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Remarks on Future Accelerators and Future Physics*

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I. INTRODUCTION

These remarks will be personal and subjective, and not confined to theory. Professor Salam already has most beautifully described, from the theorist's point of view, the need for pushing to higher energies, say 100 TeV cms. And many speakers have described the practical constraints and the very imaginative ideas on how to get there. My own recent interests have been diverse, but include some dabbling in accelerator physics. But, as a naive amateur, I have little if anything of substance to contribute to accelerator science per se. All I am prepared to do is to recount my own experience and viewpoint.

Last year, while attending the Fermilab summer school on accelerator physics,¹ I tried to look at the problem of ultrahigh energy from the perspective of the far future, namely, the years 2000-2020. If we keep up in energy with the Livingston-curve projection, it is reasonable to expect that present-day technology will have been abandoned, and that new techniques will have emerged. Ideally, the new techniques must be powerful in luminosity as well as energy. But those demands are sufficiently heavy that the possibility of alternatives using old technology in highly cost-effective ways cannot be easily dismissed.

Anyway, this year I was asked to lecture at the NATO Advanced Study Institute at Lake George, N.Y. on a topic of my choice.² I chose accelerators and decided to try a paper "design" of a conventional $p\bar{p}$ collider of $E_{\text{cms}} \sim 10^3$ TeV, just what is needed, according to Livingston, in the year 2020. And perhaps the NATO budget is big enough to pay for it. Such a 2020 ring, however clumsy, is a nice pedagogical machine.

Not only does it exhibit how typical parameters scale with energy, but it also produces enough synchrotron radiation that problems endemic to existing electron machines are prominent.

The Lake George experience was useful to me (even if less than optimal for the other participants) in appreciating that, if one blithely ignores the problem of cost, it is not at all out of the question that conventional technology still may provide machines that -- at least on paper -- work at such extreme energies. There is, therefore, reason to study most seriously whether cost-saving techniques associated with economies of scale and/or technical innovations may make large "conventional" machines affordable. Indeed, while I was lecturing at Lake George, real experts gathered at Snowmass, Colorado and explored such an approach using superferric magnet technology, with the optimists^{3,4} projecting 20 TeV center-of-mass energies for under a billion dollars. This is a most encouraging prospect.

Nevertheless the urge for other innovative approaches remains, and as I write up the Lake George lectures I find myself craving some kind of high-gradient linear device. Thus it is for me a special pleasure and privilege to learn at this meeting the problems and promise of such approaches.

II. PHYSICS

I share Prof. Salam's skepticism regarding existence of a "desert" stretching between 1 TeV and 10^{12} TeV, containing essentially no new physics. It is, however, not too easy to provide solid energy landmarks

for the accelerator designer once one exceeds the 100 GeV-1 TeV intrinsic mass or energy scale. Figure 1 shows a sketch of some fuzzy landmarks.⁵ One sees that the Salam goal of 100 TeV center of mass energy is a reasonable match to this collection of speculations.

Many speakers at this meeting have stressed the importance of high luminosity when going to very high energy. Interesting cross-sections for processes having an intrinsic mass scale $\geq M$ tend - essentially from dimensional analysis - to have cross-sections $\sigma \lesssim M^{-2}$. Certainly it is prudent - especially given the large cost of any new ultrahigh-energy facility - to make every effort to reach this level of sensitivity. However, historically it seems to me that both experimentalists and theorists have been overconservative in estimating in advance the magnitude of "interesting" cross-sections. Thus I myself put relatively large emphasis on energy, even at the expense of luminosity.

Indeed, if we consider the new data from the SPS $p\bar{p}$ collider, perhaps the most puzzling phenomenon is the large cross-section for production of high transverse-energy events which are not at all jet-like. At a level of $\sim 10^{-3}$ to 10^{-4} of the total cross-section (for which a luminosity $\sim 10^{25} \text{ cm}^{-2} \text{ sec}^{-1}$ is sufficient!), ~ 50 GeV of energy can be isotropically deposited in a central calorimeter.⁶ This phenomenon grows in importance as the energy increases, and is at present not at all understood. How does all that energy in the beams turn the corner and come out at large angles? Are we to ignore study of this phenomenon just because it does not conform to simple ideas regarding hard collisions of constituents?

