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A Full-Acceptance Detector for SSC Physics at Low and Intermediate Mass Scales*

An Expression of Interest to the SSC

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I. Introduction

This document must begin with a few words of explanation. I write it not as an experimental physicist proposing to lead a program of measurements at the SSC. Instead, I write as a theorist interested in seeing the proposed detector and physics measurements done at the SSC. It should be clear that I view this subject as important enough to warrant the effort on my part going into producing this tome. It should also be clear that nothing will happen unless members of the experimental community come forward and do real work to see whether the ideas contained herein are sound and that the physics is indeed worth a dedicated effort at the SSC. Therefore this document is directed more toward the experimental community than the SSC Laboratory. However, since initial encouragement (or discouragement) by the laboratory is evidently very important, this document also contains specific requests addressed to the SSC Laboratory.

The basic reasoning behind this work stems from the fact that hadron collider physics is much more of a program than an experiment, in contrast to electron-positron collider physics, which is more of an experiment than a program. This feature nowadays tends to get lost in the increasingly focussed atmosphere of the race to discover the Higgs particle (or its alternative). In particular, in hadron colliders not all events can be scrutinized and selectivity is very important. And there is very important physics to do at all mass scales, not only the highest. It is a folk theorem that any kind of hard-collision physics which exists at lower energies will exist at higher energies with bigger yields and better signal-to noise. Thus B-physics at the SSC is superb; W and Z yields per collision are much larger than at the Tevatron, etc. In addition the physics at the very lowest mass scales, the log-s physics, has suffered from lack of attention at energies higher than attained at the CERN ISR. And there is ample reason to believe that novel phenomena should appear at the SSC energy scale. The evidence comes from the known limitations of QCD theory to deal with high energies at low intrinsic mass scales and from cosmic-ray data.

Therefore I believe that it is appropriate to seriously consider as part of the SSC physics menu a detector optimized for the lowest mass scales rather than the highest. It should at the very least do a good job in measuring the small-angle leading particles, and for this reason alone tends to be a device quite distinct from the generic detector designs such as the SDC. This follows from the apparently unavoidable necessity of moving back the final-focus quadrupole magnets, leading to a serious diminution in luminosity, of order a factor 100. Such a device may not even be compatible with doing an optimal job on B-physics, and such a constraint is specifically absent in my thinking here. Instead I see the detector as a survey instrument, one that covers all of phase space (this means pseudorapidities up to $\pm$ 12 or so) reasonably uniformly. As a survey instrument, however, it cannot be expected to be optimal for a typical specific physics goal. But it could well be optimal for the serendipitous physics discovery that is not in anyone's design book.

I hasten to add that the priority for the high-mass scale physics remains incontestably highest, and that in my opinion an optimal job on B-physics may well be higher priority as well. However, were one to stop there, so much physics would be missing that I feel very strongly that neglecting the remainder would be a big mistake. After all, in an SSC year 10^9 complete events out of a produced 10^{13} would be recorded by such a detector, and it seems extremely arrogant to assume that there is no fundamental information to be gained by careful study of such a sample. And the information per event from this detector would be far greater in almost all respects than that acquired by the generic detectors.

One should also take note that, just as the high-p_t generic detectors will garner much useful information about low mass scale phenomena, the converse is also true. The capability for discovery physics of this detector is very high up to mass scales of 100 to 200 GeV, and might exist well beyond that, especially were SSC luminosity to increase to 10^{34}. Thus, while the main justification for this device is its capability at low mass scales, the "hidden agenda" of higher mass scale physics is considerable.

The basic criteria for the detector I envisage include the following:

1. There should be full acceptance in phase-space (the lego variables η and ϕ). For example a 10 TeV π^0 with 300 MeV transverse momentum should be detectable, and its four-momentum measured accurately.
2. There should be good, reasonably uniform sensitivity over all this phase space for momentum measurement of charged particles and π^0's, provided their transverse momenta are not too large.
3. The physics of diffractive processes (Pomeron physics), *i.e.* the physics of

event structures containing "rapidity gaps" (regions of η into which no particles are produced), must not be compromised.

These criteria are for me essentially non-negotiable, whatever one may think of the physics value attached to them. They for me define what is meant by the words "survey instrument". Highly desirable but to me negotiable are

1. Full muon coverage and momentum measurement over all phase space (provided p_t is not too large).
2. Good efficiency over all phase space for detecting vees and kinks from K_s, Λ, Σ, Ξ, etc. (at low p_t).
3. High quality microvertex coverage over all the relevant phase space for charm and bottom decay vertices.
4. Nondestructive particle identification (Cerenkov, TRD,...) wherever possible in the phase space.

In the architectures that I have looked at, this latter criterion may be compromised, but I am not sure of this. However, in any case hadron calorimetry certainly is compromised, and is left (for me) as low on the priority list. Nevertheless hadron jet studies may well be accessible up to the 100 GeV or so scale by one-by-one reconstruction of the individual charged and neutral tracks comprising the jet.

The architecture of the detector will be discussed in Section V. It is essentially two full-acceptance 20 TeV fixed-target spectrometers face-to-face. Since spectrometer length tends to scale linearly with incident beam energy, this means that each will be of order 20 times the length of a Tevatron fixed target spectrometer, or about one kilometer. The transverse dimensions remain invariant, although for practical reasons I bias toward compactness, with diameter less than a meter.

The spectrometer further divides itself naturally into five segments. The "center", covering rapidities up to $\pm$ 3 or so, is a compact barrel detector with endwalls, perhaps on the scale of CLEO or AMY. The "right-wing" and "left-wing" segments are fixed-target style devices of length 50-100 meters covering η from roughly 3 to 7. Designs of these appropriate for SSC conditions exist in the context of B-physics initiatives, such as BCD or SFT as submitted to the SSC. There also is a nice design to be found in the 1987 Berkeley summer study proceedings.

Beyond these sectors are the "radical-right" and "radical-left" wings of the device, extending from about 100m to about 1 km. In the front portions of these sectors must be the final-focus magnetic elements, but with enlarged apertures (10-30 cm diameter?) in order that they may also serve as analyzing magnets for the fast forward secondary particles of energies of one TeV and upward. This greatly increases the length of the final focus system (100m of magnets), not to mention cost, and probably leads to decrease of luminosity (relative to a standard "intermediate-luminosity" collision region) as well. However, other than this complication, the amount of detector instrumentation needed for these sectors seems relatively modest.

It is essential that this detector not share a collision region with a higher-priority experiment, if only because of the compromised luminosity. To me there are other important reasons as well, namely that if lower priority physics is attached parasitically to a higher priority project, it can simply end up never being done. Witness the fate of minimum bias physics at CDF: who in that collaboration is going to opt for that? If the lower priority physics is to be done at all, it deserves a dedicated effort.

A corollary is that if this physics is to be done at all it should be done well. SSC collisions are too precious to deserve anything less than that. The above description should make it clear that the complete spectrometer that I am talking about is big, not small; I guess a cost of \$350M and a team of a few hundred physicists for the full detector. On the other hand the device is very modular and stageable, and therefore can start small and not be fully instrumented, if necessary, until years after SSC commissioning. The natural starting point for the first stage is construction of the radical-right and radical-left sectors, the cost of which is probably dominated by the final-focus system (\$20-30M??). Even with the most rudimentary supplementary instrumentation, a lot of first-generation physics would be accessible. Therefore the big cost of the ultimate device need not enter into the cost competition for the other higher-priority first-generation proposals. The overall scale of the first-generation experiment need not be much different from a sizeable Fermilab fixed target experiment. More on this question can be found in Section VI.

It seems to me that having the beginnings of the full detector in place at SSC commissioning has enormous advantages in getting an early look at not only the generic physics, but also the nontrivial problems of backgrounds, radiation damage, etc., so that the ultimate device could benefit from early working experience. Therefore deferral in initiating this enterprise until the second generation of SSC experiments, *e.g.* waiting for bypasses and new collision halls to be developed, seems to me very unwise. Thus it is none too soon to actively consider the pros and cons of this approach.

What are the problems with this? The biggest seems to be simply the physics: is it really interesting enough to justify such a detector? To me the answer is self-evident. But I have had this idea for some time[1] and from experience I have come to realize it is not at all evident to experimentalists, who nowadays tend to prefer mining gold to going fishing. I therefore have gone to some length in documenting in Sections II-IV some of the possible physics topics of special interest. However, they do not address well enough the strength of this detector in intangibles. The intangibles include a look at a vast unexplored area of strong interaction phenomena which are out of theoretical control. They include the fact that the modularity of the detector should allow relatively quick adaptability to changes in physics interest. And the full acceptance of the detector combined with its considerable sensitivity may make the serendipitous discovery potential especially high. But there are no guarantees.

The next biggest problem is whether it can be built. Are there insoluble technical problems in the way? The worries include

1. Backgrounds from holes in calorimeter walls, beam-gas interactions, beam halo, etc.
2. The beam-pipe design is difficult; a 1 mrad particle going through a 1 mm pipe sees a meter of material.
3. Even with luminosity reduced a hundredfold relative to generic detectors, the 1 km endwall electromagnetic calorimeter has a radiation-damage problem.
4. Can the detector optics be made compatible with the machine lattice? I think so, but I have no detailed demonstration of that.

More discussion of these worries can be found in Section V.

The bottom line is that the next step, if there is to be one, is a round of serious study and simulations by interested experimentalists. Only this can serve as a basis for creating a more serious preliminary design, and for initiation of work leading to a real proposal. But interested persons MUST come forward. If they do, I will be happy to stay on board until the enterprise is underway. And while I do not want to convert to full-time experimentalist, I obviously would welcome a continuing involvement at the "godfather" level, were this to be the wish of the collaboration. But if nothing happens within this year, I am not prepared to do anything more on this by myself.

II. Physics: Final States Containing Rapidity Gaps

1. Preliminaries: where things are in phase space

Because many of the virtues of a full-acceptance detector have to do with the structure of individual events in phase space, it is appropriate here to first review what phase-space at the SSC looks like. The most useful variables to describe the coordinates of a particle or jet are transverse momentum (in magnitude), azimuthal angle phi and (pseudo-)rapidity η, defined as

$$\eta = -\log \tan \theta/2$$

These are just the (hopefully) familiar lego variables. The minimum production angles of the fastest secondaries are of order tens of microradians, implying a (fuzzy) boundary of the lego plot of

$$|\eta| < 10 - 12$$

or 22 units of η available. Figure 2.1 exhibits where various physics landmarks fall, as well as how the coverage of the major sectors of the detector are roughly partitioned. It is clear that the radical-right and -left sectors do not deal with very high-p_t secondaries, and they will not deal with high multiplicities either. The other three, however, are needed for full coverage of intermediate mass scale physics, especially when one considers that relative to the mean-value boundaries depicted in the figure, two or so more units of rapidity beyond the boundary are required to efficiently capture the decay products of the systems of interest.

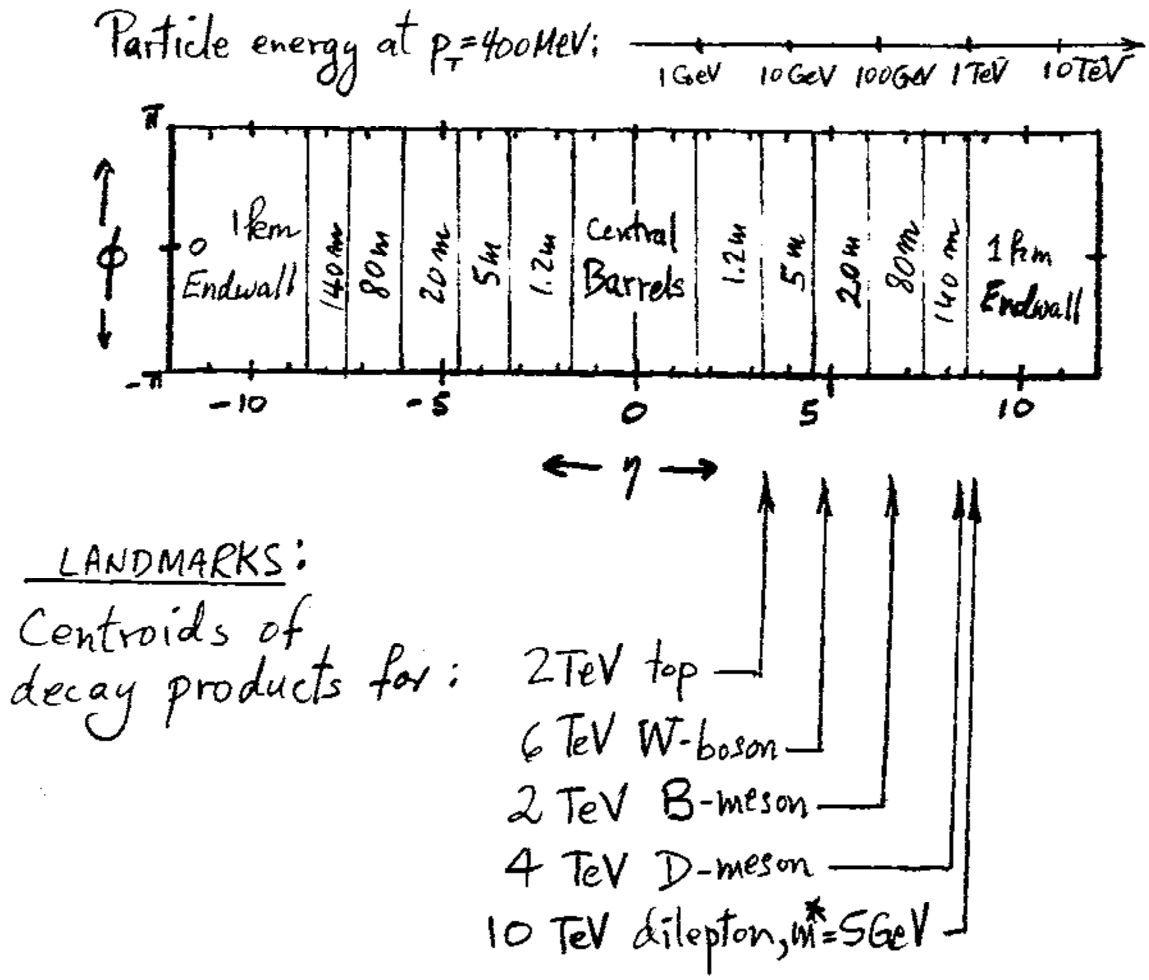


Figure 2.1. Where things are in phase space. The subdivisions indicate the acceptance regions of annular electromagnetic calorimeter walls, as described in Section V.

2. "Soft" Pomeron exchange, single and multiple

We take as a definition that elastic scattering proceeds by "exchange of a Pomeron". The final state is two particles separated by a rapidity gap of 24 units or so. We use the same words for single diffractive dissociation. The mass distribution of the excited system is

$$m \frac{d\sigma}{dm} = \text{constant}$$

so that the event topology is as shown in Fig. 2.2 with uniform probability for finding the boundary-rapidity anywhere. Double diffraction likewise is expected to

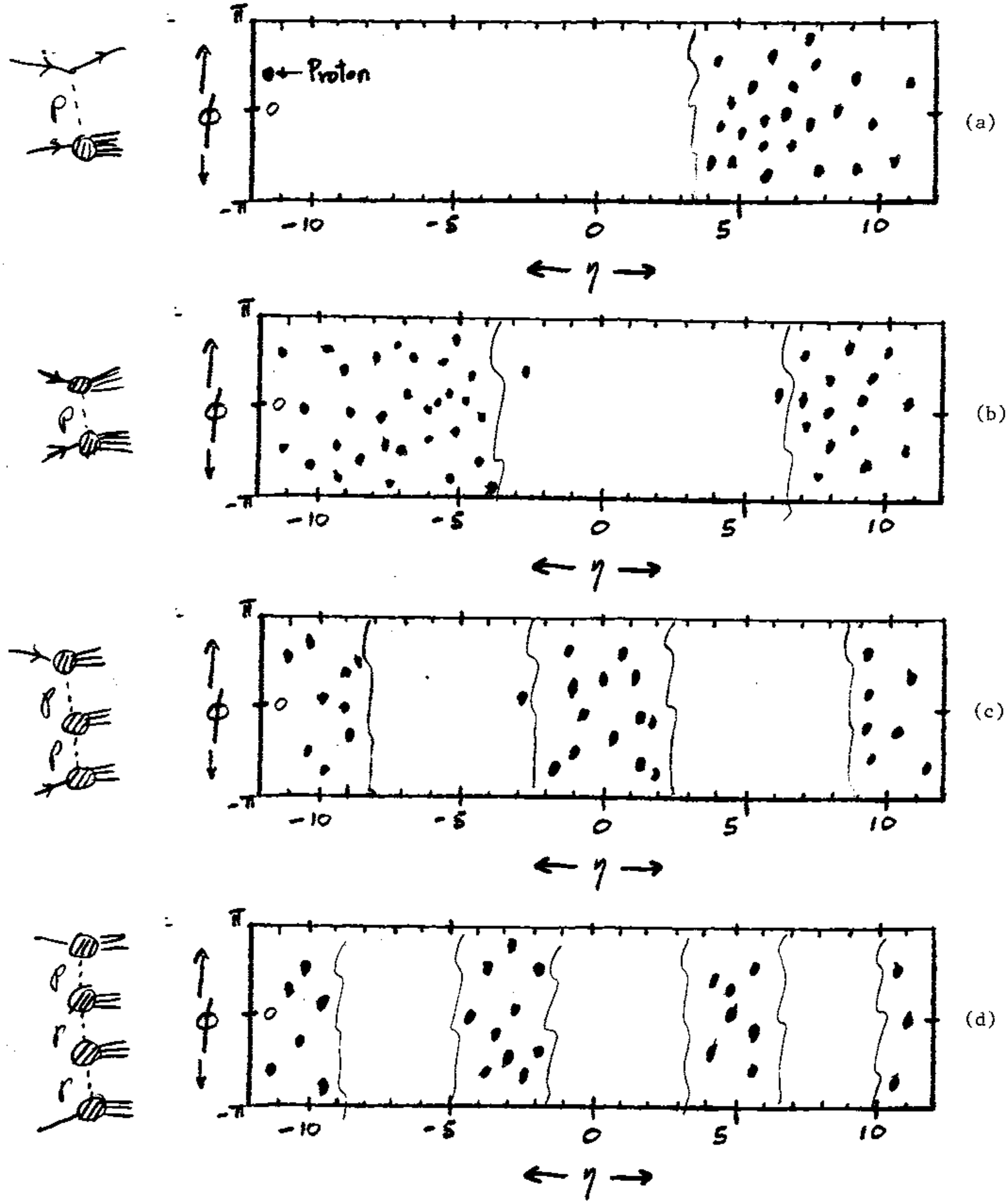


Figure 2.2. Event topologies for various diffraction dissociation processes: (a) single diffraction, (b) double diffraction, (c) Pomeron-Pomeron absorption (triple diffraction), (d) diffraction dissociation of Pomerons via Pomeron exchange (quadruple diffraction).

again occur with a uniform distribution of where the boundary rapidities occur. The minimum width for a significant rapidity gap is of order 2 to 3 units, in order to reduce Poisson fluctuations in multiplicity to a reasonably small level.

