FERMILAB - Conf - 84/73-T

SSC PARAMETERS: WHAT PHYSICS DEMANDS

C. Quigg
Fermi National Accelerator Laboratory, Batavia, Illinois 60510

Summary

Issues of energy and luminosity for the SSC are briefly reviewed.

Introduction

Two years have passed since the idea of a hadron supercollider was born here at Snowmass. A great deal has happened in the interim. The goal of the SSC has been enthusiastically embraced by the particle physics community, the Reference Designs have been completed, an umbrella organization has been put in place for the R&D phase, and leaders of that effort have been identified. It seems appropriate at this time to take stock of where we are going by posing the questions

- Why are we doing this?
- What is required to accomplish our physics objectives?
- Can experiments be done under the required conditions?

The Physics Motivation

The developments of the past decade have brought us to a new level of understanding of the fundamental interactions. Many of the experimental results on which this new understanding is founded were themselves made accessible by a new generation of accelerators which opened new energy frontiers. We now appreciate that quarks and leptons are the fundamental constituents of matter (at the current limits of resolution), and see gauge theories as the appropriate description of the strong, weak, and electromagnetic interactions.

The successes (partial though they be) of quantum chromodynamics and the $SU(2)_L \otimes U(1)_Y$ electroweak theory are well known. They invite a close and critical look at the completeness and consistency of the standard model: how much does it achieve, what is left out, and why does it work? The shortcomings of the current paradigm are many. They have to do with unanswered questions and with the apparent arbitrariness of the theory embodied in a large number of seemingly free parameters.

A particularly serious problem is associated with the spontaneous breakdown of the electroweak gauge symmetry or, in other words, with the Higgs sector of the electroweak theory. The trouble lies not only in the absence of any real prediction for the mass of the Higgs boson, but also in the instability of field theories involving elementary scalars. In a unified theory, the problem of the ambiguity of the Higgs sector is heightened by the need for two distinct steps in the symmetry breakdown, e.g., $SU(5) \rightarrow SU(3)_C \otimes SU(2)_L \otimes U(1)_Y$ at 10^{14} GeV and $SU(2)_L \otimes U(1)_Y \rightarrow U(1)_{EM}$ at 10^3 GeV. A complete theory must ensure and explain a dozen orders of magnitude between the masses of the intermediate bosons $W^{\pm}$ and Z^0 and the masses of the leptoquark bosons that would mediate proton decay.

Both general arguments such as unitarity constraints and specific conjectures for the resolution of the Higgs problem suggest 1 TeV as an energy scale on which new phenomena crucial to our understanding of the fundamental interactions must occur. For a frontier facility of the 1990's, an important desideratum is therefore the capability to explore electroweak phenomena on the 1 TeV scale.

The Accelerator Requirements

A multi-TeV hadron collider should provide the means to test thoroughly the predictions of the standard model, to illuminate the physics of electroweak symmetry breaking, and to explore the unknown. In order to translate these sentiments into requirements for accelerator performance, Estia Eichten, Ian Hinchliffe, Ken Lane, and I (EHLQ) have considered[1] a broad variety of hard-scattering processes which bear on the capabilities of a hadron-hadron collider. These include conventional processes such as the production of large transverse momentum jets in QCD and the electroweak pair production of gauge bosons. Such processes are of interest as tests of the standard model and as backgrounds to more exotic phenomena. Among the latter, we analyzed several alternatives for the Higgs sector of the electroweak theory, including the minimal Weinberg-Salam solution, supersymmetry, and technicolor. We examined modest extensions to the standard model: sequential quarks and leptons, and additional charged and neutral intermediate bosons, and also looked at manifestations of quark and lepton compositeness. In each case, we explored the prospects for production and detection, in light of the anticipated conventional backgrounds. We did not consider in detail how to distinguish one new physics signal from another.

The calculations presented in EHLQ are intended to provide a base of reference information which will provoke informed discussions of the energy and luminosity requirements for a supercollider, and of the relative merits of proton-proton and proton-antiproton collisions. Other elements, including technical feasibility, rate demands on detectors, and cost, must also be weighed in arriving at machine parameters. For each of the principal physics topics, we have given a stylized summary of collider performance as a function of c.m. energy and luminosity. These are based on discovery criteria which we believe reasonable, but which are in the end inevitably somewhat arbitrary.

Because the choice of SSC parameters will affect everyone's future, all of you should feel obligated to give the matter some thought. I urge you to use the cross sections in EHLQ to make an independent assessment of collider capabilities. The parton luminosities presented there provide a measure of collider capabilities that is not tied to specific theoretical inventions.