On the other hand, the beautiful data⁷ on the high- p_T jet events from UA1 and UA2 does clearly signal (at long last) existence of hard collisions of hadron constituents in $p\bar{p}$ interactions. Subenergies in excess of 100 GeV have already been observed; this will not be attained in e^+e^- collisions for nearly a decade. And in the next several years the $p\bar{p}$ integrated luminosity should increase by several orders of magnitude and the $p\bar{p}$ energy by a factor $\sim 3-4$. The use of hadron-jet phenomena to study basic processes in pp and $p\bar{p}$ collisions looks extremely promising.

III. PROSPECTS FOR CONVENTIONAL TECHNOLOGY

For the next twenty years it has been projected⁸ that we can continue to use existing technology to reach center-of-mass energies in the 10-50 TeV range. To be sure, such machines may be monstrously large and quite expensive, but designers seem to agree that they will work.

Would much larger conventional machines work? At Lake George, we chose for study a 500x500 TeV $p\bar{p}$ collider to learn some of the scaling laws for machine design, as well as to examine the role at such extreme energies of more fundamental problems, such as synchrotron-radiation damping, beam-current limitations, and beam-beam limits. Such a machine is so extreme that no one could or should believe that I consider it seriously. A brief parameter list is given in Table I. Ten tesla magnets were assumed as state-of-the-art; this gives a circumference of ≥ 1100 km. It is inappropriate here to go into detail. The study, carried out by amateurs, is certain to be rather superficial.

But some of the features of interest may be worth some comment here.

1. The synchrotron-radiation damping time is short, ~ 10 -20 minutes, but the phenomenon appears to be a nuisance. A momentum spread large compared to the natural spread from radiation damping is desirable in order to combat, via Landau damping, the single-beam microwave instability. And it is desirable that the nominal transverse emittance in the machine should be large compared to the "natural" emittance emergent after synchrotron damping in order to maintain a reasonable beam-beam tune shift. And of course synchrotron radiation and 10T superconducting magnets do not peacefully coexist.⁹
2. Assuming the above problems can be overcome by externally increasing both longitudinal and transverse phase volume, one might attain a luminosity per bunch crossing $L_0 \sim 10^{28} \text{ cm}^{-2}$. With 400 p and \bar{p} bunches (one bunch per betatron wavelength¹⁰) each with 5×10^{11} particles and a revolution frequency of 250 Hz, this would (optimistically) give a luminosity of $\sim 10^{33} \text{ cm}^{-2} \text{ sec}^{-1}$. However the large value of L_0 produces $> 10^3$ interactions per crossing and major headaches for the experimentalist.¹¹ At such a rate, one must abandon tracking and probably even observing muons behind absorber. However one may be able to use electromagnetic and hadronic calorimetry. At the 10^3 rates the mean energy deposition in a typical calorimeter element subtending a solid angle $\sim 10^{-1}$ steradians (large enough to contain a hadronic jet) might be hundreds of GeV. However, the fluctuations about the mean are Poissonian, and if a 1 TeV threshold for accepting jets is adopted, the background appears to be acceptably

small. And 1 TeV out of a total available energy of 10^3 TeV is indeed a reasonable minimum energy.

This exercise may have implications at lower energy, although the problems clearly worsen. The main point is that the highest luminosity need be utilized for only the highest energy subprocesses and hence the highest energy jets. If calorimetric methods suffice to observe these jets, a certain amount of "minimum-bias" background will be tolerable. Careful simulation will be needed to determine the level which can be tolerated. And experimentally one may look at the dependence of signal upon luminosity to measure the amount of pileup.

3. The scaling laws, on paper, for effects of field errors, misalignments and the like seem to go 12 , for fixed magnet technology, as E_{cms}^n , with $n < 0$. No doubt this is naive, but at least there appeared no impending manifest disaster as energy increases. This phenomenon, apparently well-known to accelerator designers, came to me as a pleasant surprise.