There is enough phase space at the SSC for 3 rapidity gaps in a single event, taking into account that a dividing region between gaps must also be at least 2 to 3 units wide. Not only can one study Pomeron-Pomeron collisions, whatever that means, but also diffraction excitation of Pomerons via Pomeron exchange. All these cross sections should be quite large.

The most highly developed descriptive formalism for this class of processes is the Reggeon calculus and Reggeon field theory developed mainly by Gribov and the Leningrad group.[2] The subject is largely moribund at present for two reasons: it needs to be data-driven to make further progress, and it also needs very large log s to make the asymptotics work. There are many theoretical issues and many programs of measurement that can be enumerated, but rather than do that here I prefer to only comment on the central physics issues as I see them.

The main question for me in this subject is what the basic physics of highly inelastic soft diffraction (large diffracted mass, low p_t secondaries) is. There are two different ways of viewing the process, s-channel and t-channel. In the s-channel viewpoint, an optical picture predominates: the sundry parts of the projectile wave function suffer differing amounts of absorption in passing through the other projectile; consequently its wave function has overlap into excited states. This is the original Good-Walker picture, elaborated now in Glauber theory and eikonal descriptions.[3] In the t-channel picture, one views the Pomeron as quite similar to a vector particle being exchanged, a view justified if the Pomeron can be represented by a pole in the complex angular momentum plane. This viewpoint, nowadays advocated by Donnachie and Landshoff[4] and expressed in Reggeon calculus, is also quite defensible and is not necessarily in conflict with the s-channel view, although it is far from obvious to me that there is compatibility.

In any case diffraction dissociation of a target proton, as viewed in its fixed-target reference frame, clearly exhibits that the Pomeron delivers energy and momentum to the struck proton in large amounts, and one must be able to decide what the quanta which carry the four-momentum are. Hard collisions, to be discussed in the next subsection, is one way, as pointed out by Ingelman and Schlein.[5]

Another is the aforementioned diffractive excitation of the Pomeron itself.

Here I offer an opinion on the structure of this "soft" Pomeron. It is simply the disturbance of the chiral vacuum condensate of quarks and antiquarks by the constituent quark moving through it. This is an ancient viewpoint, although the words have been chosen to be as modern and trendy as possible. What does it mean? I take the constituent quark to be a smallish object of radius 0.2-0.3f which obtains its mass of 350 MeV through spontaneous symmetry breaking of the strong interaction chiral symmetry. This suggests it should couple strongly to the collective excitations of the chiral condensate, better known as pions, up to mass scales of a GeV or so. This picture, due to Georgi and Manohar,[6] and recently advocated by Weinberg,[7] suggests that at this 1 GeV scale the relevant degrees of freedom are indeed constituent-quark and pion. The implication here is that the soft Pomeron really should be similar to the pre-QCD picture based upon the multiperipheral model (the multiperipheral ladders of Amati, Fubini and Stanghellini,[8] with rungs and sides built of pions.).

Modern work emphasizes a Pomeron "ladder" built from perturbative gluons.[9] It is my opinion that this Pomeron-the "hard" QCD Pomeron-plays its most central role in the phenomenon of multiple-jet production (minijets) at extreme energies. And until it is clear how extensions of perturbative QCD account for the chiral symmetry breaking, I find it prudent to keep the soft and hard Pomerons as distinct entities. I will return to phenomenology involving hard Pomerons only in Section III.

So far there has been little allusion to what the program of concrete measurements should accomplish. They should include incisive tests of factorization of vertices in the Regge-exchange diagrams, and measurements of the basic parameters of Pomeron-proton and Pomeron-Pomeron total and diffractive cross sections. Enhanced glueball and heavy flavor yields in Pomeron-Pomeron collisions have been suggested by some people, and this deserves a search (although according to the point of view expressed above there is no particular reason for success). And there is a more subtle level of measurements suggested by Reggeon calculus as well, but I am no expert on them and will attempt no elaboration here.

3. Soft Pomerons and hard processes

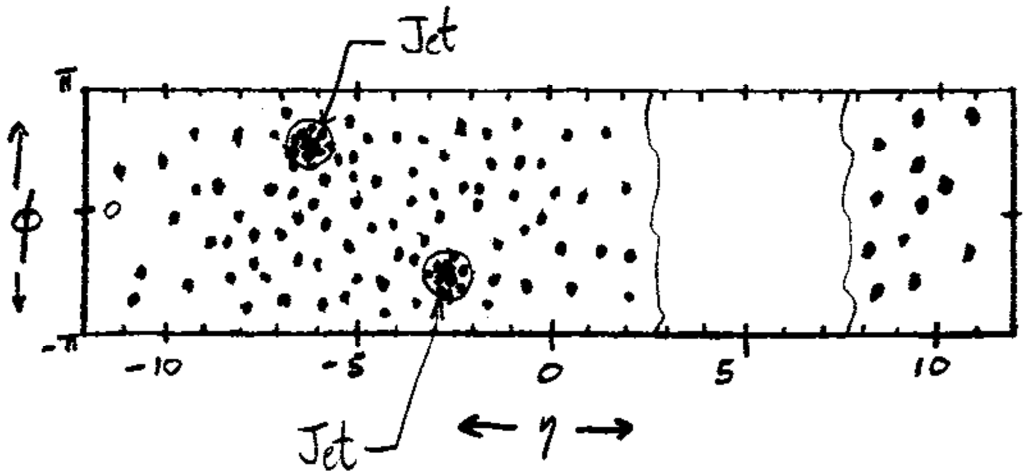


Figure 2.3. Event structure for a hard collision of a pomeron with a proton: the produced jets are coplanar.

Because the exchanged Pomeron delivers large amounts of energy and momentum, it should be possible to determine what the carriers (partons) are. One way was suggested by Ingelman and Schlein,[5] namely deep inelastic processes initiated by Pomerons. In that case the event morphology is shown in Fig. 2.3. One may view this as the collision of a parton in the Pomeron with another in the proton, producing a pair of coplanar jets in the final state. Indeed the process itself allows the operational definition of a parton distribution for the Pomeron, although there is no guarantee that this distribution is independent of the parameters of the proton which emitted the Pomeron.

Some ISR data exists,[10] establishing the existence of the process and the probable softness of the leading parton distribution. In principle excellent measurements can be made by CDF, but so far this program has suffered from low priority relative to W and top physics. A big boost should come from HERA,[11] which will measure the classic electromagnetic structure function of the Pomeron (virtual γ + Pomeron goes to hadrons). I think this entire program is of central importance in elucidating the nature of the soft Pomeron and deserves a considerable experimental effort.

I have a simple guess as to how these measurements are going to come out. The rule is simply that provided the edge of the rapidity gap is far from the produced jets, the ratio of the dijet cross section with the rapidity gap present to the dijet cross section without the gap present is the same as the corresponding ratio when the jets are not present, *i.e.* for low-p_t final states. The reasoning is based upon viewing the process in a frame where low momentum secondaries would be in the center of the rapidity gap. Because of the gap, there are only a collection of fast right-movers comprising the diffracted system without jets, and the fast left-movers from which the coplanar dijet will emerge. But it takes a time proportional to that dijet momentum (at fixed p_t) for that system to evolve. However the "decision" that no low momentum particles be emitted must occur much earlier, and it is hard to see how the eventual emergence of a dijet from some point on the outbound left-moving pancake can influence that "decision". Now the reader may well be perplexed how this argument is related to the previously described parton distribution of the Pomeron. At this writing I do not understand the relation myself. But maybe this example can help provide a feeling for why this subject has an alluring subtlety for the theorist and, I would hope, for the experimentalist as well.

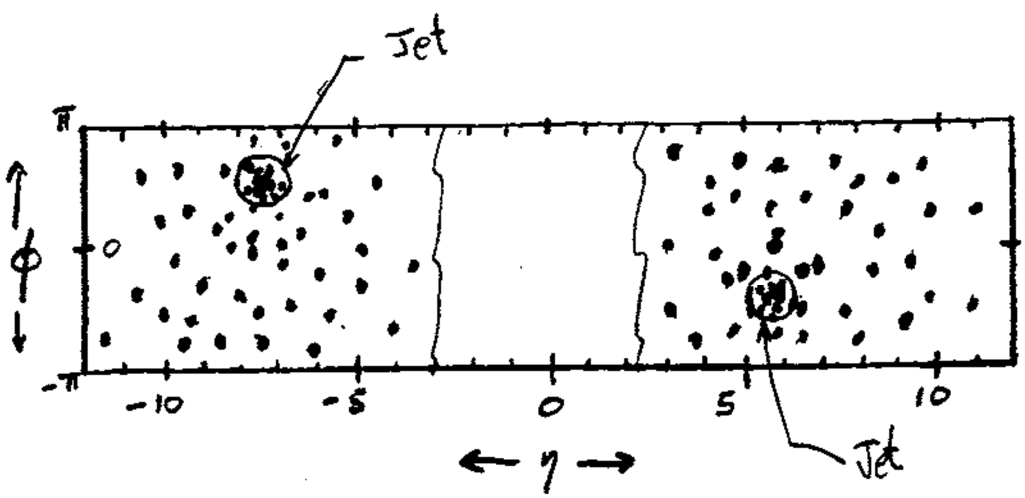


Figure 2.4. Coplanar jets which span a rapidity gap: how often does this happen??

Another interesting process is closely related. Consider a two-jet final state where the jets are coplanar but have a very large separation in rapidity, so large that a rapidity gap fits in between (Fig. 2.4). What is the probability of a gap being present? I am aware of no theoretical discussion of this possibility, and have no good guess of its frequency myself, except to expect that it will be much smaller than for the Ingelman-Schlein configuration described above. The reasoning is that single gluon exchange implies octet color separation between the outgoing systems. The accepted lore has the final-state evolution to be the superposition of two color-triplet configurations separating from each other, in other words a final state very similar (but with roughly twice the mean multiplicity) to an e^+e^- annihilation final state. In the latter case there are no rapidity gaps (except for "higher-twist" gaps which are exponentially suppressed as gap width increases). If this argument is correct, then the only way of getting the gap is to exchange an extra gluon, which must carry with it a considerable price in probability. Probably the best way to get a good answer is to do the experiment. In principle it should be easy for CDF to do. And as we shall see, this process is a potential background to more interesting measurements.

Closely related is the question of double-parton collisions, *i.e.* two binary hard collisions occurring in the same event. There exist some experimental studies of this, with results rather inconclusive as yet. When better data arrives it will again be interesting to see whether there is any fraction which contains a rapidity gap between the pairs.

4. Single and double photon exchange

The reader may well be not impressed with the above physics menu all by itself: it is good solid physics to be sure, but is it really worth the investment of an SSC collision region? In the next sections we explore physics which is more fashionable but has in it event structures with rapidity gaps. The cross-section and background estimates will have their uncertainties. But the biggest uncertainty will be directly traceable to the lack of understanding of the Pomeron physics described above. Understanding it is something of a prerequisite to the physics that follows.

The main thrust of the following sections is to use event structure, in particular rapidity gaps in concert with jet structure, to greatly suppress backgrounds for hard processes involving collisions of photons and/or W's and Z's with each other. The idea is I believe due to Khoze,[12] although to my knowledge there has not yet been much detailed development of this very promising approach.

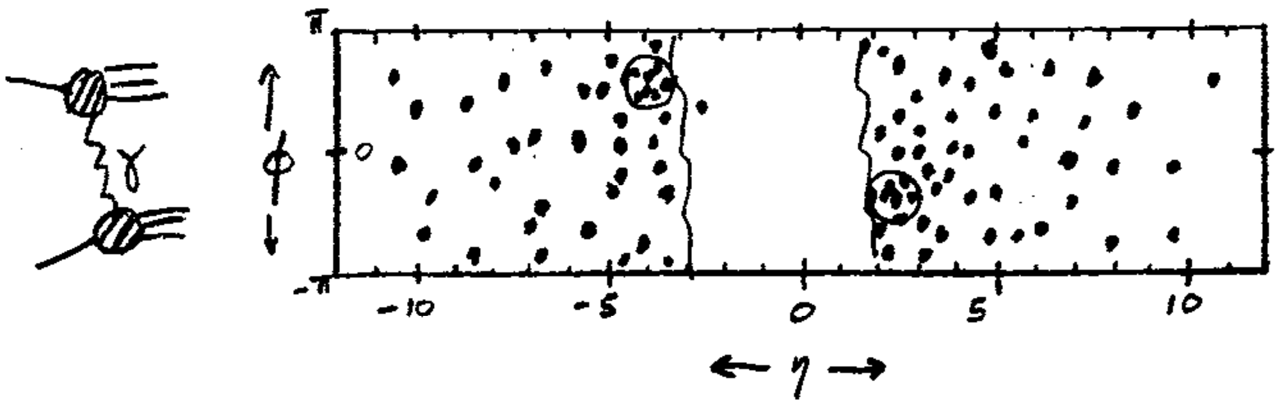


Figure 2.5. Feynman diagram and event structure for a photon-exchange process. The rapidity gap is assumed here to survive absorption effects caused by interactions of spectator partons.

We begin with single photon exchange, which naively is described by the Feynman diagram shown in Fig. 2.5. The event structure from this naive contribution is also shown in the figure. It has the general features of double diffraction, including (for dx/x parton distributions) the feature of uniform distribution of the position of the gap boundaries in the lego plot. However, at the edge of each gap there will be a "tagging jet", with $p_t = q$, where q is the transverse momentum of the exchanged photon. This is a simple but quite important feature of what can be called HERA kinematics; in collider mode deep-inelastic final states are distorted relative to what one is used to in fixed target mode.

Another important feature of the final-state morphology follows from only the assumption of a uniform rapidity distribution of produced hadrons in the usual cms frame of proton and virtual photon. A straightforward but tricky Lorentz boost leads one to the conclusion that if the footprint of the tagging jet is taken to be a circle with the standard radius $R = 0.7$ in the lego variables, then the boundary of the rapidity gap should be taken as tangent to this circle. The mean number of hadrons per event leaking into the gap can be shown to be

$$\langle n \rangle = \frac{1}{2}\,\frac{dN}{d\eta}\,e^{-2R}$$

which for $dN/d\eta = 4$ is about 0.5. We call this configuration a tagging jet at the edge of the gap.

While the cross section for these events is considerable, there is potentially a much larger background of events from Pomeron exchange. Dijets from gluon exchange at the same p_t but with no rapidity gap are 10^5 times more frequent. The dangerous configuration, however, is the one discussed above which was so uncertain; furthermore one has to determine whether the jets easily approach the edge of the gap and can be candidates. Advocates of a t-channel Pomeron picture, such as Donnachie and Landshoff, may find a big background, but as yet this is nearly uncharted theoretical territory.[13]

An additional problem is whether to believe the cross section estimate based on the naive Feynman diagram. If any of the spectators in the hadrons choose to undergo soft collisions, the rapidity gap will be filled in. We return to this question in the final subsection of this section. But the estimate there gives a mean survival probability of about 20%. This is in my view a not unreasonable guess, but one which is near the upper limit.

