RESEARCH ARTICLE OPEN ACCESS

The Future of High Energy Accelerators in Physics

Sarita Negi[1], Smita Singh[2], S.K.Goel[3]
Dept. of Physics, Integral University, Kursi Road, Lucknow, India

Abstract:

In this report I shall attempt to converse certain of the questions regarding the use of ultra-high energy accelerators which may be likely to rise in the future. Evidently such remarks must be taken with a definite amount of scepticism: main, there is a natural amount of healthy disparity on the subject; and furthermore, we are working in a meadow where the rate of development of new knowledge frequently surpasses the rate at which the parameters of accelerators are prolonged. It has been true only rarely that the parameters of new accelerators were set for correct physical explanations: the noteworthy exemption is the Bevatron, whose energy was intentionally set overhead antiproton threshold; the post war group of 300-500 MeV accelerators was instigated before the discovery of the pion (μ-pair production was frequently stated in connection with the choice of energy!); the Cosmotron energy was set earlier the discovery of K elements.

Keywords — **Particles, scattering, High Energy Accelerators.**

INTRODUCTION

It is acknowledged that the production methods of the stable and unstable particles in the "next" energy region will become more manifold and therefore complex; on the other hand, the energy of 25 GeV (laboratory-energy) accelerators is not adequate to achieve diversities which permit rationally clean statistical interpretation. Henceforth, unless new particles enter the picture it is implausible that this field will maintain its present interest. There exists a real possibility that the rise of productivity of the accelerators' as the energy is augmented will not continue in the fields worried with the study of strong exchanges.

Few physicists, though, are willing to assurance that the grouping scheme of particles is now complete. Disregarding the question of the isotopicsinglet pion, a search for new particles is clearly designated, although neither the energy region nor the means of search is clear. New particles might be exceptionally short-lived at higher energies, and in fact the transition from real to simulated particles is constant. In addition, the requisite sensitivity of search is in general undefined unless assumptions are made concerning the interface involved. Two remarks in connection with electron machines are of interest here. First, the cross-section for the production of pairs of any presumed exciting particle by photons can be projected within a hesitation dependent only on the electromagnetic structure factors of the particle or its electromagnetic properties other than charge and also on the possible interactions amongst the formed particles. Henceforth if a search with y rays (the intensity essential is high) is carried out, then a negative result has certain meaning concerning the "existence" of such a particle. Furthermore, 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 replicated in such spectra.

There exists a group of experimentations that are concerned with the structure of the important units or the interface of single pairs of particles that requisite higher energy and in most cases greater intensities than now exist. I shall calculate some of these prospects:

1) Cross-section measurements using the extrapolation technique of Chew, which efficiently produce meson and K particle targets. The process becomes more operative if the "spectator" element of a three-body final state has very low energy; this in turn needs very high energy of the bombarding particle.

2) Electron-proton scattering must give more and more detailed info on nucleon structure and the related problems at higher electron energies though the interpretation could become complex.

3) Despite the fact that the centre-of-mass momentum transfer in μ-e, π-e, and K-e scattering procedures induced by π, μ, or K beams of energy E_o, on stationary electrons of rest mass m_o, is only $(2m_oE_o)$^1/2, this momentum transfer becomes of an stimulating magnitude (100 MeV/c) at incident energies above 10 GeV. Hence the measurement of spirited "knock-ons "at

very high energies is a comparatively upfront means of examining the structure of unstable particles.

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

5) The investigation of small branching ratios in the weak deterioration of unbalanced particles is chiefly 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 probable experiments on the static properties of artificially-produced particles. For example, measurement of the precession rates of antiprotons and hyperon spins in magnetic fields is definitely possible with intensities slightly higher than those now existing.

In computing the qualitatively new difficulties which can be confronted if a new range of energy or intensity or both becomes accessible, I have conceivably given the impression of understating the tremendous gaps in information existing in the measurements of interactions and production cross-sections, the branching ratios and irregularities in weak declines. Amongst 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 imprecise and incomplete, in particular at high primary energies and large scattering angles. At the same time, the production of high energy secondary-particle beams needs a large primary intensity ever since even at high primary energies the produce of high-energy secondary particles is small. We note that knowledge of such a yield, which is fundamentally the yield at low diversity, is itself of interest.

2) Systematic knowledge on antihyperons does not exist.

3) The K^θ- K^0 vs. $θ_2$-$θ_1$, complex (comprising the mass alterations) rests unexplored in detail. Additional exploration is restricted chiefly by lack of intensity.

4) Polarization measurements of recoil products give valuable info in several cases and do not exist.

