-energy particles. Here I quote a résumé by Robert Oppenheimer of the sense of this part of the discussion.

"The clearest reason for a super-high energy machine is the same reason that motivated the present generation of accelerators, from the Gev electron synchrotrons, the electron linac and the cosmotron to the 30-Gev A.G. synchrotrons in Brookhaven and Geneva: we do not know what we shall find, what finer structure of matter, what new heavier ingredients. There are some new points.

"(1) In the past, cosmic rays were enough to reveal, but not fully to describe, new particles and new processes. This is not happening today and one can hardly be confident that it will, for new particles will probably be too short-lived and new processes too rare.

"(2) Our description of nuclear and subnuclear physics is incomplete, full of arbitrary and ununderstood numbers and parameters, and wide open; there appear to be essential clues that are missing, buried in high energy phenomena. Such are the nucleon 'core,' the

The author is on the staff of the Laboratory of Nuclear Studies, Cornell University, Ithaca, N.Y. Discussants at the informal meeting on ultrahigh-energy accelerators, on 28 August 1960, were as follows: L. W. Alvarez, E. Amaldi, R. F. Bacher, G. Bernardini, H. A. Bethe, B. Blochintsev, N. Bogolubov, G. F. Chew, G. Cocconi, R. L. Cool, M. Danysz, S. D. Drell, R. P. Feynman, D. Glaser, B. Gregory, L. Haworth, W. Heisenberg, W. Jentschke, N. Kemmer, M. S. Kozadaev, T. D. Lee, E. M. McMillan, R. E. Marshak, R. F. Mozley, S. Y. Nikitin, J. R. Oppenheimer, W. K. H. Panofsky, T. G. Pickavance, K. R. Symon, V. I. Veksler, W. D. Walker, and R. R. Wilson.

masses of the 'elementary particles,' the interaction constants themselves.

"(3) Highly unstable heavy particles will probably be found. Stable, or relatively stable, new particles, with new quantum numbers, may be analogous for the baryon-meson system of the μ-meson in relation to the electron.

"(4) Today we do not in any real sense understand the nuclear and subnuclear world. We think it likely that essential novelty will appear at the 'super-high energies' that will promote this understanding. We are confident that a knowledge of what does in fact occur in this domain will take us a long way toward this understanding."

To this we might add a few specific problems, such as the detailed investigation of form factors of particles, determination of the energy at which nucleon and antinucleon cross sections approach each other (that is, what new channels open up at high energy), and study of diffraction disintegration at 100 Gev or more. Also, it might be pointed out that just as the use of high-energy particles has helped clarify many essentially low-energy nuclear problems, so might ultrahigh energy help to shed light on our present problems at high energy.

On the subject of weak interactions T. D. Lee made the following comment.

"We know that if you take the present Fermi theory and extrapolate it to an energy of 300 Gev in the c. of m. [center-of-mass] system it will violate unitarity; it has to be wrong. Exactly at what energy it becomes incorrect is not known. If there is an intermediate boson it will become wrong at a c. of m. energy of about the mass of the boson. If there is no such thing, then the theory will become wrong at some intermediate energy above 100 Mev and below 300 Gev. Now 300 Gev c. of m. energy is very hard to obtain since if you convert it to laboratory energy, it becomes super-super high energy. On the other hand, if the breakdown is not much higher than the order of 10 Gev, then machine energies of the order of several hundred Gev's seem to be quite reasonable."

As to the probability of finding the essential clues to an understanding of subnuclear physics at superhigh energies, it will come as no surprise that the range of opinion between optimism and pessimism is fairly uniformly populated by physicists—but is shaded a bit toward optimism.

Accelerator Design

From these discussions, which seemed to indicate that it would be desirable to have particles of ultrahigh energy, we turned to a discussion of methods of producing such energies. What might be built, with our present knowledge and conventional techniques, with confidence of successful operation? This depends on the cost, of course, and it was generally agreed that for, say, $100 million—or at most $200 million—it would be feasible to push the design of a conventional alternating-gradient proton synchrotron to 100 Gev or even higher, and that this energy level might also cover the first round of experiments. With the same reasoning, but pushing the kind of tolerances that must be held, we could even think of attaining 1000 Gev, and at a cost of less than $1 billion—really a bargain, of course.

Circular electron accelerators seem to be out of the question at hundreds of Gev, so linear accelerators are in order. For these we were given the rough figure of $\$2.5 \cdot 10^6$ per Gev. The disadvantage of the linear accelerator lies in its poor duty cycle and its cost. At high repetition rates the duty cycle is not so bad and might be improved further by cooling the accelerator; furthermore, a new mechanism suggested by Drell indicates that electrons could be very effective in giving highly collimated beams of secondary particles such as pions or kaons. For protons, linear accelerators were felt to be too wasteful of radio-frequency power and too costly to compete with circular machines.