Even with the survival-probability put in, there would be 10 to 100 events per SSC year (10^{38} integrated luminosity) with q^2 in excess of 10^5 GeV^2. These, however, would suffer a large background from W and Z exchange.

In any case, it is not very clear how much physics interest there is in this process, especially given that its normalization is made uncertain by the large absorption correction. More interesting is the two-photon process. We consider various final states in turn:

$a)$ $\gamma + \gamma \to \mu^+ + \mu^-$.

The event structure (Fig. 2.6) is a rapidity gap with two tagging jets at the edges and a dimuon pair (isolated) within the gap. The signature looks very good, and its virtue is to provide a precise measure of the absorption correction, since everything else is calculable.

$b)$ $\gamma + \gamma \to$ quark pairs .

This is the structure function of the virtual photon: The final quanta are predominantly $u\bar{u}$ and $c\bar{c}$. The attainable pair masses go up to about 50 GeV; hence there could be some very significant QCD tests performed on this sample. This is a

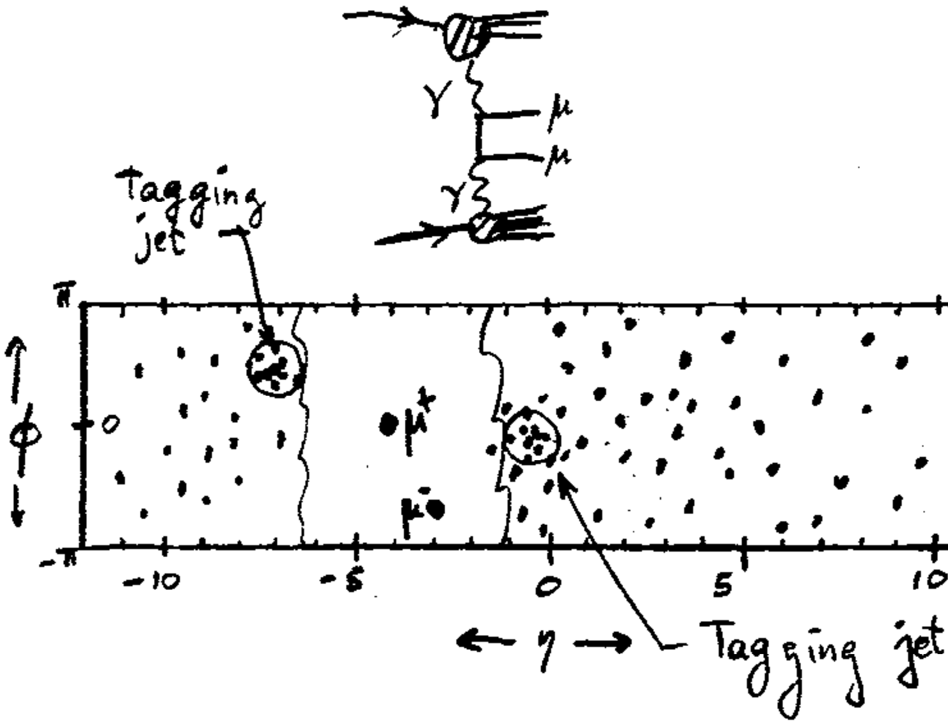


Figure 2.6. Feynman diagram and event structure for the two-photon process $\gamma + \gamma \to \mu^+ + \mu^-$.

classic QCD process for which the theory is reputed to be especially clean.[14] The event topology is two rapidity gaps with tagging jets on all four edges. There could be a big Pomeron background, but if so that would also be interesting physics.

c) $\gamma + \gamma \to W^+ + W^-$.

No question: this process *is* interesting. Because the cross section is dominated by t and u channel W exchange, a rapidity gap can develop between the produced W's, leading to impressive event topologies as shown in Fig. 2.7. I have estimated the number of events with these topologies per SSC year (including the 20% survival probability of the rapidity gaps) to be of order 30-300, although careful work is needed to get an accurate number. In any case, the number is probably too small to really probe the sensitivity level of TeV-mass-scale WW physics, unless the luminosity eventually well exceeds the assumed value here of 10^{31}.

From a more general viewpoint, the γ-γ cross section should be of order 100

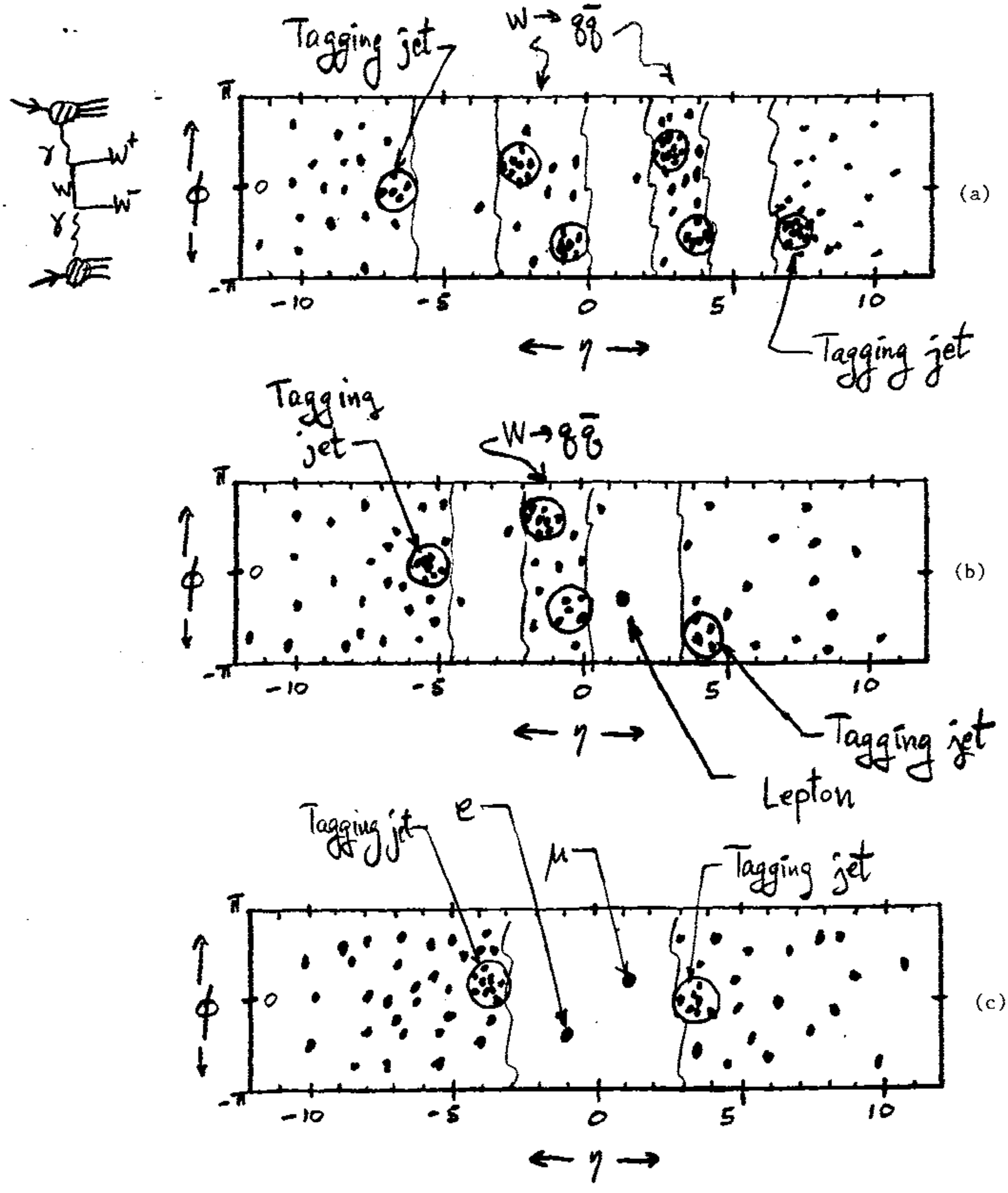


Figure 2.7. Event structure for the process $\gamma\gamma \rightarrow W^+W^-$: (a) both W's decay hadronically (45%), (b) one W decays semileptonically (45%), (c) both W's decay leptonically (10%).

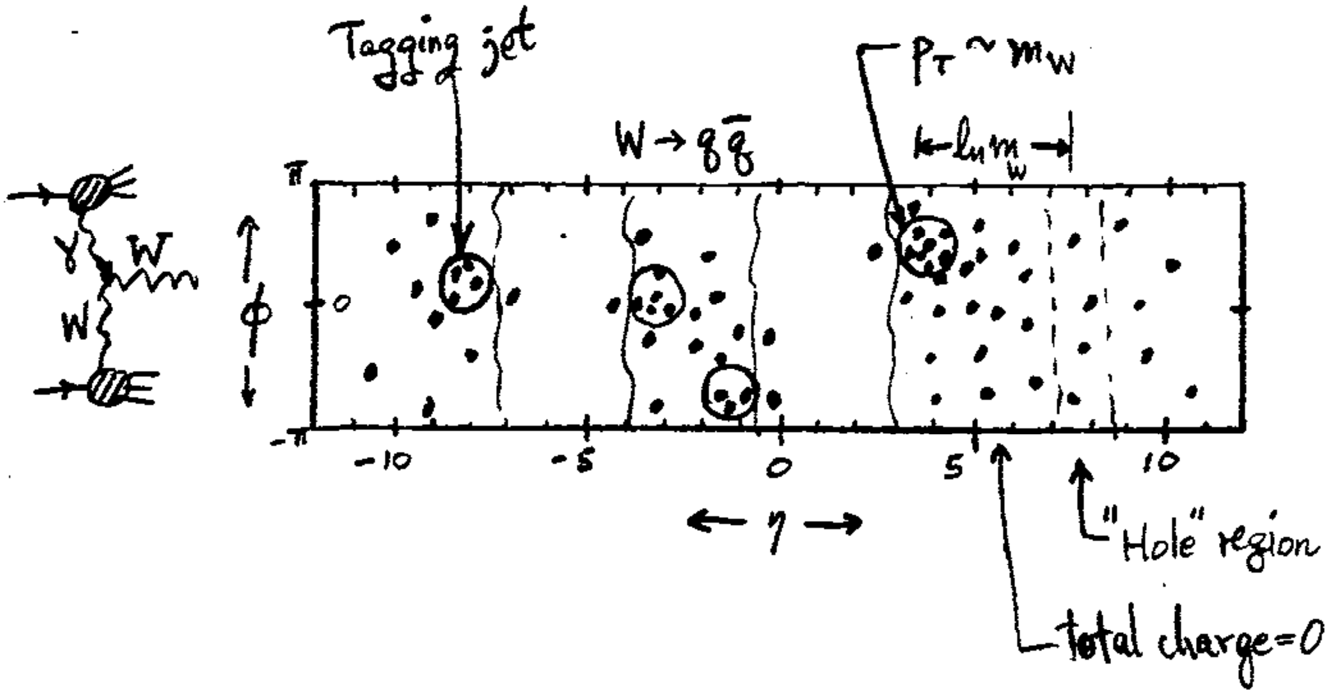


Figure 2.8. Event structure for the process $\gamma + W \to W$ with $W \to q\bar{q}$ assumed. Note the even charge of the beam jet and the odd charge of the W decay products provide signatures.

picobarns or larger in order to garner 100 events per SSC year (at 10^{31} luminosity).

5. Single and double W and Z or γ exchange

Evidently there are interesting rates for processes involving W and/or Z exchanges as well. Single W-exchange events occur at a rate of 1000 per SSC year, but it is not clear to me what one does with them.

Again the two boson processes look more interesting, and we again enumerate a few of the possibilities:

a) $\gamma + W \to W$.

This is quark-W Coulomb scattering. A rough estimate gives about 100 per SSC year. The physics is unclear, although one gains a very clean sample of isolated W's, which might yield a good mass measurement. The signature to me looks pretty good (Fig. 2.8).

In fact this is a good place to mention another tagging strategy which in principle might eliminate Pomeron backgrounds. The beam jet connected to the

W has even charge (0 or 2) while the background jets have odd, unit charge. Therefore a count of the number of charged tracks in the beam jet can tag the W-exchange events. The typical charged multiplicities are not so large (of order 10) and all the tracks are of small angle and high energy, and should be intercepted by the planes of silicon strip detectors inserted within the beam pipe. It seems to me not out of the question that efficiencies per track in excess of 97-99%, which is what is needed, might be attainable, especially since there are a plethora of diffractive events on which to practice.

b) $W + W \to Z$
$W + W \to q + \overline{q}$
$W + W \to W + W$.

Were there not the LEPII program, the first reaction might be interesting. The second reaction is interesting only inasmuch as it is suppressed because of the virtuality of the exchanged W's. It is an unwelcome background for the Higgs production process to be discussed next. W-W scattering is certainly interesting, but I estimate the number of events per SSC year is order 10, and these are essentially just WW Coulomb scatters. I suspect that these hold not much interest unless the luminosity is considerably higher. It will be important to carefully determine what conditions are needed. This is especially true when the tagging-jets have $p_T < m_W$, because the very interesting longitudinal W's predominate over the transverse W's (I am indebted to Stan Brodsky for this remark[15]).

6. Intermediate-mass Higgs search

The process

$$W + W \to \text{Higgs}$$

is of special interest in the intermediate mass range of 100-160 GeV. The event topology is shown in Fig. 2.9 and consists of two rapidity gaps, 4 jets on the edges of the gaps, the two central jets being $b + \overline{b}$, building the Higgs mass. The usual backgrounds from gluon fusion are eliminated by the tagging jets and the rapidity gap. If necessary the beam-jet charges might provide another signature. And the b's in the jets are hopefully efficiently tagged with the microvertex system. The irreducible background comes from $b + \overline{b}$ production by the W's, although this

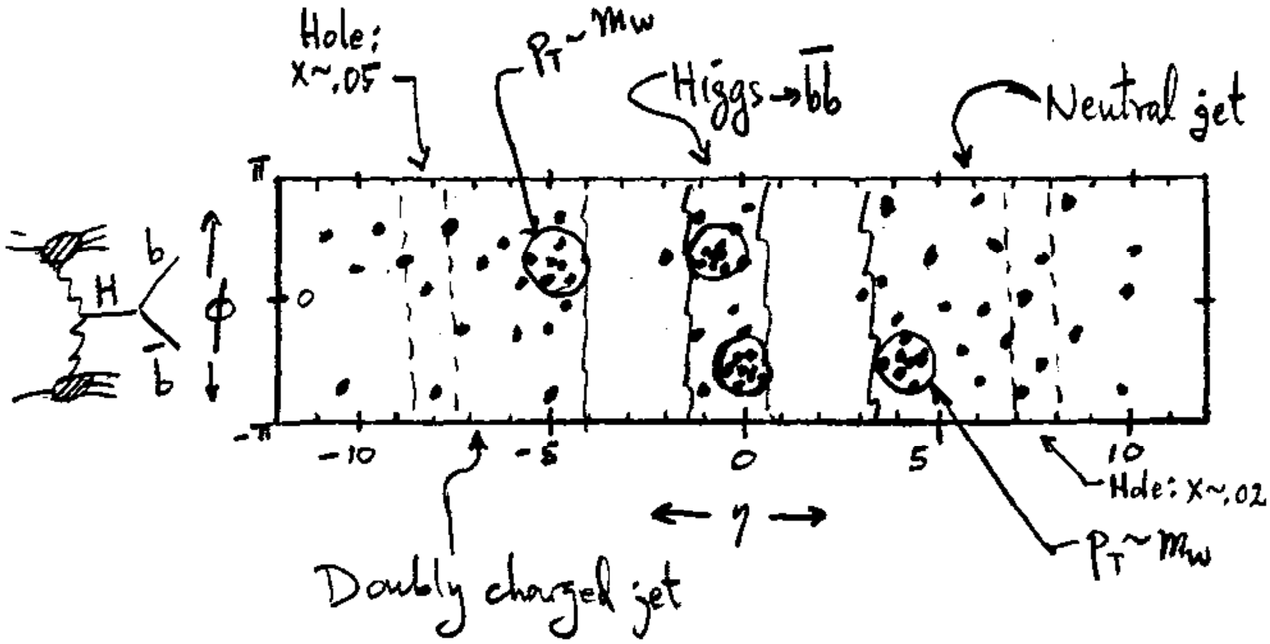


Figure 2.9. Event structure for the process $W^+W^- \to$ Higgs $\to b\bar{b}$.

background is suppressed, as already mentioned above, by the large virtuality of the exchanged W's.

