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THE FUTURE OF HIGH ENERGY ACCELERATORS IN PHYSICS

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In this report I shall attempt to discuss some of the questions concerning the use of ultra-high-energy accelerators which might be expected to arise in the future. Clearly such remarks must be taken with a certain amount of scepticism : first, there is a natural amount of healthy disagreement on the subject; and secondly, we are working in a field where the rate of evolution of new knowledge often exceeds the rate at which the parameters of accelerators are extended. It has been true only rarely that the parameters of new accelerators were set for correct physical reasons : the notable exception is the Bevatron, whose energy was deliberately set above antiproton threshold; the postwar group of 300-500 MeV accelerators was initiated before the discovery of the pion (μ-pair production was frequently mentioned in connection with the choice of energy !); the Cosmotron energy was set before the discovery of K particles.

It is recognized that the production processes of the stable and unstable particles in the " next " energy region will become more multiple and therefore complex; on the other hand, the energy of 25 GeV (laboratory-energy) accelerators is not sufficient to attain multiplicities which allow reasonably clean statistical interpretation. Hence, unless new particles enter the picture it is unlikely that this field will retain its present interest. There exists a real possibility that the rise of productivity of the accelerators' as the energy is increased will not continue in the fields concerned with the study of strong interactions.

Few physicists, however, are willing to guarantee that the classification scheme of particles is now complete. Ignoring the question of the isotopic-singlet pion, a search for new particles is clearly indicated, although neither the energy region nor the means of search is clear. New particles might be exceedingly short-lived at higher energies, and in fact the transition from real to virtual particles is continuous. In addition, the required sensitivity of search is in general uncertain unless assumptions are made concerning the interaction involved. Two remarks in connection with electron machines are of interest here. First, the cross-section for the production of pairs of any assumed charged particle by photons can be predicted within an uncertainty depending only on the electromagnetic structure factors of the particle or its electromagnetic properties other than charge and also on the possible interactions between the created particles. Hence if a search with γ rays (the intensity required is high) is carried out, then a negative result has definite meaning regarding the " existence " of such a particle. Secondly, a possible means of search for particles of very short life exists via the interpretation of the spectra of inelastic electron scattering on the proton; new production thresholds will be reflected in such spectra.

There exists a group of experiments that are concerned with the structure of the fundamental particles or the interaction of single pairs of particles that require higher energies and in most cases higher intensities than are now available. I shall enumerate some of these possibilities :

1) Cross-section measurements using the extrapolation method of Chew, which effectively produce meson and K particle targets. The procedure becomes more effective if the " spectator " particle of a three-body final state has very low energy; this in turn requires very high energy of the bombarding particle.

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2) Electron-proton scattering should give more and more detailed information on nucleon structure and the associated problems at higher electron energies although the interpretation might become complex.

3) Despite the fact that the center-of-mass momentum transfer in μ-e, π-e, and K-e scattering processes induced by π, μ, or K beams of energy E_0 on stationary electrons of rest mass m_0 is only $(2m_0E_0)^{1/2}$, this momentum transfer becomes of an interesting magnitude (100 MeV/c) at incident energies above 10 GeV. Hence the measurement of energetic " knock-ons " at very high energies is a relatively straightforward means of investigating the structure of unstable particles.

4) The problem of an anomalous muon-nucleon interaction, which has been in a state of experimental ambiguity for a decade, can be attacked by comparing muon-proton scattering with electron-proton scattering at corresponding momentum transfers. To do this, both high energy and high intensity are required. Note that the production of a " pure " muon beam is much easier at energies greater than 10 GeV than at lower energies since the strongly-interacting particles can be removed by filtration.

5) The investigation of small branching ratios in the weak decay of unstable particles is primarily a matter of intensity of production of such particles. Thus far, the limit of such ratios is 10^{-6}, but much smaller fractions are of the greatest interest.

6) High-intensity high-energy beams make possible experiments on the static properties of artificially-produced particles. For instance, measurement of the precession rates of antiprotons and hyperon spins in magnetic fields is certainly possible with intensities somewhat higher than those now available.

In enumerating the qualitatively new problems which can be attacked if a new range of energy or intensity or both becomes available, I have possibly given the impression of underemphasizing the tremendous gaps in information existing in the measurements of interactions and production cross-sections, the branching ratios and asymmetries in weak decays. Among such gaps are :

1) Our knowledge of total cross sections and angular distributions of pions, antiprotons, and K particles scattered by protons and neutrons is very inaccurate and incomplete, in particular at high primary energies and large scattering angles. At the same time, the production of high-energy secondary-particle beams requires a large primary intensity since even at high primary energies the yield of high-energy secondary particles is small. We note that knowledge of such a yield, which is essentially the yield at low multiplicity, is itself of interest.

2) Systematic knowledge on antihyperons does not exist.

3) The K^o-$\bar{K}^o$ vs. θ_2-θ_1 complex (including the mass differences) remains unexplored in detail. Further exploration is limited primarily by lack of intensity.

4) Polarization measurements of recoil products give valuable information in many cases and are not available.

5) Reactions in which a K particle is associated with a $\bar{K}$ rather than a hyperon are virtually unknown; their systematic study would be sensitive to the K-$\bar{K}$ interactions.

6) Machine-made energies will be large enough to explore the validity of the models describing the multiple production of secondary particles.