To open the discussion of these issues, I remind you of the conclusions reached by EHLQ:

- "We are confident that a 40 TeV collider which permits experimentation at integrated luminosities of 10^{39} cm^{-2} will make possible a detailed exploration of the 1 TeV scale.

> - For a 10 TeV device, the same guarantees cannot so comfortably be made. At this lower energy, the upper reaches of the expected mass ranges for new phenomena are inaccessible, even at an integrated luminosity of 10^{40} cm^{-2}."

Energy and luminosity matters have also received some attention at the Lausanne LHC workshop. According to the Summary Report,

> "The highest energy [18 TeV] would be desirable, but there is no known threshold; the key point is to reach at least 1 TeV at the constituent level. A high luminosity, say $\sim 10^{33}$ cm^{-2} sec^{-1}, would be an important asset."

Of course, it does not suffice merely to skim the conclusions of these documents. It is important to examine how the authors arrived at their pronouncements, and what assumptions were made.[3] Nevertheless, I read the LHC conclusions as supporting EHLQ's belief that to complete the electroweak agenda, an energy in excess of 10 TeV is desirable. I hasten to add that, faced with the choice of a 10 TeV collider or nothing, I would opt for the 10 TeV collider.[4] In EHLQ's words,

> "We are not so foolish as to say that a 10 TeV collider is without interest, or to assert that our calculations prove that it is inadequate to the task of sorting out the physics of electroweak symmetry breaking. We cannot state the precise location of the dividing line between our confidence at (40 TeV, 10^{39} cm^{-2}) and our trepidation at (10 TeV, 10^{40} cm^{-2})."

Two points have emerged from the examination of the same set of physics topics at Snowmass. First, there is no disagreement with the conclusion that a 40 TeV collider operating at 10^{32} cm^{-2} sec^{-1} can accomplish the physics goals we now foresee. Second, there is general agreement that detectors will operate satisfactorily at 10^{32} cm^{-2} sec^{-1}. I regard this as important progress toward defining what the SSC should be. The physics and detector discussions of the past year have brought us close to a demonstration that we can achieve what we set out to do with the SSC, in that the experiments to investigate "known" phenomena can be done with techniques now at hand.

I should say that I do not consider this conclusion to be an argument against higher luminosity. Energy-luminosity tradeoffs are complex, and the discovery reach of a machine will be extended by the possibility of higher-luminosity running. Detector capabilities will surely increase markedly over the lifetime of the SSC.

Protons or Antiprotons?

The physics interest of $\bar{p}p$ versus pp collisions has been considered in a number of studies, including the Chicago workshop.[5] The consensus is represented by EHLQ's assessment that

> "for hard-scattering processes, the advantage of $\bar{p}p$ over pp collisions (at the same energy and luminosity) for the production of massive states is limited to a few special situations in which the presence of valence antiquarks is important and the integrated collider luminosity exceeds 5×10^{38} cm^{-2}."

Examples of situations in which $\bar{p}p$ collisions may afford a greater physics reach are the production and degree of polarization of new intermediate bosons, and some compositeness tests.

This suggests that the choice of $p^{\pm}p$ collisions should be based on accelerator concerns, cost, luminosity, energy, and the number of interactions per crossing tolerable in detectors. Hard-scattering processes do not express a strong preference for one beam or the other. On the number of events per crossing, it is worth noting that the detector analysts have endorsed $\langle N\rangle=1$,[6] whereas the SSC Reference Designs Study[7] achieves $\mathcal{L} = 10^{33}$ cm^{-2} sec^{-1} with 10 interactions per crossing.

Progress at Snowmass

Everyone who has participated in these discussions is keenly aware of the immense amount of work that remains before us in order to make experimentation at the SSC a reality. However, a number of important beginnings have been made here. Among them, I would cite the following as especially significant:

- Work toward the detection with high efficiency of intermediate bosons, and toward understanding the 4-jet QCD background.
- Assessing the needs for test beams.
- Work in progress on realistic simulations of supersymmetric particle production and decay.
- Identification of barriers to detector performance.
- A joining of issues on the ep and fixed-target options.

An issue that has not been joined here (because no "true believers" came forward) concerns the importance of polarized beams. The selling point is that polarization offers a more differential probe, and this can be illustrated by specific examples in the study of new W's and Z's, and in the interpretation of hints for quark-lepton compositeness. The examples that come to (my) mind all qualify as "second round" experiments, but this does not automatically mark them as second rate. The opportunities afforded by polarization deserve a thorough look.

In this connection, it is useful to note that plausible polarized structure functions are readily available. Dave Hochberg has observed[8] that the Carlitz-Kaur prescription[9] which reproduces the polarized-e polarized-p scattering data of the SLAC-Yale experiment[10] essentially commutes with the QCD evolution of structure functions. The operational importance of this remark is that you can select your favorite set of large-Q^2 structure functions and compute from them, by a simple procedure, the spin-dependent structure functions.