While this strategy cannot be found anywhere in the Higgs-hunter's guide,[16] useful cross section estimates can. These must be corrected for the requirement that enough phase-space for the two gaps plus Higgs products exist, and for the factor 5 for absorption. A rough estimate, based upon a spacing of the two quarks which initiate the hard process ("holes") of at least 15 units of rapidity (the cost is a factor 12), still gives an estimate of the number of events per SSC year of order 30-100. The yield versus mass is shown in Fig. 2.10.

It cannot be claimed that this detector is ideal for a search using this strategy. But it doesn't do so badly, and this example may help indicate the outer limits of discovery potential possessed by it.

7. Survival of the rapidity gap; absorption corrections

This subsection is essentially a short appendix. It is included because so many of the above estimates are dependent on the notion that the absorption corrections are not too large, *i.e.* the spectator interactions do not fill in the rapidity-gap

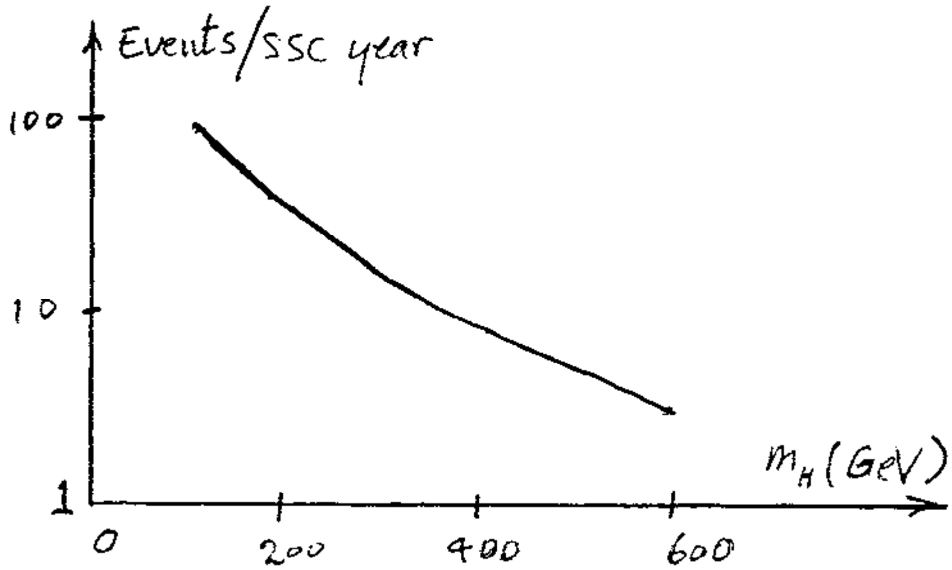


Figure 2.10. Yield of Higgs events per SSC year ($\int \mathcal{L}\, dt = 10^{38} cm^{-2}$) with the rapidity-gap topology of Fig. 2.9 versus mass.

present at the naive level of calculation. We estimated a survival probability of about 20%, and here is presented the basis of that estimate.

At the naive level of calculation, the hard cross section is a convolution of parton densities in the transverse, impact plane:

$$\sigma_{\text{naive}} = c \int d^2b\, \rho(b)\, \rho(B-b)\ .$$

We assume that the hard cross section including the absorption correction is simply obtained by writing

$$\sigma = c \int d^2b\, \rho(b)\, \rho(B-b)\ |S(b)|^2$$

where $|S(B)^2|$ is the transmission probability of the two protons at impact parameter B. The above formula is justified if and only if the partons are an uncorrelated "gas"; we come back to this later.

Now we assume the transmission probability is of eikonal form, as is traditionally done

$$|S(B)|^2 = \exp -\nu \int d^2b\, \rho(b)\, \rho(B-b)$$

with the convolution of densities the same as before, and normalized to unity at $B = 0$. The quantity ν, the "central absorption", is found from fits to elastic and total cross section data, and is known to rise slowly with energy, with an extrapolation to SSC energies giving roughly 5 (I have not done a careful study of the calculations, but have benefitted from communications with R. Cahn, B. Margolis, and F. Halzen).

Then it is a straightforward calculation to show that, with Gaussian densities, the ratio of corrected to uncorrected cross sections is just $1/\nu$ for large ν, and the rough value of 20% for the survival probability is thereby assured.

However the assumption of uncorrelated partons is suspect. For example if the proton were built of three small black-disc constituent quarks, one can readily see that the calculation changes a lot. What is needed is a lot of gray area in the impact plane where absorption is present but not complete. This is most likely true, especially for my preferred view of the Pomeron as related to the disturbance of the chiral condensate around the constituent quark, because the size of that disturbance is measured by the Compton wavelength of the pion. So I consider the factor 5 a very reasonable estimate, but still far from being a solid prediction.

III. Physics: Nondiffractive "Soft" Processes

1. Conventional studies

The physics of nondiffractive soft processes is evidently already a well-developed subject at all available energies: it's what happens! This is classic minimum-bias physics, and it is not my intention here to enumerate a long list of topics, which form the table of contents in conference proceedings. Nevertheless, there has been only the most spotty experimental survey of the subject at contemporary collider energies, in part because of priorities but also because of limited acceptance and lack of particle identification. Large Feynman x used to be the favorite arena for measurements; now there is nothing. And this is the fragmentation region of the objects that really constitute the proton. Nook-and-cranny enterprises such as UA5 (streamer chamber, no magnet) at CERN, and E735 (small acceptance charged particle spectrometer at low luminosity, together with minimal tracking information) at FNAL have provided some information. UA1 and CDF have provided some as well. But there is nothing like the comprehensive surveys once done at fixed target energies. At the high energies of the SSC, the underlying physics is quite different, as manifested already at the SPS in the predominance of long-range rapidity correlations, something not at all present at lower energies. The total cross section will have risen by more than a factor two, and minijets will have become commonplace in the final states. So a full survey of inclusive properties, multiplicity distributions, their related correlation functions, etc. will deserve a full-scale professional study. Certainly if study of the total cross-section behavior is of interest (as it seems to be), then so should be the study of the phenomena which build it.

I will say little more about the generic minimum bias program, which should be reasonably familiar. What will follow will try to emphasize the features more unique to the property of full acceptance possessed by this detector.

2. Fixed-target mode and the question of s-dependence

For many topics in minimum-bias physics, the question of s-dependence is of considerable importance for theoretical interpretations. The fact that this spectrometer has fixed-target architecture allows the acquisition of data in fixed-target mode, just by putting a gas-jet target in the region of the colliding beams (but not when they are in collision, of course). This provides data at $\sqrt{s} = 200$ GeV, a rather good interpolation from fixed target energies to the SSC (although another point at 2000 GeV would be most welcome). This feature is especially valuable in the forward direction, where particles of the same Feynman x go into the same spectrometer elements in both collider and fixed target mode. And indeed there are *two* fixed target experiments: the left arm can check the results of the right arm.

3. Quark tagging

One of the disadvantages of proton-proton collisions is that the projectiles are too complicated. Not only are they made of extended objects called constituent quarks (not to mention strings and/or bags), but there are three of them in each beam. Indeed, many find this not only disadvantageous but downright abhorrent. Even supposing that the proton can be depicted in terms of an additive quark model, there are several configurations in the impact plane that the proton pancake can have on arrival at the collision point. It can be 3 transversely separated and distinct quarks, it can be a quark and a diquark (one quark shadowing another) or a triquark. If we only worry here about central constituent-quark collisions, there are a dozen possibilities: (1) all quarks miss, but there is an interaction anyway, (2) a single qq pair interacts, (3) a pair of qq interactions occur at different impact parameters, ... (12) two triquarks interact. And it is possible that these different initial-state classes lead to distinct final-state event classes.

With this plethora of possibilities to average over, it may for many purposes be difficult to make clean interpretations of what is going on, especially if there is some aspect of nonperturbative QCD involved. Therefore event tags which eliminate or enhance some of those dozen options might be a valuable general tool. One candidate is what may be called quark tagging; it is simply the observation of a leading (but nondiffractive) baryon, say with x between 0.5 and 0.9. For example if the tag is a fast neutron, then it would seem very probable that the quantum that interacted nontrivially was an up quark, with its companion quarks flying forward to make the neutron. With such tags, one might hope to get a reasonably pure sample of (constituent-level) up-up collisions. Similarly a Δ^{++} tag might produce an enhanced d-quark "beam." A fast forward meson and no baryon might indicate that two of the quarks interacted, and nothing forward, all three. Real life is probably not quite that simplistic, but the strategy seems to me of possible high value. A starting point might be deep inelastic scattering, including a neutron tag, to see whether the parton distribution so obtained would be what one might expect from a single constituent up quark. (There is a theoretical formalism for this—the triple Regge formalism—and the measurement would be of the structure function of the Reggeon, probably ρ. While the language is very different, I suspect the physics is not.) I do not know whether HERA could do such a deep inelastic program, and I doubt that there is any possibility elsewhere—except within this experiment itself, using the large samples of gamma exchange or W, Z exchange events discussed in Section II.

4. High multiplicity

Already there is a significant data base at collider energies and a keen interest in the origin of the very high multiplicity events seen at high energy. It is the same issue which motivates the heavy-ion program: is quark-gluon plasma produced, and if so, can one find observables which indicate that it is? Of course, this is a more speculative topic in hadron-hadron collisions, but interesting nonetheless. I prefer to think of the question in slightly more general terms than that. It is an experimental fact that very large depositions of transverse energy per unit rapidity do occur when the entropy (particle-number) is also very large, *i.e.* the energy is not in high-p_t jets. If this energy emerges radially from the collision region in a shell no more than a fermi thick, it follows from simple geometry that the produced hadrons cannot be formed until the radius of this shell is more than 5 f., because otherwise the hadrons would be overlapping so much that there would be no way to consider them as real particles. Therefore there must occur in these processes quasi-macroscopic transport phenomena. There are two extreme views. The first is statistical; the entropy was produced very early in the evolution, at a time less than 1f, with the produced plasma then flowing outwards according to the laws of hydrodynamics. If this occurs, then the initial temperature of the plasma attainable in hadron-hadron collisions can far exceed what is expected for ion-ion collisions. On the other hand, a perturbative-QCD point of view would have relatively little entropy produced at early times, in the form of some number of virtual gluons. Then as the gluon branching processes evolve, the entropy grows exponentially, so only at the final stage of the expansion is there the large value observed in the final-state hadrons. Distinguishing these two extremes is to me the primary question. An important general attack is the study of fluctuations, which are likely to be of different character: are they Gaussian or fractal in nature? There are many other diagnostic tools proposed, which need not be reviewed here.

The large acceptance of the spectrometer is clearly a useful advantage, provided the high multiplicity does not swamp the device. (This looks not too bad; even for $dN/d\eta$ 20 times the mean, the average number of photons hitting a calorimeter wall is only 200. Assuming 1 cm^2 for the area of a shower, this is only 5% of the calorimeter area. The situation for charged tracks to this naive theorist also looks no worse.) Also, existence of full acceptance would provide a measure of the fraction of the energy escaping the high-multiplicity "fireball". For example it would be very interesting to see what the maximum attainable multiplicity is in a single quark-quark collision, using the fast-forward-neutron tag described in the previous section.

Even more interesting to me would be to search for high-multiplicity events in which there are no leading particles surviving at all. Can there exist rapidity gaps at the *ends* of the lego-plot; *e.g.* no secondary particles emergent with energy greater than, say 200 GeV? The most central collision imaginable is a central triquark-triquark collision (recall a triquark is three quarks all with the same impact parameter which shadow each other). The frequency of these is of order ten per hour. It may be that the core of the constituent quark is quite black, because of the small-x, gluon overlap problem seen (cf. Section IV) in perturbative QCD. So maybe it is thinkable that the quarks can stop each other, leading to a "Landau initial state". By that I mean the initial condition assumed by Landau when formulating his hydrodynamical model of high energy hadron collisions in the 1950s,[17] namely that all the incident energy is thermalized and deposited into a volume whose longitudinal dimension is $1/\gamma$ of the transverse dimension. Under these circumstances the initial energy density is enormous:

$$\mathcal{E} = \frac{40\ TeV}{\pi(0.25f)^3} \cdot \frac{1}{\gamma} \approx 1.5 \times 10^{10}\ GeV/f^3\ .$$

This leads in turn to an impressive initial temperature as well:

$$T \sim \left(\frac{1.5 \times 10^{10}}{2}\right)^{1/4} \times 200\ MeV = 70\ GeV\ .$$

However the initial entropy of 40 TeV/70 GeV $\approx$ 600 (which in Landau hydrodynamics is also the final multiplicity) is not as impressive. It is proportional to the initial volume, which is small, both because of the Lorentz contraction (which leads to an $s^{1/4}$ energy dependence of the multiplicity) and because of the small size of the constituent quark. So large central multiplicity by itself may not be an optimal signature. The reader is invited to speculate on what is optimal.

I have no idea how probable this scenario is, only that the odds must increase fairly strongly with energy because the quark opacity is very likely increasing. One must remember that the current excitement about sphaleron-induced baryon-number violating processes would have looked much less likely than this 5 years ago, and that we are 10 years away from these measurements.

5. Unusual event structures

There is so much phase space per event that the search for unusual patterns of deposition of p_t and/or entropy (particle density in the lego plot) may yield surprises. Centauros are a historic example, one which is not completely dead yet. The small samples of cosmic-ray events always inspire such searches, most of which (but not all) go nowhere. (We return to the cosmic-ray situation in the next subsection.) No doubt any initial sample of data would do the same and stimulate higher statistics searches for marginal patterns seen initially. While the eye can be very good (sometimes too good) in finding unexpected patterns, there are specific suggestions made as well. These include bands of high density at a fixed η or rings of high density in the lego plot caused by Cerenkov-like radiation of gluons by partons moving through the collision debris.[18] There have been hints in cosmic ray data of multijet final states where all the jets lie in a single plane containing the beam axis, and there is a tendency toward coplanarity in QCD calculations as well.[19] So perhaps there is an event class with horizontal high-density bands (fixed ϕ) in the lego plot. There may occur in high multiplicity events nonrandom "large-scale structures" similar to the voids and "great wall" seen in large scale galaxy distributions. Such structures might be a natural consequence of a first-order phase transition into quark gluon plasma—or the fractal nature of QCD branching processes. And the leading regions are not immune from surprises, since the small-x phenomena may lead to a violent dissociation of a constituent quark when it is hit by an incident dense wall of gluons.

All of the above can be easily dismissed as mad ravings, without getting an argument from me. But to do so is not science. What seems to me clear is that too much of the QCD theory is out of control to be complacent on these questions. There is no substitute for going out and having a direct look.

Maybe it is worth mentioning that it is of clear interest to repeat all such studies on events containing rapidity gaps and quark tags.

6. The cosmic ray connection

There is already a small amount of data available at the SSC energy scale, and much of the most relevant information comes from the Pamir-Chacaltaya emulsion chambers. There is a very recent summary of the situation,[20] and as usual there are claims of unusual phenomena. It is important to recall that the detector is essentially a fine grained lead-emulsion electromagnetic calorimeter, followed by 60 cm of carbon absorber, followed by another lead-emulsion calorimeter. There is a TeV scale detection threshold for the showers, so there is a strong bias toward observation of the leading particle distribution only. The energy scale for the primaries is 10^3 TeV to 10^5 TeV. What is reported includes

a) Difficulty in accounting for the yield and x-distribution of gammas (π^0's) using smooth extrapolations of collider data to higher energies. Quite strong violation of Feynman scaling is indicated, but no one as yet has a good model.

b) An excess of hadron (non-π^0) energy fraction relative to what is obtained in simulations; *i.e.* there is a tendency toward Centauro behavior in a statistical sense, although no more smoking-gun Centauro candidates have been exhibited.

c) At the highest energies, a class of penetrating shower-clusters (hadron-like, not pure electromagnetic) are seen, with estimated relative p_t's in the tens of MeV—too large to be electromagnetic and too small to be conventional hadronic. These clusters dominate the total energy of the event (they "lead"). It is claimed that there are too many of them to be explained as events originating at low altitude above their detector.

While item a) is credible because of the rapidly changing yield of QCD gluon bremsstrahlung and minijets with energy, items b) and c) are more difficult, especially item c), which to me would require some part of the produced final-state system to somehow evolve into an exotic soliton-like state of large size, which decays in some way which generates only low p_t secondaries.

An attractive candidate is pion condensate formed in the wake of collision debris, some of which is driven out along the beam directions. If this were to happen, there might also be Bose-Einstein enhancement, and large Centauro-like fluctuations in neutral/charged pion ratios.[21]

Directly addressing these questions at the SSC absolutely requires something like the radical-right and radical-left spectrometer sectors because all the observed phenomena are at Feynman x larger than 0.04 or so. The Chacaltaya-Pamir report[20] contain several quotes that make perfect propaganda for this device. A typical example is therefore repeated here:

"The results of our present study indicate that the characteristics of very high energy nuclear interactions, near around or exceeding 10^{16} eV, must be novel and cannot be accounted for with a simple extrapolation of our knowledge obtained through the accelerator experiments in lower energy region. It is especially remarkable that most of those novel nature of hadronic interaction are seen in the forwardmost small angular region where the cosmic-ray observation covers in its full potentiality."