Nevertheless the machine, with a cost $\geq 10^{2.5 \pm .5}$ TeVatron units, is a monster. Either very major cost-savings must be found or else one must go to new technology.

Cost-saving methods might not be utterly out of the question. The Snowmass studies of machines consisting of very long superferric $2.5 \pm .5$ tesla magnets contained in simple pipes claim 3 savings in unit costs of a factor $\sim 6-7$. If these claims, which need a lot more detailed study, hold up there may be considerably more longevity for the "old" technology.

IV. HIGH GRADIENT LINEAR ACCELERATION

Nowadays ideas on high energy linear acceleration tend quite naturally to emphasize electron acceleration in order to avoid the difficulty of building e^+e^- storage rings with energies much higher than LEP. Even a major breakthrough in technique along the lines discussed at this meeting might lead to energies "only" on the TeV scale. Given the highly productive history of e^+e^- colliders with cms energies small compared to energies contemporaneously available in hadron machines, it makes clear sense to vigorously pursue this direction.

However, the name of the game remains high energy, and if we wish to reach cms energies in excess of 100 TeV, this may not suffice. Speaking here as the naive, greedy theorist, I would argue that if we opt for new technology to reach for the highest energies, it must promise, however vaguely, to eventually attain the 100 TeV cms energy regime. Otherwise we can probably survive at the lower energies, most likely as easily, as economically, and as quickly using tried-and-true technology.

Therefore, the "specification" that (again speaking as naive theorist) I would like to impose is an accelerating gradient in excess of 10 GeV/m. This outrageous requirement may well exceed all kinds of fundamental limits. Certainly this gradient, more appropriately described as $1 \text{ eV}/\text{A}^\circ$, is sure to wreak havoc with any material in the neighborhood of the accelerated beam. And there seem to be some theorems¹³ which say there had better be some matter (or plasma) near the beam or else there will be no acceleration: pure radiation field is not enough.

But along with such a drastic specification, I would like to suggest loosening of other traditional specifications. In particular:

1. In the beginning (e.g. for a few years?), luminosity should not matter. A few accelerated particles per day with energy ≥ 10 TeV would suffice, especially were those particles to be protons. They could be made to interact in a visual detector, with some modest output of physics results. With any kind of growth potential, fixed-target physics results could help stimulate the accelerator improvements that would be sure to follow any initial success.
2. Again, especially for protons, but also for electrons, beam quality does not matter much; $\Delta p/p \sim 100\%$ should, for example, be considered acceptable.
3. Given the above easygoing attitude, one need not require that a single pulsing of the accelerator leave it undamaged - provided that replacement or repair of the damaged portion can be done reasonably quickly and economically.

It has been repeatedly emphasized that evolution of a novel technique for reaching high energies is difficult to realize as a pure accelerator-R&D project without attainable and interesting physics goals en route. In this respect, proton acceleration may have a slight edge over electron, inasmuch as each proton interaction is "interesting," whereas most electron interactions are pure electromagnetic.

What about the device itself? Most likely the injected beam should be highly relativistic with low emittance. Assuming its transverse dimensions are submillimeter, this allows in principle a structure of no more than order millimeter transverse dimensions. Since the required stored energy is at least 1 J/mm^3 there is a high premium on keeping

transverse dimensions small. If this can be done, (and with the efficiency somehow high), the total delivered energy might not be more than 1 MJ/km. The peak power is, however, enormous; this energy optimally should be locally delivered in some small number of picoseconds, implying peak powers at least in the range of hundreds of gigawatts. At least in the field of laser physics, such numbers are not outrageous.

But these ravings are outrageous enough as they stand. I have no idea whether there is any credible mechanism of acceleration within such guidelines, although Palmer's laser acceleration¹⁴ is not too dissimilar in spirit. I would guess that one of the most important criteria for the device is that it be periodic, with a high degree of reproducibility of field profile among the basic longitudinal elements, however complex that profile might be.