Knowledge on photoproduction of particles other than pions is extremely fragmentary; in particular, the data on K photoproduction are so scanty that some very fundamental questions remain open that could be answered by more copious data of this type. Although the K photoproduction data can become more complete with existing machines, the photo-excitation data of strange-particle and antiparticle processes other than K-hyperon production must await the completion of the multi-GeV electron machines. Note that in general photo-excitation data are more susceptible to theoretical interpretation than heavy-particle production data since only one nucleon is involved in the initial state.

In this connection I should like to mention the significance of electron production of new particles.

The ratio of cross-sections of electron-production to photoproduction of new particles gives the ratio of yields produced by virtual (i.e., non-zero mass) and real photons; this in turn serves to explore the spatial distribution of the electromagnetic property of the system which initially absorbs the photon, and also helps to establish the multipolarity of the photon involved.

Finally, I should like to comment on the experiments on the limit of validity of quantum electrodynamics, which is the one area in fundamental particle physics where experiment and theory are in exact quantitative agreement for the full range of energies explored to date. If one believes that special relativity remains valid, then deviations from theory in processes involving photons and electrons must involve intermediate states which are " far off the energy shell, " i.e. involve virtual photons or electrons of large invariant four-momentum. This fact also implies that very small cross-sections will necessarily be involved.

Experiments on the limits of quantum electrodynamics fall into three classes :

1) Colliding-beam electron-electron scattering experiments.

2) Experiments in which the invariant momentum transfer is increased by using a heavy particle (proton) to decrease the motion of the center of mass. The properties of the proton are eliminated from the process by using the experimental results from electron scattering. Experiments in this class are large-angle electron-positron pair production, bremsstrahlung with the photon emitted at large angles, and wide-angle pair production by electrons (tridents). Such experiments require high-intensity electron beams, but not necessarily high energy.

3) Experiments in which, despite the center-of-mass motion, a high invariant momentum transfer is assured by sufficiently large primary energy. The primary interest in such experiments begins at about 10 GeV laboratory electron or photon energy. Included in this group are electron-electron scattering, positron-electron scattering and annihilation in flight, and the electron-photon Compton effect. In principle such experiments could be carried out at lower energy and at high accuracy; however, both the experimental problems and the uncertainty of the higher-order theoretical corrections make this impractical. At the very highest energies, ambiguities might again arise between possible breakdown in quantum electrodynamics and the uncertainty in the calculation of corrections.

The summary of experiments given here includes problems which require either higher intensity than is now available, or higher energy, or both; and some of the experiments require protons and some electrons. This illustrates that even in terms of experiments which can be definitely projected, a diversity of tools remains necessary. If one adds the fact that the actual questions of greatest importance might well have shifted substantially in six to ten years, one is forced to conclude that we cannot afford to emphasize a single direction in high-energy instrumentation.

This remark applies both to the accelerators and to the methods of detection : the different questions demand different solutions. I should like to discuss this question briefly. At the highest energies there are three principal problems different from those usually encountered in low-energy detection : 1) the problem of distinguishing highly relativistic particles; 2) the large multiplicity of possible reaction channels; 3) the frequent importance of a very small fraction of the total number of events.

In addition to these problems, there are those of signal vs. background and resolution vs. transmission, which are in principle common to lower-energy experiments, but are in practice frequently difficult to handle because of the large cost and weight of shields or magnetic-analyzing devices.

The problem of particle identification is still essentially unsolved for the highest velocity particles. Progress has been made in terms of Cherenkov counters of controlled refractive index, radio-frequency selectors, and the rare-gas ion chambers. In addition, as knowledge increases, identification becomes easier by observations of interactions or decay modes. Nevertheless, until this problem is in a more satisfactory state, any comparison of detection methods for multi-GeV particles is clearly premature; in particular, the need for diverse methods of attack remains paramount.

I hope that the foregoing discussion has shown that although some of the traditional fields of experimental and theoretical interest in high-energy physics may be less productive as the next range in energy and intensity is explored, there is very strong reason to believe that other, less explored areas will be the source of important results. Hence the time cannot be predicted when physicists will feel that the study of high-energy physics has passed its peak of fruitfulness.

STUDIES AT BERKELEY AND MURA ON FUTURE HIGH ENERGY PROTON ACCELERATORS

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I. INTRODUCTION

The general thesis that there is a need for additional particle accelerators needs no defense at this conference, and is particularly obvious to all who are aware of the crowded experimental schedules at each of the major existing machines, the rather small number of really definitive experiments which can be conducted at any one of them each year, and the great mass of unknown facts that will be required to solve the most challenging physical problems of our time. In the sessions to follow it will be demonstrated that ample inventiveness has been applied throughout the world to produce a whole new generation of accelerator concepts and techniques, so that the new accelerators about which we speak will certainly be freed of many of the limitations affecting the productivity of the machines now operating or under construction. In the not-so-distant past it was possible for each innovator, and those associated with him, simply to convert his concepts into hardware and apply the results to the discovery of new physical facts. However, we have rapidly progressed to the stage where the feasibility of an accelerator proposal can only be determined by the detailed application of technical, engineering, and computing skills as well as of intuition and experience in the accelerator art. A more disturbing phase of our progress into the presently feasible range of particle energies is that the equipment required is so large and costly as to require difficult decisions of national and even international policy to finance its construction and use.

In this situation it becomes increasingly important to assure that all available 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 high-energy physicists have given serious thought to these problems, and it is appropriate that they should arrive at a diversity of answers. I have been asked to report in this paper on some of the points which have been brought out in a series of informal meetings of a group of physicists at the Lawrence Radiation Laboratory at Berkeley during the past year, and in a more intensive, one-week