We can expect that some progress will be made in the art of accelerator design in the next several years, the effect of which will be to lower the cost of attaining ultrahigh energies below the costs predicted on the basis of conventional techniques and to reduce the predicted effort. One example of this is the tandem proton synchrotron under consideration now at California Institute of Technology. This uses a small-radius large-gap machine feeding into a large-radius small-gap machine. At the meeting, energy of 300 Gev was mentioned, at a cost of $125 million; however, this estimate was challenged by some, who felt that the accelerator itself does not represent the major part of the cost. This exchange brought out the point of view that, since the cost of the accelerator alone is likely to be about half the total cost of the laboratory, differences of costs between different types should not be taken too seriously. However, isn't it reasonable to look for something that sets the scale of the expense? There is another direction in which technical advances would lead to reduction of cost in accelerator construction—progress toward devices in which the alignment of the magnet is continuously maintained by servomechanisms. The time of traversal of the particles through the large magnets we are now discussing begins to get comfortably long, and thus new possibilities can be considered.

In general, the proton accelerators discussed above can be expected to give intensities ranging from 10^{11} to 10^{12} protons per pulse. K. R. Symon remarked that these intensities can be increased by a factor of at least 100 by using fixed-field alternating gradient methods, but at a cost of 1.5 to 2 times that of more conventional accelerators—a statement that did not go unchallenged by those who felt that the increase in intensity might be closer to 10.

It appears that we can be optimistic about the likelihood of improving conventional accelerators. If that is so, then what is the possibility of a really important innovation being made in the not-too-distant future? No one at the discussion knew of any development giving promise of such success. V. I. Veksler pointed to the possible use of interactions in which many particles collide with one particle, but said that theoretical estimates show this to be quite difficult to achieve. On the other hand, one should not entirely discount the possibility that plasma machines will be developed in the future. Plasma physics is still in its infancy, and developments may occur which could make construction of such machines feasible (*1*). Nevertheless, this possibility should not be given much weight. Less spectacular has been discussion of devices of very high magnetic field, especially those using cryogenic cooling of the coils or even the new superconductors. Although cooling the coils appeared to be an attractive idea at first, it now appears to be beset with difficulties, when the actual numbers and procedures are examined in some detail, as they were in a study recently made at Berkeley. Although we do not see significant innovations emerging at this time, apart from the use of the new superconductors, we must keep in mind the fact that good ideas have occurred regularly in the past.

Typical, though, of a really new development is the use of storage rings, which, when used in conjunction with a conventional alternating-gradient or fixed-field alternating-gradient synchrotron, could achieve colliding proton beams. Serious work based on this design has been going on for several years. At present it appears that for reasons of cost, design complexity, and experimental convenience, two-way accelerators would not be competitive with a conventional alternating-gradient synchrotron plus storage rings (*2*). It seems unwise to count on the *intensities* available from storage rings ever reaching those of conventional machines; for example, if the injecting accelerator were operating an intensity of $\sim 3 \times 10^{12}$ protons per pulse, and if the injecting and stacking process were carried out with very high efficiency, then a storage ring might equal the rate density of an alternating-gradient synchrotron operating at an intensity of 10^{10} protons per pulse on a liquid-hydrogen target.

For *exploratory* work at very high center-of-mass energies, storage rings appear quite promising, and their relatively low cost makes them attractive even though there are clearly many experiments in which it would be difficult to use them because of their low intensity. For counter experiments, it appears that the intensities which might be available in storage rings would be sufficient to permit detection of new effects or new particles but could never produce intense secondary beams. Furthermore, the secondary reaction products will not be produced at high energy. Inasmuch as colliding-beam devices seem much more limited with respect to the kinds of experiments that can be carried out, and because the technical development is, in fact, yet in its infancy, it seems clear that, for an energy range accessible to both, the conventional accelerator is considerably more desirable.

It was generally agreed at the meeting that, since we can now foresee the possibility of building accelerators with energies up to hundreds of Gev, corresponding to center-of-mass energies of perhaps 20 to 40 Gev, these and not colliding beams might be expected to furnish the next increase in energy. On the other hand, the colliding-beams technique might always be used to extend the center-of-mass energy of any accelerator, and in a really spectacular manner; hence, provision for installation of storage rings should always be considered when a new accelerator is to be constructed. It is unfortunate that the practical development of colliding beams has lagged so far that we do not yet know the limiting factors, and do not yet have the background of experience necessary for proper planning in this respect.

With regard to the question of building machines of superintensity as an alternative to machines of ultrahigh energy, the following might be said. It is perhaps a mistake to think of these as alternatives: both seem to be desirable. One can predict with considerable confidence that the neutrino experiments which can be made by using high energy (10 to 30 Gev) and very high intensities (the required intensity is not yet known) will constitute research into a really new field of knowledge. Exploratory measurements are already being designed, and these will naturally lead on to the next steps. Quite distinct from this is the construction of ultrahigh-energy accelerators, the subject of discussion at the meeting. However, it should be pointed out that production of secondary beams of high intensity may be most readily achieved by going to superenergies.