7. The Low-p_t Frontier

The remarks in the previous subsection regarding cosmic-ray evidence and its possible interpretation lead toward additional implications for physics utilizing this detector. The theoretical implications are an unexpected byproduct of the work on this document, and at present are being investigated with energy and enthusiasm by Marvin Weinstein and myself.

Suppose it is really the case that there exists an event class consisting of a group of leading particles with small transverse momenta (less than 100 MeV) and which carry the majority of the beam momentum. Presuming that they are pions, this means that, if this group of particles has a mass of order the nucleon mass, they will be more or less at rest in the rest frame of the cosmic ray projectile. A simple picture emerges in that frame: the proton at rest has everything carried away downstream by the incident projectile except for its pion cloud (perturbed chiral condensate), which simply decays into soft, semirelativistic pions. Since these particles are bosonic, and the process is quasiclassical, it may be reasonable to anticipate an atypically large amount of fluctuation in their charged/neutral ratio.

If this happens in the forward/backward regions of phase space, it is hard to avoid the conclusion that it will happen in the more central regions as well. If so, there should be at all rapidity intervals a component of the particle spectrum with anomalously low p_t. And it should be of not inconsequential size (Fig. 3.1). One might view this component as a perturbed, "disoriented" chiral condensate, contained within the shell of normal produced particles moving outward from the collision at the speed of light. Because it is more slow-moving, it eventually decouples into a "pion cloud" which decays into an especially soft component.

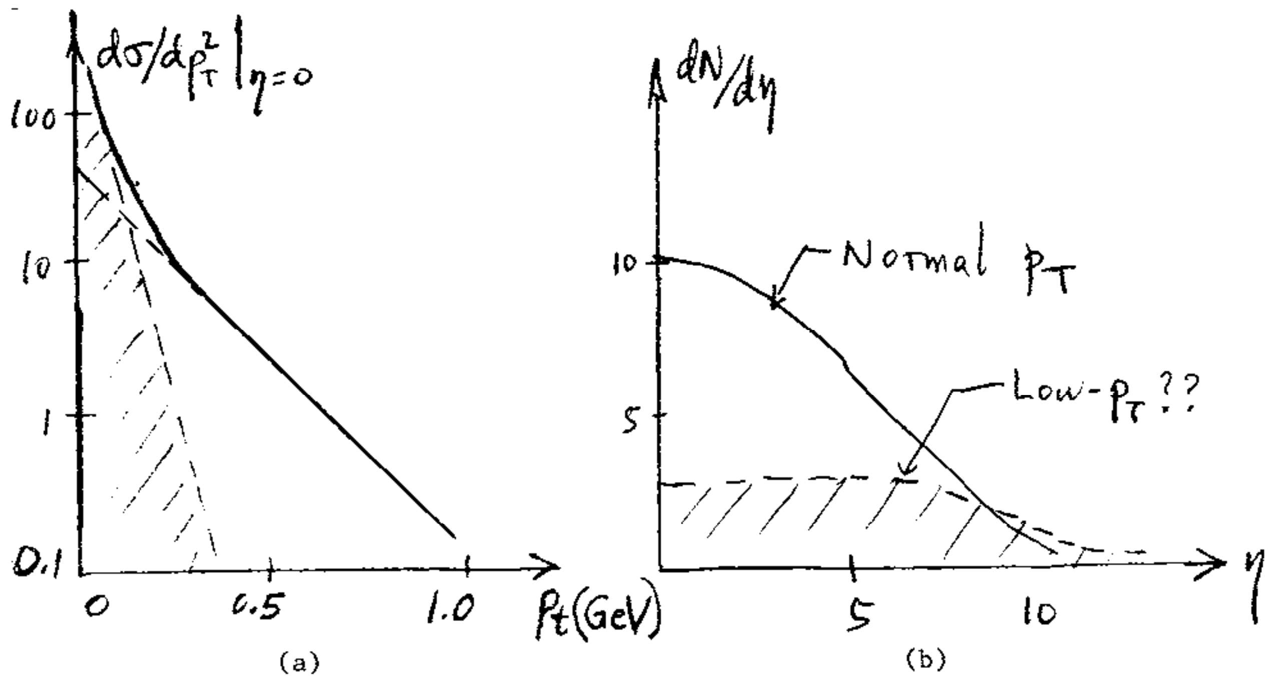


Figure 3.1. Possible distributions in (a) p_t and (b) η of a hypothetical, anomalous low-p_T component in the pion inclusive distributions.

There is some experimental evidence that this may actually happen. There is an excess of low p_t hadrons observed both in nucleon and ion collisions, with the strongest effect occurring in the highest multiplicity events, a feature very consistent with the above picture. Also observed are unexplained excesses of low p_t direct photons and electron-positron pairs. These are too numerous to be accounted for in terms of inner bremsstrahlung. But charge fluctuations of a coherent, charged pion gas might be just what is needed. There is theoretical work on the whole subject by Van Hove,[22] who pulled together some of the evidence and proposed an interpretation based on ultracold quark-gluon plasma.

All kinds of measurements suggest themselves, especially the charge distribution of this soft component in the lego plot on an event by event basis. Some of this might be doable already with existing data sets. But this detector again seems especially well suited (cf. Fig. 5.2) to this task in two respects. Low p_t particles are efficiently identified. And even with quadrupole optics the momentum resolution attainable just with the silicon tracking within the beam pipe should be competitive with what is attained for particles of p_t of 1-2 GeV using the full detector. Likewise the low p_t photons, difficult to see at 90 degrees in most detectors, turn into reasonably energetic ones in the forward direction beyond, say, rapidities of four or so. We might mention that the interesting products of the pion clouds of the projectiles have laboratory angles of no more than 50 microradians.

It should be clear that if bulk chiral condensate is in some sense observable in hadron-hadron collisions, it is a matter of quite fundamental importance. Just as the relationship of the quark-gluon plasma phase to the normal phase tells us about the nature of confinement, this phenomenon might tell us something about the nature of the chiral phase. Phenomena involving the chiral phase may in turn be very similar to what happens in the Higgs sector. Additional insights into the poorly-understood hadronic analogue might have quite far-reaching implications.

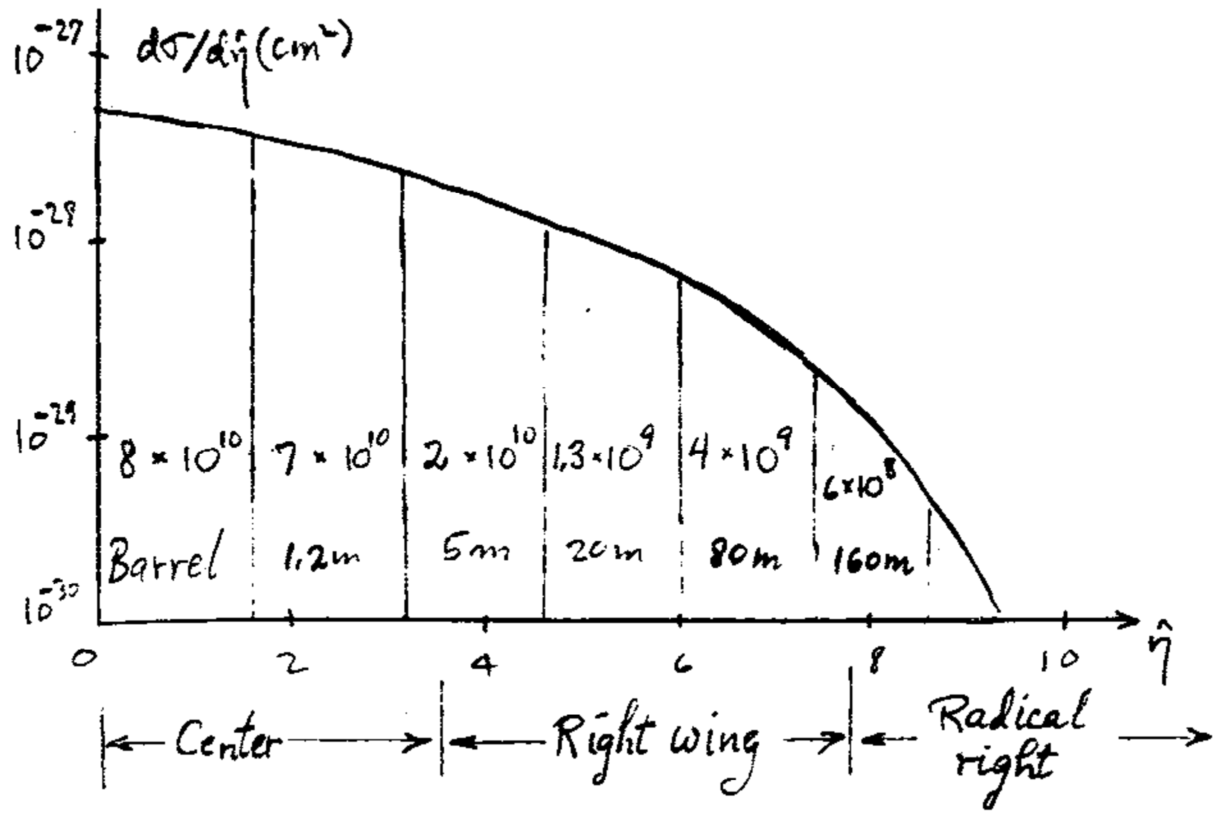


Figure 3.2. Yield of charmed hadrons as function of rapidity, along with the number acquired per SSC year by the various modules of the detector.

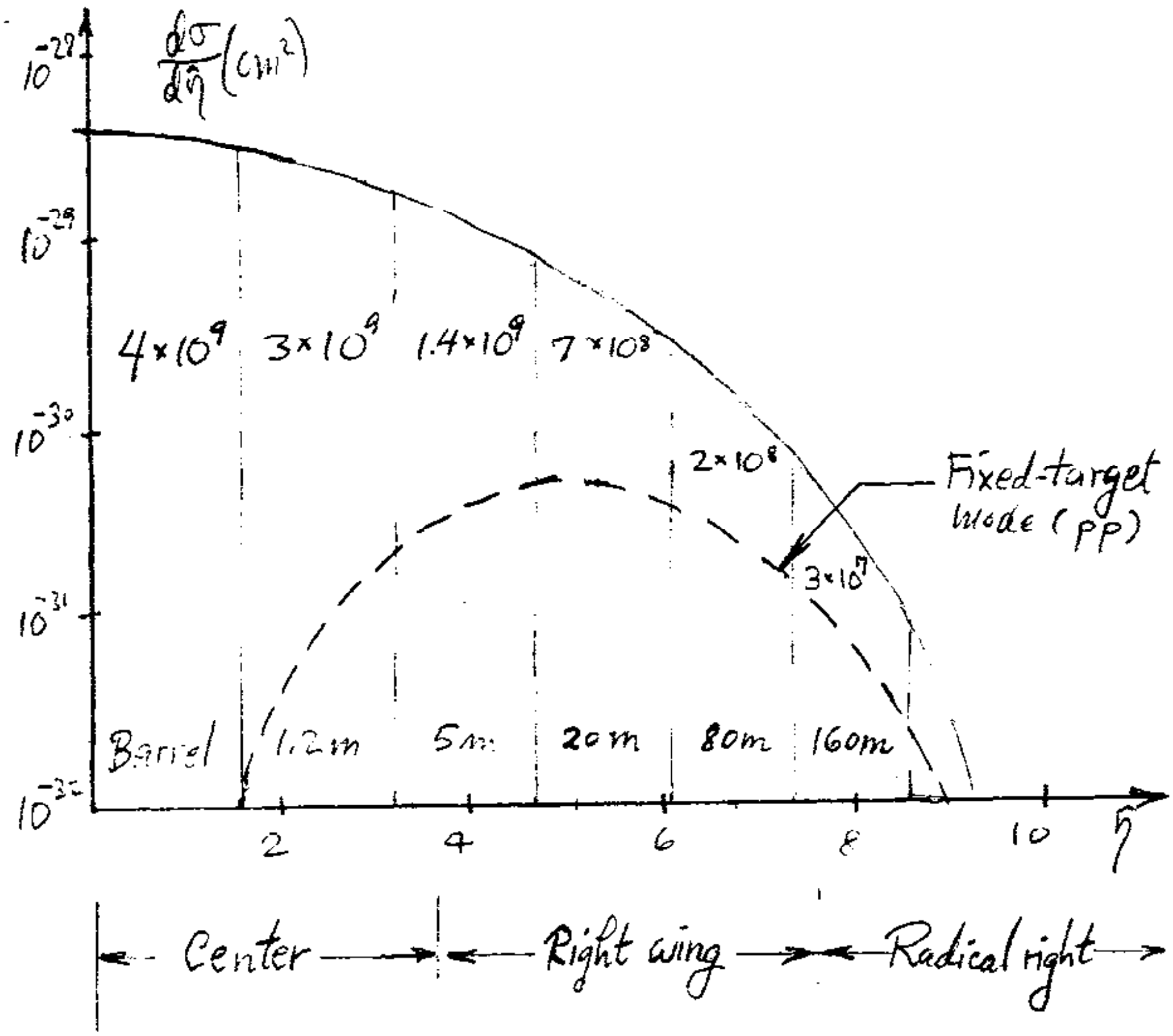


Figure 3.3. Yield of bottom hadrons as function of rapidity along with the annual yield into the modules of the spectometer. Also shown is the yield in fixed-target mode. A hydrogen gas-jet target is assumed.

8. Charm

The yield of charmed hadrons into this spectrometer is enormous, and is shown in Fig. 3.2. One sees that much of the yield is quite far forward, allowing a lot of choice in where to concentrate one's efforts. By the year 2000, the state of the art should be, according to Fermilab planning, over one million reconstructed charm events per experiment. In addition, large super-clean samples of comparable size should be available from the electron-positron tau-charm factory in Spain. Looking at the yields in Fig. 3.2, assuming efficient reconstruction (3-10%) and efficient event selection (10%?), one could perhaps improve things further by 2 to 3 orders of magnitude, impressive indeed.

The main question is what the physics might be. The important topics, if any, will probably be generated by what happens in the coming decade. Perhaps the search for rare or forbidden decays becomes interesting, or even CP-violation searches, although it is not the particle of choice for most rare-decay or CP-violation searchers. Perhaps generic spectroscopy questions will be hot topics, such as charmless semileptonic decays used to help normalize the corresponding B-decays—an important subject in the CP-violation world. There should be enough sensitivity to explore the rare species of charmed hadrons such as the Ω_c *(ssc!)* or the *ccu* or maybe even *ccc* baryons. Production dynamics, especially at very small/large x, will draw interest, as well as charm in jets and charm in events with rapidity gaps. And of course charm as a tag for bottom physics, W-decays, or new-particle decays, would be very welcome.

9. Bottom

There has been so much discussion of bottom physics at the SSC that little need be said here. The yield versus rapidity is shown in Fig. 3.3 in order to remind the reader where in phase space the B's go. One should remember that this represents the distribution of the centroid of the decay products of the B in question, so that two or so units of rapidity should be added on to one's chosen cut to get good acceptance. Even so it is clear that the radical-right/left sectors do not get much yield, so that their role might be limited to production-dynamics studies, such as studies of B-production in diffractive events.

Relative to generic B-detectors, there is in principle only the luminosity compromise which distinguishes this device from the others (other than motivation, and the design compromises associated with those mixed motives). It could run in fixed-target mode as well. But I prefer to leave to others the pros and cons of that option-other than exhibiting the fixed target yields in Fig. 3.3.

IV. Physics: Hard Processes

1. Very small x

There is much interest in the behavior of parton distributions, in particular the gluon distribution, at moderate Q^2 (say between 10 and 100 GeV^2) and at extremely small x. This happens because the calculated number of gluons becomes so large that they don't fit in the impact plane without overlapping. If we consider the gluons which have evolved from one valence quark according to the Altarelli-Parisi formalism, then they should fit within that constituent quark, because there is not much diffusion in impact parameter as one goes down the evolutionary branching process. Thus at a q^2 of 30 GeV^{-2}, the "size" of a single gluon is about

$$\pi q^{-2} = 0.004 f^2$$

With the radius of a constituent quark taken to be 0.25f, one then gets a rough limit of about 50 gluons in the quark before they start to overlap. When significant overlap occurs, nonperturbative effects enter and the questions become especially interesting. In particular, one might even expect strong absorptive effects in the center of the distributions under these circumstances. (Can the source become "black"??) Theoretical estimates of gluon distributions vary somewhat, but the saturation effects typically are expected to begin when x is smaller than about 10^{-4} or 10^{-5}. It therefore becomes very topical at HERA, and there has been an entire workshop devoted to this problem.[23]

According to the Drell-Yan rule for hard collisions

$$x_1 x_2 = m^2/s$$

the way to small x_2 is to make x_1 as large as possible and m^2 as small as possible, as well as making s as large as possible. Thus not-so-hard hard processes in the far forward direction are optimal. With $x_1 = 0.3$, and $m^2 = 20\ GeV^2$, one gets down to x_2 of less than 10^{-7}, three orders of magnitude below HERA.