V. ACTION

How much work on advanced accelerator R&D can we expect in the future? The discussion in conjunction with the talk of John Adams regarding the difficulty in attracting young European physicists into accelerator science was disheartening. I somehow feel the U.S. situation to be slightly better, with young experimentalists (and even occasional theorists) at SLAC, Fermilab, Cornell, and elsewhere increasingly drawn in. But it is only a start. Despite the recommendations of distinguished committees, it is difficult - especially in these demanding times - to divert funds into speculative,

pure R&D ventures with little expectation of short-term payoff. This meeting has also shown the likely importance of cross-fertilization and close contacts with other fields; laser and plasma physics especially come to mind.

Another neglected area, in my opinion, is in teaching. Perhaps it is only my advancing age, but I found it difficult to locate up-to-date, lucid introductory material on the fundamentals of accelerators. Often the best material is in unpublished reports (sometimes out of print) or even internal laboratory memoranda. The archival literature is spotty. If the subject were more easily approachable, perhaps more physicists would approach it.

Actually the situation is improving. The ICFA workshop proceedings are excellent new sources of material on problems at the highest energies. Summer schools on accelerator physics have been held in the U.S. for the last two years¹⁵; these contain many excellent lecture series. And finally this meeting itself has been extremely valuable in helping to assess the long range prospects. To the organizers goes not only my expression of gratitude but, I am sure, that of all participants.

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3. R. Huson, et.al. and references therein, Proceedings of the DPF Workshop on Future High Energy Physics and Facilities, Snowmass, Colo., July, 1982 (to be published).
4. L. Lederman, presentation to the DPF Workshop on Future High Energy Physics and Facilities, Snowmass, Colo., July 1982.
5. For more detailed discussion of the physics underlying this plot, see reference 1; also J. Bjorken, Physica Scripta 25,69 (1982).
6. UA1 Collaboration, G. Arnison et.al., CERN preprint CERN-EP/82-122, to be published in 1982 Paris Conference.
7. UA2 Collaboration, M. Barnes et.al., CERN preprint CERN-EP/82-141, to be published in Physics Letters.
8. For example, cf. Proceedings of the Second ICFA Workshop on Possibilities and Limitations of Accelerators and Detectors, ed. U. Amaldi, CERN RD/450-1500 (1980).
9. It appears that warm scrapers interspersed between groups of magnets may allow the synchrotron-radiation to be removed without loading the cryogenics.
10. We use a scheme of (electrostatic) kickers to separate p and \bar{p} bunches at the unwanted crossings. This was studied in some detail at the Snowmass workshop; cf. R. Diebold et.al., Argonne preprint

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11. For a discussion of the headaches, see L. Lederman and R. Schwitters; appendix to reference 4.
12. N. King, ref. 8, p. 125; and L. Teng, Proceedings of the 11th International Conference on High Energy Accelerators, Geneva, 1980, p. 910.
13. J. Lawson, Report RL-75-043, Rutherford Labs, Chilton, U.K., (1975), IEEE Transactions on Nuclear Science NS-26, 4217 (1979); P. Woodward, Journal IEE93, Part IIIA, 1554 (1947).
14. R. Palmer, Particle Accelerators 11, 81 (1980).
15. "Physics of High Energy Particle Accelerators" (Fermilab Summer School, 1981), AIP Conference Proceedings No. 87, ed. R. Carrigan, F. Huson, and M. Month (1982). Still to be published is a second volume from this school as well as the proceedings of the 1982 SLAC Summer School on Particle Acceleration.

TABLE I. Some Parameters for a 500x500 TeV $p\bar{p}$ Collider

Magnetic field	10 T
Radius	170 km.
Tune	400
Revolution frequency	250 Hz
RF frequency	500 MHz
Synchrotron frequency	1 Hz
Synchrotron radiation loss per turn	3 GeV
Radiation damping times	10,20 min
Particles per bunch	5×10^{11} (?)
Number of bunches	400
Beam-beam tune shift	.010 (?)
β -function at collision points	20 m
Luminosity per crossing	10^{28} cm^{-2} (?)
Luminosity	$10^{33} \text{ cm}^{-2} \text{ sec}^{-1}$ (??)

FIGURE CAPTION

Fig. 1 Energy landmarks for future accelerators (taken from references 1 and 2).