Experimental Techniques

The discussion livened up considerably as we turned to the question of the experimental techniques that would be available at ultrahigh energy. Some years ago physicists were pessimistic about experimenting with 30-Gev protons. It appeared from the discussion that their fears were not well grounded, inasmuch as the experiments now being made, while not easy, are not very much more difficult than the earlier experiments because of the many advances in technique which have accurred, and most of these techniques would also be useful at superenergies. Among these various advances the following can be mentioned. Cerenkov counters in which a 1-meter path of gas allows for the resolution of $\Delta\beta$ of $4 \cdot 10^{-4}$; spark chambers which have the advantage of responding to a triggered event; bubble chambers in which γ rather than β can be measured by observing the δ-rays from liquid hydrogen (use of a chamber 10 meters long would make this method particularly effective in distinguishing particles, but at not too large values of γ); radiofrequency separators which can be effective up to 20 Gev/c but which will be very expensive; crossed electric and magnetic field devices which can be used to separate particles up to about 10 Gev/c; and storage rings which can serve as particle separators by allowing the most unstable particles to decay (a device especially useful for preparing pure beams of antiprotons).

As the energy gets higher, new methods of distinguishing particles will become practical. For example, the increase in density of a track in a Freon bubble chamber with increasing momentum and, related to this, the lateral extension of the electric field may very probably be applicable to this problem. There is reason to believe that a number of new developments in technique will be made as specific problems in ultrahigh-energy physics arise.

It was, of couse, not possible in a limited time to go far in examining the kind of experiments that might actually be made. Nor was there very much agreement on exactly how to proceed with the experimentation. It was brought out that, in general, a large number of particles will come out in a core of very small angle, as in a cosmic-ray jet. Because much of the energy comes out as invisible π^0's, it would seem no longer possible to determine completely a particular interaction event, as is now possible in a bubble chamber below a momentum of about 1.5 Gev/c. To many present, this seemed no cause for despair. Nuclear physicists have long since learned to disregard what happens to atomic electrons during a nuclear collision. In a similar manner, in superenergy collisions we must learn to distinguish the particles or events with unusual properties from those that are simply obeying the rules of phase space and the laws of physics valid for low-energy phenomena. Distinguishing the very-high-energy particles that come out, or looking at quasielastic collisions were mentioned as examples. Where a particular model is being tested, it may be enough simply to sample average behavior in collisions. Despite the note of optimism on which this last part of the discussion closed (albeit with some mutterings and misgivings on the part of a few of those present), it is clear that particular experiments must be examined in detail to see that meaningful results can actually be obtained. We should be sure that the significant physical processes are not likely to be masked by backgrounds due to uninteresting particles also produced in the interactions.

Summary

Summarizing, one can say that the construction of ultrahigh-energy accelerators will be justifiable even at presently predictable costs. There is little doubt that eventually physicists will push into the domain of superintensity as well as that of superenergy. If they do this, it will be because the information that will then become available will be needed in order to formulate a more complete picture of nature. For the time being, we should examine the results of the 30-Gev synchrotrons in the light of their bearing on these large constructions of the future. But if these projects are to be realized in a time comparable to our lifetime, then those study projects which have become a necessary prelude to actual construction should be started now (*3*).

Notes

1. During a discussion with G. I. Budker concerning plasma instabilities and their deleterious effect on plasma accelerators, I asked if this did not mean we should be very pessimistic about any success. "Yes," said Budker, "but don't forget that a plasma is like a woman, the outlook can change most rapidly!"
2. K. Symon estimated that the cost of producing colliding beams of protons attaining 30 Gev in the center of mass would be about $200 million. The collision yield would be about 10^5 $cm^{-2}sec^{-1}$, with 10 percent gas background at 10^{-8} mm-Hg. G. K. O'Neill says that the cost of a storage ring set might be similar to that of the alternating-gradient synchrotron used as its injector; for example, addition of storage rings to the Brookhaven National Laboratory alternating-gradient synchrotron would cost approximately $35 million, for 60 Gev in the center of mass ("equivalent energy," about 2150 Gev).
3. I gratefully acknowledge the assistance, in preparation of this report, of R. F. Mozley and W. D. Walker, who prepared conference notes, and of Robert Oppenheimer, Gerald Pickavance, and Keith Symon, who sent additional comments and summaries.

Forthcoming Events

June

5–16. Operations Research and Systems Engineering, Baltimore, Md. (Dean, School of Engineering, Johns Hopkins Univ., Baltimore 18)

6–8. Tissue Culture Assoc., 12th annual, Detroit, Mich. (F. E. Payne, Dept. of Epidemiology, Univ. of Michigan, Ann Arbor)

8–11. American Electroencephalographic Soc., Atlantic City, N.J. (G. A. Ulett, Malcolm Bliss Mental Health Center, 1420 Grattan, St. Louis 4, Mo.)