The processes to study are familiar ones from Fermilab/CERN fixed target programs: direct photons (perhaps the best), Drell-Yan dileptons and onium production, and perhaps hadron jets. The rapidity range is in the radical right/left sectors of the spectrometer, extending into the more central ones as well. There is plenty of cross section, and signal to noise should be no worse and probably better than at lower energies.

2. Minijets

Minijets are identifiable jets with p_t "as low as possible", which in practice is no less than 5 to 10 GeV. The minijet phenomenon is endemic at the SSC scale, with almost every inelastic event containing a minijet somewhere in the lego plot. The evolution of the minijets with increasing energy and fixed p_t probably has a very similar pattern to the Altarelli-Parisi gluons; in fact the problems are closely related—perhaps identical. So their study will bear closely on the corresponding one at the parton level. But the former has the advantage of allowing the examination of multijet final states and their patterns and correlation structures in the lego plot. The minijets are so prevalent that they are important in understanding the behavior of the total cross section and also the behavior of high multiplicity final states, as discussed in the previous section. The questions of event morphology are discussed in more detail in the next subsection.

3. Multijet event topologies and the "hard" Pomeron

It was mentioned in Section II.2 that I keep distinct the concepts of soft and hard Pomeron. The former was argued to be connected to the disturbance of the chiral condensate of light fermions by the passage of a fast constituent quark through it. The latter, to be discussed here, is to be associated with a disturbance of the color field by the passage of colored partons through it. To see the distinction, I like to imagine that in the SSC there were colliding upsilon beams or colliding B beams. In upsilon-upsilon collisions, there is negligible coupling of the upsilon to the chiral condensate, since its internal structure is (presumably) almost completely color Coulomb field. An upsilon-upsilon collision is well described at the parton level by one gluon exchange. Within that approximation one has after the collision two receding color-octet excited upsilons, and the hadronization is similar to the e^+e^- annihilation process. To be sure the general features of most final states will be generic. But there seems to be no analogue of diffraction dissociation. It must go by two-gluon exchange, which appears to be highly suppressed. On the other hand, the collision of two B-mesons is essentially just the collision of two light constituent quarks, and the soft Pomeron is dominant.

There is certainly a role for the hard Pomeron to play, and the natural arena is at higher p_t. The hard Pomeron is essentially a ladder built from gluons, but the fact that the gluons are spin-1 gauge particles creates special features. In particular there are leading log effects not only in $1/x$ but also in q^2, so that very small x is of significance. The largest q^2 is the province of generic central detectors, and perhaps the strongest contribution a full acceptance detector can make is to look at multijet event structure at moderate q^2. Let us define a jet as all the particles within an appropriately placed circle of radius 0.7 in the lego plot (as determined by some kind of cluster algorithm.) Then the density of jets of scale p_t is roughly $3\alpha_s/2\pi$ per event per unit rapidity (and per unit log p_t^2). What is meant by this is that if there is a high-p_t event of at least this scale of p_t, then this is the number of extra radiated jets of this scale to expect in that event.

If this were the whole story the jet multiplicity would not seem to grow fast. But the calculations show the opposite. This occurs because there are jets within jets; each circle of radius 0.7 contains lower p_t jets radiated by the primary jets, but which stay contained within the circle. Also the lego plot itself gets populated with more jets of lower p_t scales; and these also generate jets within jets. The pattern is fractal in nature, and a nice summary of the situation is given by Gustafson and his Lund colleagues.[24]

There has been an enormous amount of work in this area. But when I look for crisp statements of what kind of event morphology should be seen in a multigluon final state, I find little help. A start is provided by Mueller and Navelet,[25] who suggest an inclusive 2-jet measurement at fixed x_1 and x_2 (reasonably large) and look for the growth with s of the cross section due to the multigluon production (mainly minijet, evidently). I would like to see suggestions on what actually builds this energy growth–what do the multijet final states look like in the lego plot? In particular, are the jets (and by this I mean the "experimentalists' jets" involving the circle of radius 0.7) distributed at random as would be the case for photons in QED? The "color-coherence" effects might suggest otherwise.

Perhaps a prototype of what I am thinking about might be of use here. Suppose we set a minimum p_t of 20 GeV for the jets we are talking about, and a maximum p_t not much larger, no more than twice as big, just to simplify the discussion.

Demand a Mueller-Navelet jet pair at the extremes of the lego plot. Then there will be one extra jet per 10 units of rapidity or so, according to our rule. This is not the big number the perturbative QCD people get, again because their calculations go to smaller p_t, and they count the jets within jets.

However, while the average number of extra jets is not enormous, it is not too much of a price to pay to just request them in the final state and see where they go. For example, ask for six extra jets. (The cost in cross section is order 10^4.) The question I am interested in is where those jets preferentially go in the lego plot. The reason is that if one knows the dominant architectures for the final state morphology of the jets at the highest p_t scale present in the event, then this provides all that is needed to predict the number and distribution in the lego plot of all the lower p_t gluons. They will not significantly modify the overall architecture-at least to leading logarithm accuracy. And if a pattern is preferred at the highest p_t scale, it is likely to be present at all scales, because of the property of self-similarity (fractal behavior) of the QCD cascade.

I don't know the answer, but here throw out a conjecture. Add the jets one at a time. The three-jet final state is problematic; maybe the third likes to get as close as possible to the leading jets, or perhaps it likes to wander in the center of the lego plot. Four jets is more interesting. There may be a preference in this case for either double gluon dissociation or a "two-gluon process" (Fig. 4.1a), with p_t balance of the relevant pairs in order to keep the q^2 of the exchanged gluons as small as possible. But it is also possible to have both occur at the same time by aligning the planes of the two pairs. The pattern in the lego plot is shown in Fig. 4.1b for this case, along with the instant generalization to the 8-jet case of interest. I suspect that a symmetric, completely coplanar pattern such as this might represent at least a local maximum in the jet cross section, differential in all angular variables, and with the p_t magnitudes fixed at their minimum values (the value of the experimental p_t cut). (There are two p_t-conservation constraints to be imposed on the angle variables, of course.) Whether there is enough phase space about the minimum to make it a dominant contribution is another question. But if it is not, what is the dominant contribution?[26]

According to the rules of color dipole antennas, the radiation of subleading jets (p_t's below the cuts) into the lego plot is such that each color line in the

(a)

(b)

(c)

(d)

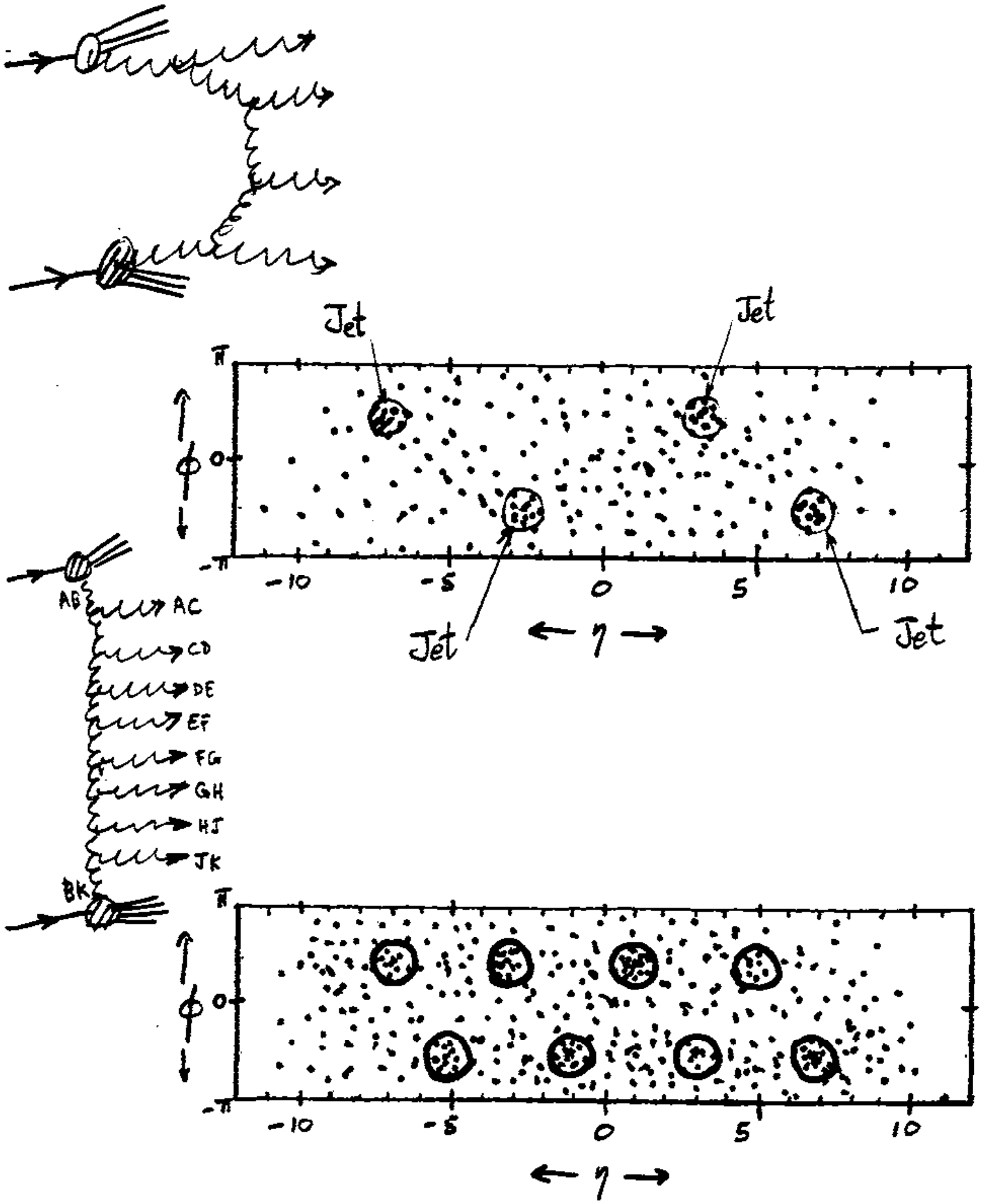


Figure 4.1. Feynman graphs and event topologies in multigluon final states: (a) 4-jet event, and (b) its lego plot, (c) 8-jet event and (d) its lego plot.

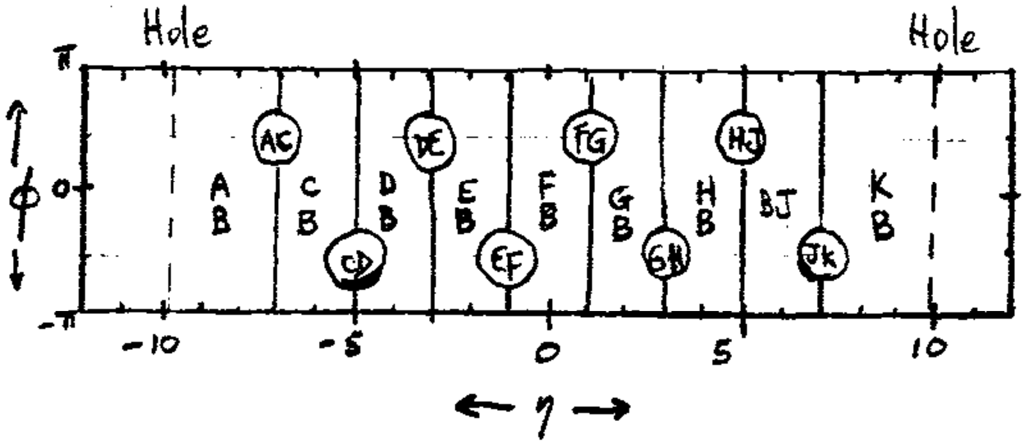


Figure 4.2. Color flow in the lego plot.

multiperipheral diagram is a source of gluons as if it were a quark-pair radiating gluons. We can then trace in the lego plot that the radiation is local in that sense (Fig. 4.2). Evidently the subleading jets will if anything enhance any coplanarity present in the leading order, except to fatten up the jets in the way that happens in e^+e^- annihilation.

Examination of the lego plot shows that these jets more or less fit in the left/right-wing sectors of the spectrometer. The role of radical left/right sectors is completion of the acceptance, and perhaps the use of the quark tag to clean up the underlying-event backgrounds. In this kind of study, one quark-quark collision at a time is quite enough!

One other use of the far forward direction is to study beam-jet radiation. There should be a modification of the multiplicity or p_t distribution in the region of rapidity bounded by the "hole fragmentation" rapidity, *i.e.* the rapidity that the initial-state parton had before interacting. This is best studied in very high p_t binary processes.

4. W and Z production

The cross sections for W and Z production are quite large and extend out to rapidities of 6 or so, with a need for acceptance out to perhaps 8. They are shown in Fig. 4.3. What is the physics? The cross section and production distributions in the far forward direction measure well the quark structure functions. Thanks to the

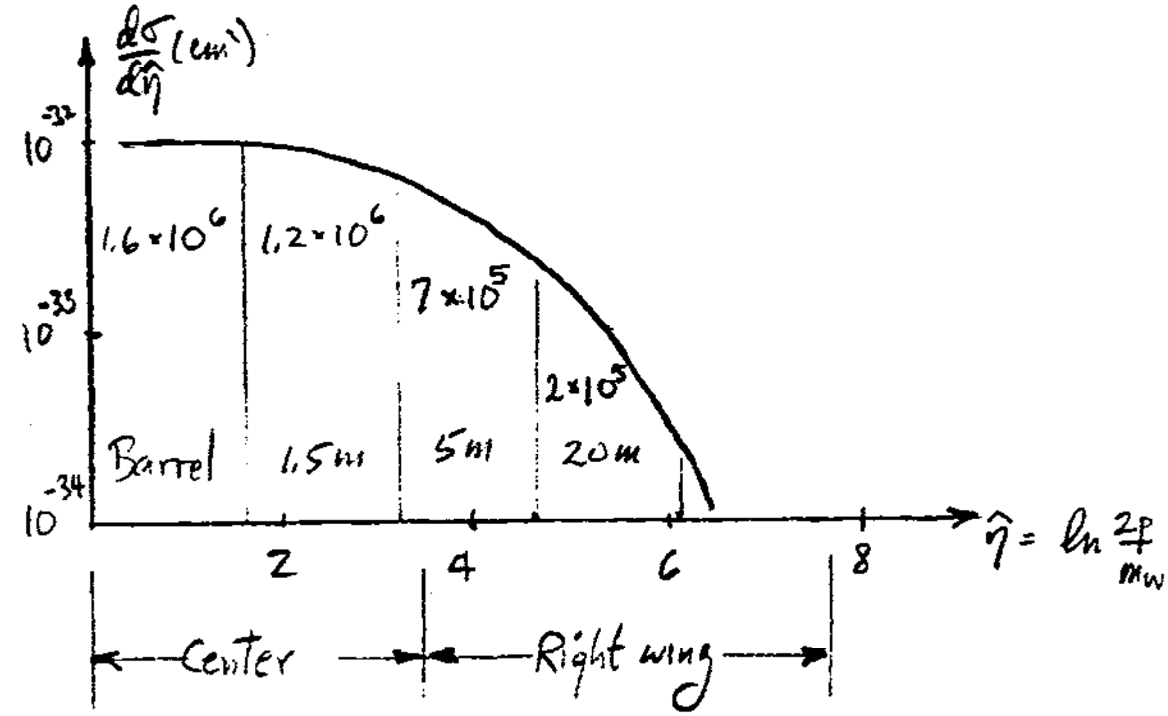


Figure 4.3. Cross sections for W production and the yield per SSC year into the modules of the spectrometer.

evidence for Gottfried sum rule violation,[27] the anti-up and anti-down distributions at the least are not at all well determined, and W and Z production is an excellent way to pin them down. This Drell-Yan process is also an excellent means of studying perturbative QCD, and the acceptance for the beam-jet gluon radiation may be valuable in this context.

It may also be of interest to measure the W production cross section in diffractive events, *i.e.* Pomeron-proton collisions, to get another clean handle on the quark structure function of the Pomeron.

Another possibility which should be investigated is whether missing longitudinal momentum in W events can be measured in this spectrometer well enough to determine the missing p_ℓ of the neutrino. Since forward tracks, charged and neutral, can be measured to a percent or better, and many of the W's have a large longitudinal momentum, this should not be instantly dismissed as impractical.

My personal opinion here is that, while these observations complement and extend the observations to be made with generic detectors, they nevertheless do not provide an especially strong rationale for this spectrometer.