5) Reactions in which a K particle is related with a Ψ if rather than a hyperon are nearly unknown; their systematic study would be sensitive to the K-Ψ interactions.

6) Machine-made energies will be large sufficient to discover the validity of the models relating the numerous productions of inferior particles.

Information on photo production of particles other than pions is tremendously fragmentary; in specific, the data on K photo production are so flimsy that some very fundamental questions remain open that could be replied by more copious data of this kind. Although the K photo production data can become more complete with remaining machines, the photo-excitation data of strange-particle and antiparticle procedures other than K-hyperon production must await the accomplishment 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 intricate in the initial state. In this assembly i should like to reference the importance of electron production of new particles.

The ratio of cross-sections of electron-production to photoproduction of new particles gives the ratio of produces formed by virtual (i.e., non-zero mass) and real photons; this in turn helps to discover the spatial distribution of the electromagnetic property of the system which originally absorbs the photon, and also helps to inaugurate the multipolarity of the photon complicated.

Lastly, i should like to mention 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 precise quantitative agreement for the full range of energies discovered to date. If one believes that special relativity rests valid, then deviations from theory in procedures involving photons and electrons need to include intermediate states which are " far off the energy shell, "]i.e. implicate virtual photons or electrons of large invariant four-momentum. This element also indicates that very small cross-sections will essentially 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 transferal is augmented by using a heavy particle (proton) to reduction the motion of the centre of mass. The properties of the proton are eradicated from the procedure by using the investigational 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 experimentations need high-intensity electron beams, but not essentially high energy.

3) Experimentations in which, although the centre-of-mass motion, a high invariant momentum transfer is guaranteed by necessarily large primary energy. The primary interest in such experiments begins at about 10 GeV laboratory electron or photon energy. Comprised in this group are electron-electron scattering, positron-electron scattering and annihilation in flight, and the electron-photon Compton Effect. In principle such experiments might be carried out at lower energy and at high accurateness; though, both the investigational difficulties and the uncertainty of the higher-order theoretical corrections make this unfeasible. At the very highest energies, uncertainties might yet again arise amongst possible breakdown in quantum electrodynamics and the ambiguity in the calculation of rectifications.

The summary of experiments given here comprises problems which necessitate either higher intensity than is now available, or higher energy, or both; and some of the experiments need protons and some electrons. This exemplifies that even in terms of experiments which can be absolutely projected, a diversity of tools remains essential. If one adds the fact that the actual questions of greatest significance might well have shifted considerably in six to ten years, one is forced to accomplish that we cannot afford to accentuate a single direction in high-energy instrumentation.

This observation applies both to the accelerators and to the approaches of detection: the diverse questions demand dissimilar solutions. I should like to confer this question concisely. At the highest energies there are three principal problems different from those usually encountered in low-energy detection: I) the problem of differentiating highly relativistic particles; 2) the large multiplicity of possible reaction channels; 3) the frequent significance of a very small fraction of the total number of occasions.

In tallying to these difficulties, there are those of signal vs. background and resolution vs. transmission, which are in principle communal to lower-energy experiments, but are in practice regularly problematic to handle for the reason that of the large cost and weight of shields or magnetic-analysing devices.

The problem of particle identification is still fundamentally unsolved for the highest velocity particles. Growth has been made in terms of Cherenkov counters of controlled refractive index, radio-frequency selectors, and the rare-gas ion chambers. In totalling, as knowledge grows, identification befits easier by observations of interactions or waning modes. Nonetheless, until this problem is in a more pleasing state, any comparison of detection approaches for multi-GeV particles is evidently premature; in specific, the need for diverse approaches of attack remnants paramount.

CONCLUSION

I hope that the foregoing discussion has revealed that while some of the traditional fields of investigational and speculative interest in high-energy physics might be less productive as the next range in energy and intensity is discovered, there is very strong reason to have faith in that other, less explored areas will be the source of significant results. Hence the time cannot be predicted when physicists will sense that the learning of high-energy physics has delivered its peak of effectiveness.

REFERENCES

1. HIGH ENERGY ACCELERATORS IN PHYSICS W. K. H. Panofsky, Stanford University, Stanford, Calif. Page No: 3- 6.
2. Eidelman, S., et al. (2004) Review of Particle Physics. Physics Letters B, 592.
3. Nakamura, K., et al. (2010) (PDG), JP G 37, 07502.
4. Tavernier, S. (2010) Experimental Techniques in Nuclear and Particle Physics. Springer, Berlin Heidelberg.
5. B. Liu, et al., Proceedings of 2005 Particle Accelerator Conference, Knoxville, 16-20 May 2005, p. 1.
6. S. Fukuda, et al., Particle Accelerators, Vol. 27, 1990, p. 145.