As a final remark on polarization, I note my confusion about the impact on the accelerator design. I have heard assessments ranging from "a piece of cake" to "well-nigh impossible." This must be an answerable question. It is important that the technical issues be understood quickly.

Trigger Rates

An important task begun[11] but not completed at Snowmass '84 is confronting the challenges of trigger rates at high luminosity. The point to emphasize is that there are substantial rates for hard-scattering processes, and not merely for the fluff generated by peripheral collisions. A few examples will call attention to the "opportunities" we face: at $\mathscr{L}=10^{33}$ cm^{-2} sec^{-1} and $\sqrt{s}=40$ TeV, we anticipate

W pairs at 0.1 Hz

400 GeV/c^2 $H \rightarrow W^+W^-$ at 10^{-2} Hz

400 GeV/c^2 quarks at 1/2 Hz

500 GeV/c^2 technieta ($P_8^{0\prime}$) at 1 Hz

500 GeV/c^2 gluinos at 1 Hz.

In addition, a "high-E_T" trigger with threshold set at 2 TeV will count at 1 Hz from two-jet QCD events. the E_T-trigger rate is shown in Fig. 1 for pp collisions at 10, 40, and 100 TeV.

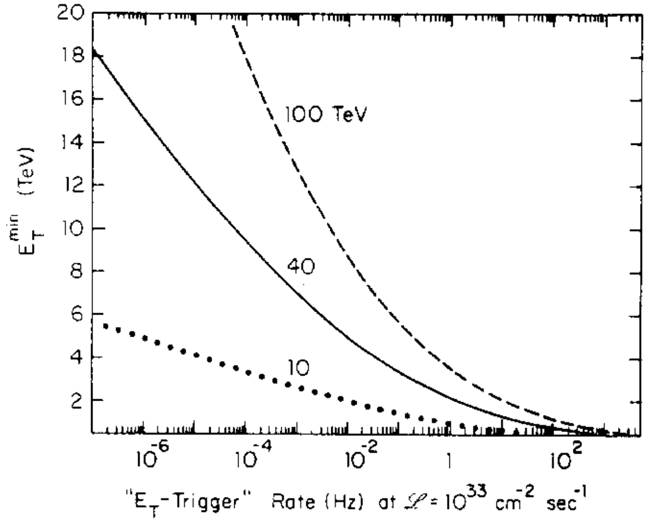

FIG. 1. Counting rate for an E_T-trigger in pp collisions at an instantaneous luminosity of 10^{33} cm^{-2} sec^{-1} (after EHLQ).

It is evident that very selective triggers will be essential for high-luminosity operation. To approach these issues we urgently need more, better, and faster Monte Carlo programs.

Acknowledgements

It is a pleasure to thank my collaborators Estia Eichten, Ian Hinchliffe, and Ken Lane for essential discussions. The devoted efforts of the conference staff have contributed greatly to the success and Gemütlichkeit of the workshop. For the work carried out at Snowmass, we owe special thanks to Robin Craven for the impressive operation of the Computing Facility.

Footnotes and References

1. E. Eichten, I. Hinchliffe, K. Lane, and C. Quigg, "Supercollider Physics," FERMILAB-Pub-84/17-T, to appear in Reviews of Modern Physics.

2. G. Brianti, et al., Summary Report of the Workshop on the Feasibility of Hadron Colliders in the LEP Tunnel.

3. To cite a trivial example: EHLQ adopt a standard experimental year of 10^7 seconds at nominal machine luminosity, whereas the Lausanne Workshop year is more nearly a full calendar year.

4. Whether the world needs two of them is a different question.

5. "$\bar{p}p$ Options for the Supercollider," edited by J.E. Pilcher and A.R. White, Argonne National Laboratory and The University of Chicago, February, 1984.

6. I am told they would really like N=1, without Poissonian tails!

7. Report of the Reference Designs Study Group on the Superconducting Super Collider, May 8, 1984.

8. D. Hochberg, FERMILAB-Pub-84/72-T.

9. R. Carlitz and J. Kaur, Phys. Rev. Lett. 38, 673 (1977); J. Kaur, Nucl. Phys. B128, 219 (1977).

10. M.J. Alguard, et al., Phys. Rev. Lett. 37, 1261 (1976); ibid., 41, 70 (1978); G. Baum, et al., Phys. Rev. Lett. 51, 1135 (1983).

11. See the report of the Calorimeter Trigger Group, L. Price, M. Abolins, and R. Wagner, these proceedings, p. .