5. Top production

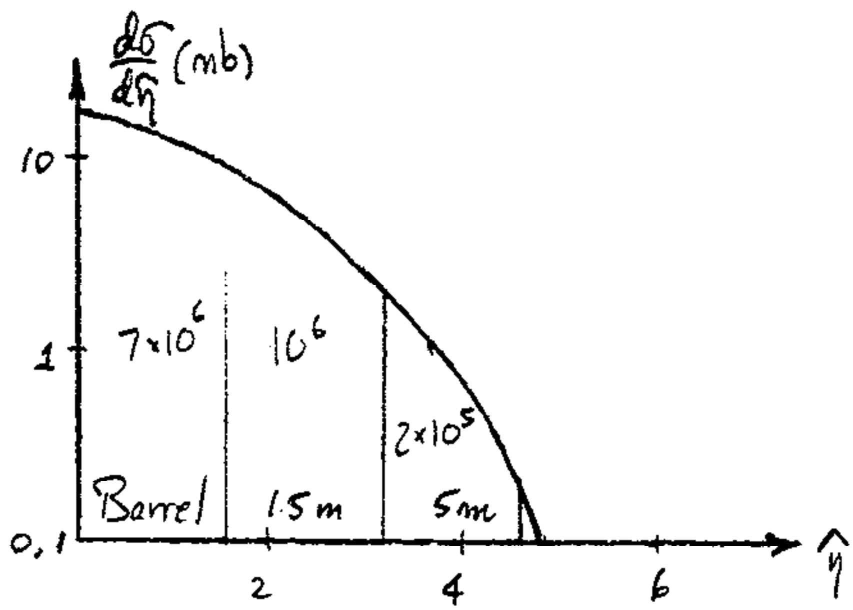


Figure 4.4. Cross sections for top production and the yield per SSC year into the modules of the spectrometer.

For a top-quark mass of 140 GeV, the cross section is much more central than for the W's above. The yield into the full detector (Fig. 4.4) is in excess of 10^7 per SSC year. Even accounting for acceptance for decay products of leading tops, there is precious little beyond an η of 5 or 6. The physics for this detector seems again to be mainly event structure studies of the associated QCD radiation, etc. along with production-dynamics studies in the forward direction, and as always a study of top production by Pomerons.

I checked briefly some exotic production mechanisms, shown in Fig. 4.5. Searching for singly produced leading t's looked interesting, especially via W-exchange, where a measurement would give a direct determination of interesting

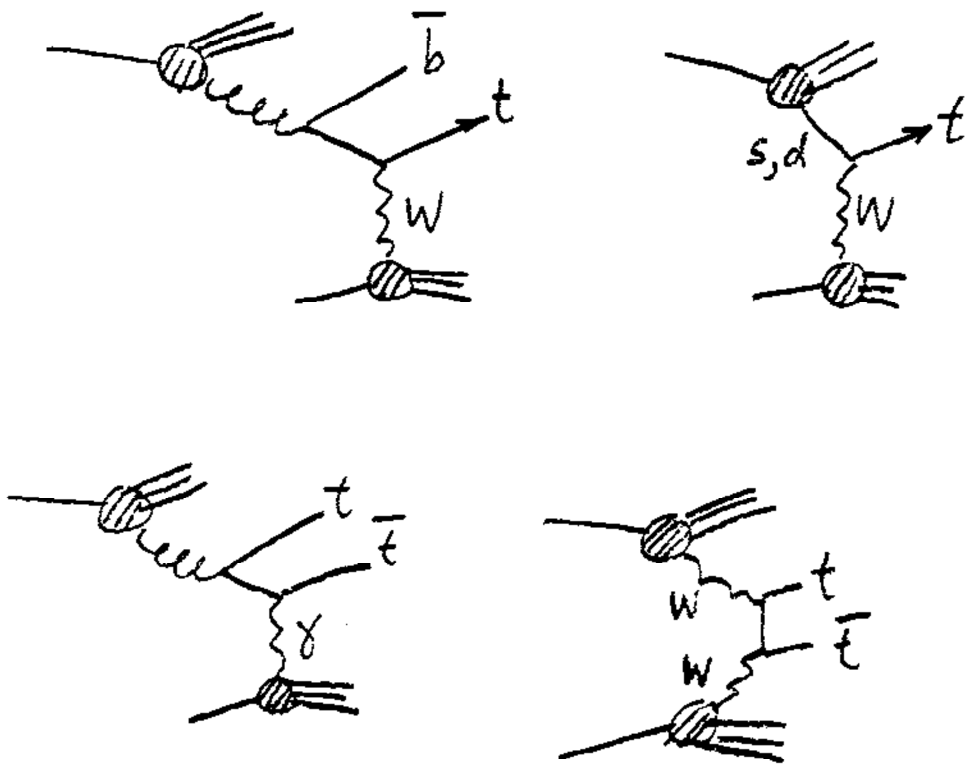


Figure 4.5. Exotic production mechanisms for single top quarks.

CKM matrix elements. A quick estimate of the yield led to discouragement. But it might be worth a more careful look.

Again my personal opinion on this is that this topic will be better done, for the most part, in generic detectors or an optimized B-detector.

6. New particle searches: a "dark Higgs sector"?

Everyone has their favorite list of new physics and new particles: extra quarks, extra leptons, generic axions, leptoquarks, superpartners, extended Higgs sectors, evidence of compositeness, fractionally charged objects or other heavy stable objects, etc. I have not attempted to look at all this in a systematic way. And I expect it in general would be hard to defend this detector as an optimal search instrument in any specific scenario. If one knows what one is searching for, there is a better instrument to be designed. But in the case where one doesn't know and discovers by blundering into a data set that doesn't for some reason fit conventional wisdom, then this device may be very good. But this is a subjective opinion which is hard to defend.

The defense, such as it is, rests on the presumption that per event this detector is capable of acquiring much more information relevant to searches than generic detectors. There is the larger acceptance, the good charged and neutral particle momentum resolution over all of phase space, the probable presence of Cerenkov detection in some regions of phase space, and the modularity, which may allow adaptability and quick response to results from elsewhere regarding where to concentrate one's efforts.

The main deficiency is of course that the most likely place for the new physics is at very high mass scales, where the detector architecture is weakest and where a lot of luminosity is given away. But there is no theorem that says the breakout from the standard model *requires* the TeV mass scale. We have to wait and see.

As an example of what could happen, I close with one from the Higgs sector. Suppose the conventional Higgs particles interact reasonably strongly with another piece of the Higgs sector (the "dark" sector) which contains only gauge singlet particles; they have neither weak charge nor color nor electromagnetic charge. But suppose this new sector has also undergone spontaneous symmetry breakdown, with its own pion-like Nambu-Goldstone-bosons, massless or massive, which shall be called k. (We assume the k's are not eaten by some other gauge bosons.) The relationship of the longitudinal W-Z electroweak modes to the k's is something like the relationship of pions to kaons, except that kaons are not isospin singlet. The relevant new interaction is the scattering of pairs of longitudinal W's or Z's into k's, either resonantly or nonresonantly. The essence of the phenomenology is most easily seen if we assume for simplicity that the dark Higgs sector can be described by a linear σ-model formalism, as usually assumed for the standard-model Higgs sector. Then the coupling between the two Higgs worlds is just a nonderivative quartic coupling, quadratic in ordinary Higgs fields as well as in the dark Higgs fields. There will be an extra dark, massive Higgs boson which we call S, and the main effect of the coupling term between dark and ordinary sectors is to mix S with the usual Higgs particle H. This leads to disastrous consequences. The unmixed S typically decays quite readily into invisible kk final states, with a width orders of magnitude larger than the standard Higgs width, unless the Higgs is in the mass range of hundreds of GeV (See Fig. 4.6). Therefore, unless the mixing is extremely weak, both the H and the S decay overwhelmingly into invisible modes. And even in the very high mass range, there is the possibility of degradation of the conventional signals by a lessening of the branching ratio and/or a broadening of the total width.

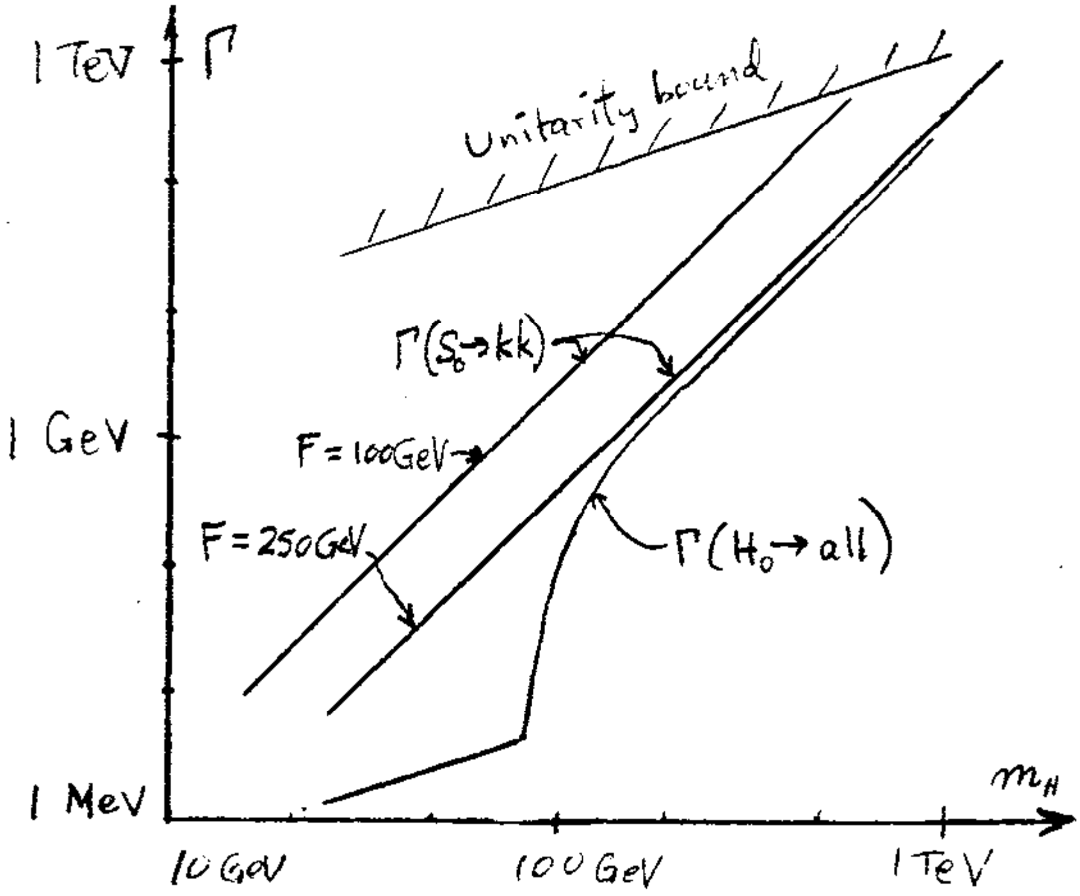


Figure 4.6. Width versus mass of an unmixed dark Higgs boson into a pair of Nambu-Goldstone particles. The choices of decay constants are $F = 250$ GeV and $F =$ 100 GeV (roughly ten times the minimum value allowed experimentally). The internal group for the dark sector was taken to be 0(4). Also shown is the same quantity for the conventional standard-model Higgs boson.

The mass-scale of the dark Higgs sector, as estimated from the size of its vacuum condensate F (analogous to the Higgs' 246 GeV), could be quite low. A value of 10-20 GeV would limit the dark Higgs mass to less than 40-80 GeV (the unitarity constraint), consistent with LEP phenomenology. The phenomenology of other weak processes needs to be examined, along with cosmology, to see whether other limits on F exist, and whether limits exist on the mass of the k.

This general idea is present in the literature, in the context of majoron models,[28–30] and is briefly mentioned in the Higgs-hunter's guide.[31] But while the possibility of the Higgs decay into invisible modes is discussed at length, those models put the condensate into the electroweak sector (Their new Higgs fields, unlike the ones discussed here, transform nontrivially under the $SU(2) \times U(1)$ gauge group.). Those models have more theoretical motivation than a dark Higgs sector has. Nevertheless these k's might be good for something, like cygnets or dark matter, although I have nothing concrete yet to suggest. However, while these words were being written, Barbieri and Hall[32] independently came upon the dark-Higgs scenario in the context of interpretation of the evidence for a 17 keV neutrino.

From the point of view of SSC Higgs searches, this scenario is not just dark, it's downright morbid. But the situation may not be hopeless. The full-acceptance spectrometer has at least a fighting chance to try to find these Higgs'. The same technique described in Section II.6 for the intermediate-mass Higgs-particle search can be attempted. It might work even given that the k's are long lived or stable, or decay into undetectable objects. The signature in the lego plot would be again the evenly charged beam jets with their tagging jets of 50-100 GeV of p_t on the edges of the rapidity gap. And in between there would be nothing in the rapidity gap. There would be background events from single or double Z production, where the Z's decay into neutrinos. It is calculable. And one might do a pretty good job on reconstructing the four-momenta of the beam jets, thereby determining the invisible mass produced by the W pair (See the comments in Section IV.4). There is another background from single W-exchange events, where some of the beam-jet energies are lost, *e.g.* to neutrinos. While this may be serious, the fact that the tagging jets are coplanar for the background and noncoplanar for the signal should be of considerable help.

This dark-Higgs scenario looks very unlikely. But recall the problem of anticipating the nature of the strong interactions, given only knowledge of the nucleons and pions and their interactions at low energy. And then evaluate the credibility of a claim that were $\pi - \pi$ scattering to be done at the energy of the proposed sigma resonance (the "Higgs particle of the strong interactions"), the most prominent feature would be an S^* bound state of Goldstone modes called kaons. Then evaluate the credibility of an additional claim that the key to the strong interactions lay in the existence of a symmetry group (color) which commuted with all known or imagined symmetries of weak, electromagnetic, and strong interactions. The dark Higgs scenario is no less credible than all that!

V. The Detector

1. Basic Architecture

I must begin with words of apology to those who, unlike me, really know how to design a spectrometer. But I have thought about this for some time[1] and for better or worse feel the need to document some of the ideas.

The general description was given in the introduction. The spectrometer is essentially two 20 TeV fixed target spectrometers face to face. The central barrel region is relatively unremarkable, and the next sectors (the right/left wings) are likewise reasonably familiar objects, extending from a few meters downstream of the target to 100 meters or so. Beyond are the radical right/left sectors extending out to a kilometer or so, with the final-focus optical elements in the front ends. In arriving at this description, I first went through a simpler exercise, which will be briefly described, since in my mind it expresses some of the most basic properties of instruments of this type. The example is very symmetric ("Lorentz-boost invariant"), and many properties of it exhibit simple scaling laws which I find helpful. However, as an actual prototype of a detector it is not at all optimized. The situation is a little like the idealistic architect who designs an edifice based on grand principles of esthetics and symmetry, only to find that his clients find it impractical, with the final result only poorly reflecting the grand design–even though the occupants are much happier.

The starting point is the requirement of full acceptance for photon detection, which requires calorimeter walls. An endwall even at one kilometer needs quite fine grained resolution, and it is hard to move it in much closer. Obviously it can only cover the most forward angles and a sequence of annular calorimeter walls upstream are needed. For simplicity make them of identical size and shape. A basic parameter is the aspect ratio, the ratio of outer to inner diameter, somewhere between 2 and 10, say. I take it to be 4. Then with this choice, each wall is four

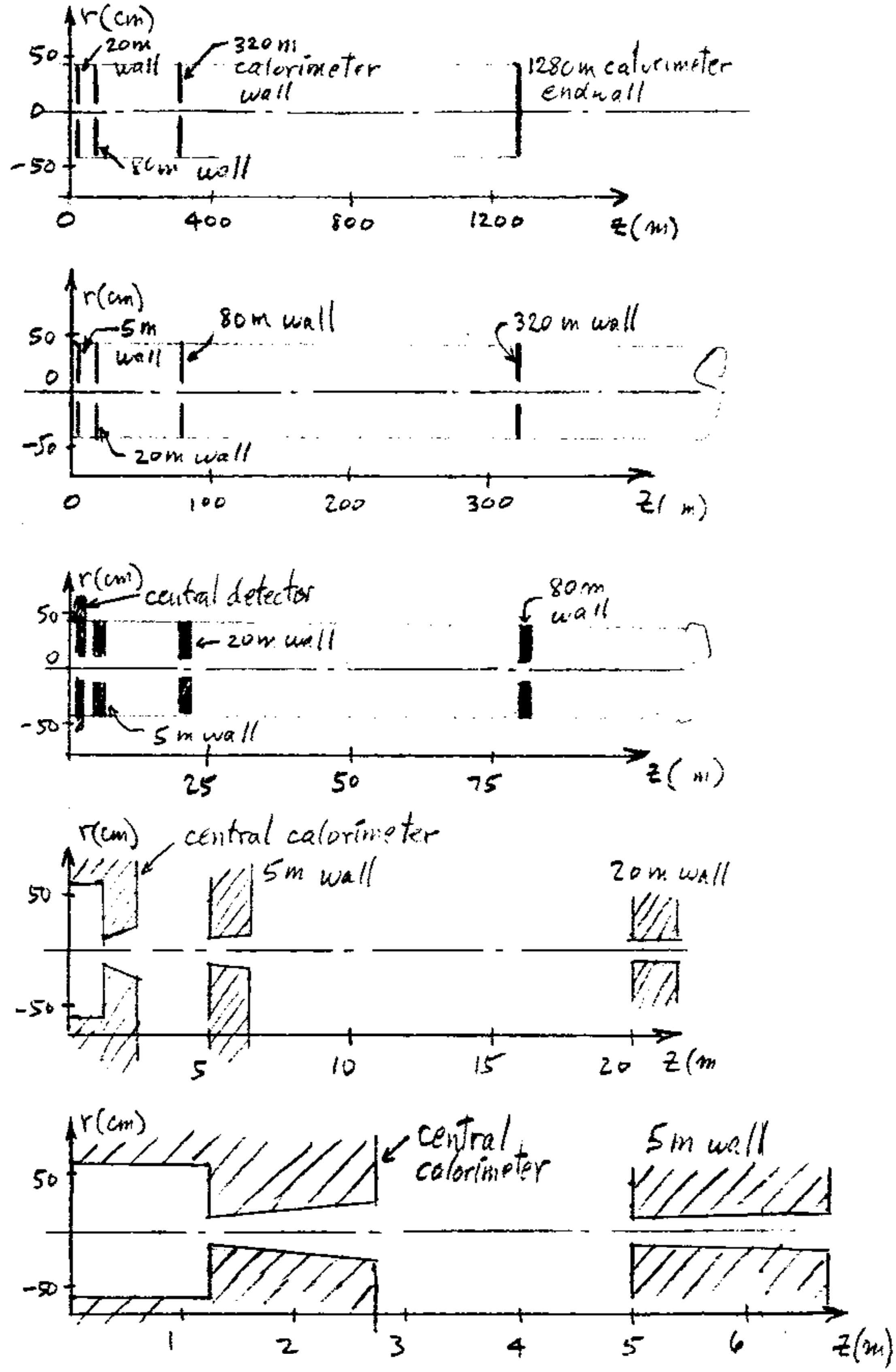


Figure 5.1. The first try at the calorimetric architecture of the spectrometer. Magnetic elements, not shown, go behind each wall; a solenoid may as usual be used for the barrel region.

times closer to the collision point than the previous one. With an endwall at 1280m, this give annular walls at 320, 80, 20, and 5m, before barrel geometry enters the game. A cartoon of the calorimeter architecture is given in Fig. 5.1. Just this simple consideration of photon detection divides the spectrometer into modules, each labeled roughly by its rapidity, and with a rapidity window of $\log 4 = 1.4$. The total number of such modules is of order 10, and what goes in each one can again be identical. In the shadow of each calorimeter wall (where else?) goes an analyzing magnet with a p_t kick for the particles of interest of, say, 0.5-2 GeV. Then if the same number of tracking elements are placed within the module, spaced in proportion to the longitudinal dimensions (projective geometry), then the momentum resolution (as estimated from the magnitude of the sagitta) will not depend on which module one chooses. And, as discussed a little more in the next subsection, it can be expected to be quite good for the generic tracks.

If the tracking elements are also annular, as ideally they should be, then the acceptance in rapidity again is the log of the aspect ratio, and nowhere are tracking elements burdened by an inordinate multiplicity of tracks. Despite the total mean charged multiplicity of order 100, the mean number to which a given module is sensitive is about 10. I would guess the density of tracking elements should be reasonably uniform in $\log r - \log z$ space, and a cartoon of this is shown in Fig. 5.2. (This is clear evidence that this spectrometer is not yet ready to turn over to the technicians.) The case shown assumes a big beam pipe downstream with silicon tracking inserted within via Roman-pot technology. Alternatively one might opt for as small a pipe as possible everywhere, with all tracking on the outside. More will be said about this in subsection 5.4.

I bias toward small transverse dimensions, with inner diameter of a calorimeter wall of 20 cm. and outer diameter 80 cm. I also choose a goal of 0.03×0.03 for the "pixel size" in lego variables of the electromagnetic calorimeter. This implies a resolution in space of a few millimeters in locating the electromagnetic shower cores. Going to even smaller aperture has the disadvantage that the wall intercepts too many K_s, Λ, etc. before they decay, since (independent of rapidity) the mean transverse separation from the spectrometer axis of the decay vertices is typically several centimeters. The small transverse size implies, by the way, a mean $\pi \to \mu$ decay probability of 3-4% per track, essentially independent of rapidity.

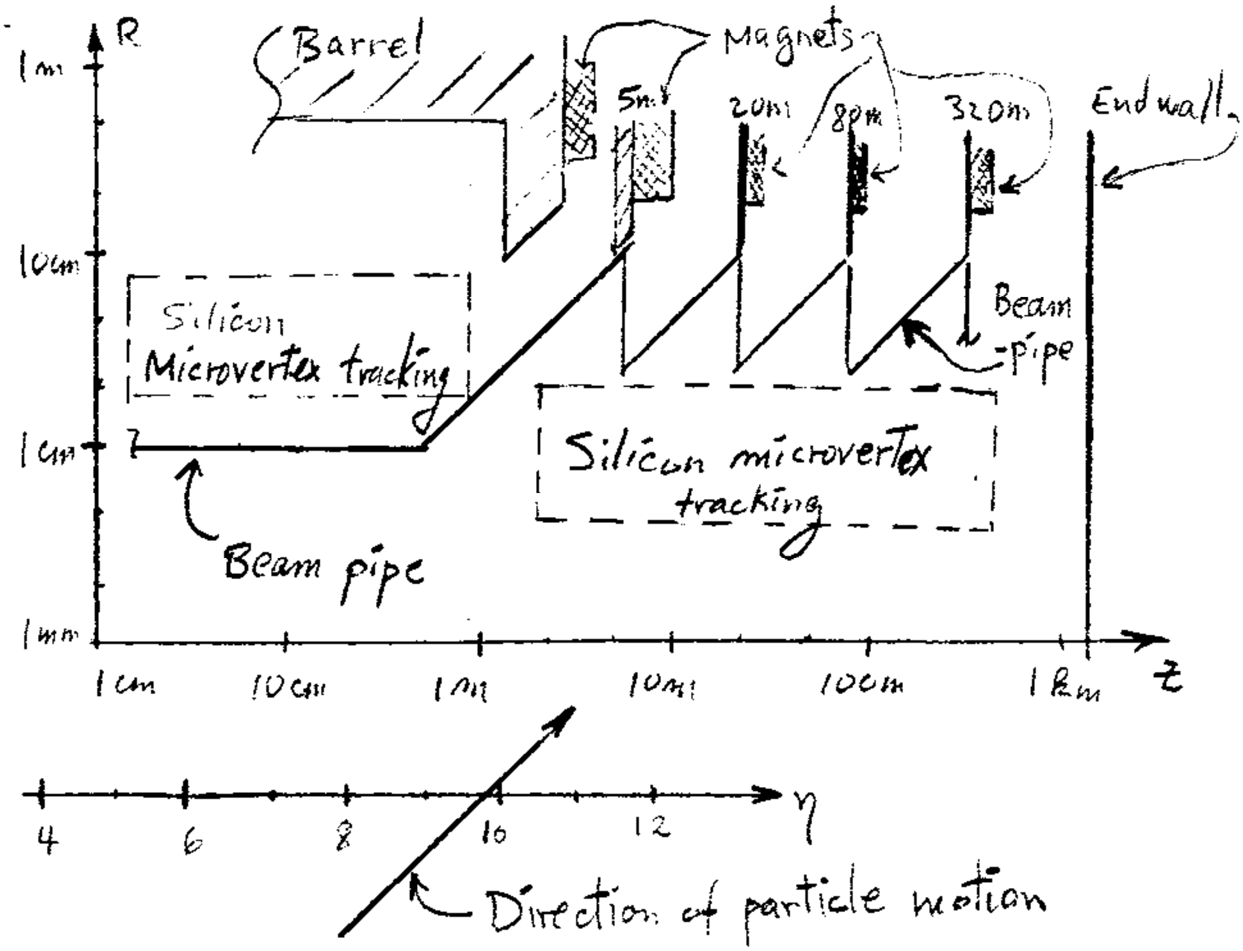


Figure 5.2. The same spectrometer plotted in log-log coordinates. There is much outer tracking, which is not shown.

Thus far the discussion has emphasized the uniformity of each module and its contents. However this symmetry inevitably will (and should) be broken by their inhabitants, who will optimize for the physics available at that rapidity, and will adapt to the myriad of practical problems, *e.g.* backgrounds, which are not at all boost-invariant. I regard this as a feature to be encouraged: module-to module variety (consistent with the overall general architecture and with good-neighborliness) is a way of optimizing performance, while standardization is not. [Indeed, between here and the end of this document, the 320m calorimeter wall will have moved forward to 140m.]

As far as particle identification is concerned, muons should be straightforward.

Evidently the calorimeter walls will be made thick enough to more than contain the hadronic showers without much tonnage; 15 tons/wall is quite enough. Electron identification might be enhanced by TRD's. Cerenkov identification is something I don't understand well, but there will certainly be modules in the left and right wings where it is practical.

The very small transverse dimensions of the walls seem to me to probably compromise the quality of hadron calorimetry that can be done. This is not to say that the absorber behind the electromagnetic walls should not be instrumented, only that I don't have confidence in the quality of the results without being convinced to the contrary by some realistic simulations. But even if the result is discouraging, there is the possibility of reconstructing the jets track by track. The limitations of this method include the following:

a) Poor p_t resolution of the leading charged particle. Take a p_t of 100 GeV for the jet. The leading particle has on average a third of that. With a 30 percent resolution, this contributes 10 percent to the jet resolution. Provided all remaining particles are found, they do not appreciably increase this error.

b) Missing p_t from neutral K's, neutrons, etc. No more than 10 percent of the jets will have more than 10 percent of the p_t contained in such particles; this seems an acceptable loss (loss because high p_t is reclassified into a lower p_t bin mistakenly.).

c) Poor two-track separation of leading particles in a jet. Even at the aforementioned 100 GeV p_t scale, I get, without taking account of the magnetic bending, a typical leading-particle separation of several millimeters at the calorimeter wall where they are destroyed. To me this seems safe.

Therefore it seems possible that a decent job on jets can be done up to the 100 GeV range of p_t, even without the hadron calorimetry. But of course such a claim needs backup from simulations.

No mention of where the circulating beams go has been made as yet. This is best deferred until after discussion of practical questions involving the machine lattice and the beam-pipe problem. A standard 75 μrad crossing angle appears appropriate, so the beam-dynamics questions are the same as for the intermediate-luminosity detectors—out to 100m or so. It looks like the final-focus quadrupole magnet system should begin at about 140m, with both circulating beams close to each other (*i.e.* within 1 cm or so) at that distance. The architecture of the 140-320m module is then dominated by the magnetic elements, which in turn is dominated by accelerator considerations. While this is the heart of the proposition, I haven't done too much on it because of the need for feedback from the SSC. More is said about this in subsection 5.6.

2. Tracking and Optics

If dipole magnets are used behind the calorimeter walls, with p_t kick of 1-2 GeV, then the sagitta of a typical track of $p_t = 10$ GeV (chosen to keep the geometry very simple) is about 2 mm if the particle hits the inner edge of the calorimeter, at radius of 10 cm. The sagitta will be 4 times bigger (for the same p_t of the incident particle) at the outer radius, leading to a dependence of resolution on rapidity of a sawtooth character (Fig. 5.3). The absolute normalization is a blind guess based upon comparison of what is achieved with a few existing spectrometers. Also shown is the resolution of the electromagnetic calorimeter, which in the forward region becomes comparable to the charged particle resolution.

Important is the beam-pipe, which imparts, at the least, a lot of multiple-scattering to forward tracks passing through it at grazing angles of incidence. If a big beam pipe is chosen, this problem can be mitigated by measuring the production angle only inside the pipe and the momentum only outside the pipe-although the sagitta will be reduced and resolution impaired. My choice of parameters does not do well on this point but iterating the design would help. If the beam-pipe is kept small, one should try to minimize the fraction of charged tracks penetrating the pipe at grazing incidence.

A natural alternative to dipoles would be use of quadrupoles. This choice has both advantages and disadvantages. It evidently is easier on the machine optics. Magnet cost is not a consideration; these magnets are inexpensive, and a p_t kick at maximum aperture ("at the coil") of 2-3 GeV is roughly equivalent in its effect to the 1-2 GeV kick of the dipole. But the main advantage in my mind is that the quadrupole field does not impart large kinks to a particle trajectory when it is very close to the beam axis (before it is measured) nor does it impart large kinks to it even when it is within the beam pipe, where (for choice of a large radius) silicon microvertex tracking has the job of finding charm/bottom decay vertices. An example of what I mean is shown in Fig. 5.4 which shows an example of the

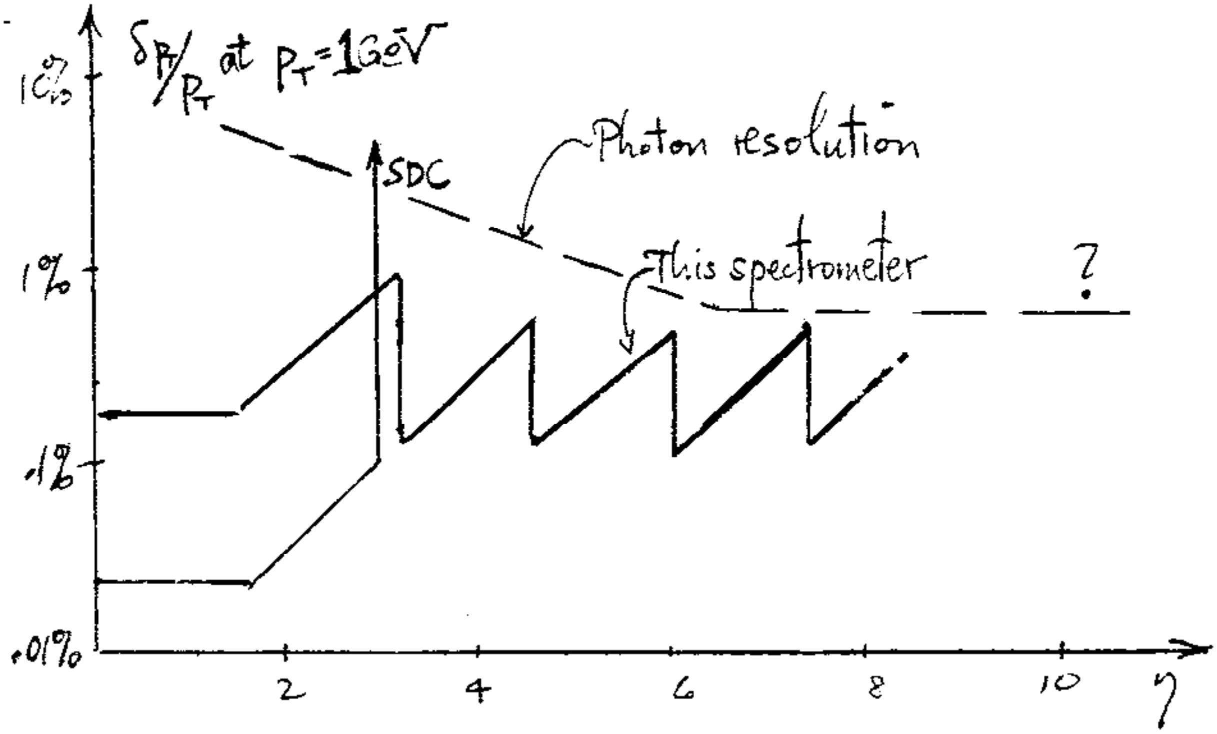


Figure 5.3. A guess at the dependence of resolution on rapidity. Dipole magnets are assumed. Resolution in the radical-right sector is discussed later and shown in Section VII.

trajectories in the transverse coordinates for the two cases. In the case of the dipole the production angle has to be found by extrapolation through the dipole bends (with the requirements of very accurate field maps), while for the quadrupole it is a relatively small correction.

A disadvantage of quadrupole magnets is that (again at fixed p_t) the rapidity-dependence of the momentum resolution has four times as much fluctuation as for the case of dipoles. The resolution is 16 times worse when the particle hits the wall at 10 cm than it is at 40 cm. A cartoon of this case is exhibited in Fig. 5.5. This is best mitigated by using a smaller aspect ratio, say 2 instead of 4, or simply placing one more quadrupole (with at least twice the aperture) in each module. There may be disadvantages at forward angles, where it may be useful to sweep charged particles very hard through the beam pipe to keep that problem minimized. More on that question appears in subsection 5.4.

Momentum resolution in the radical-right/left sectors should be very good

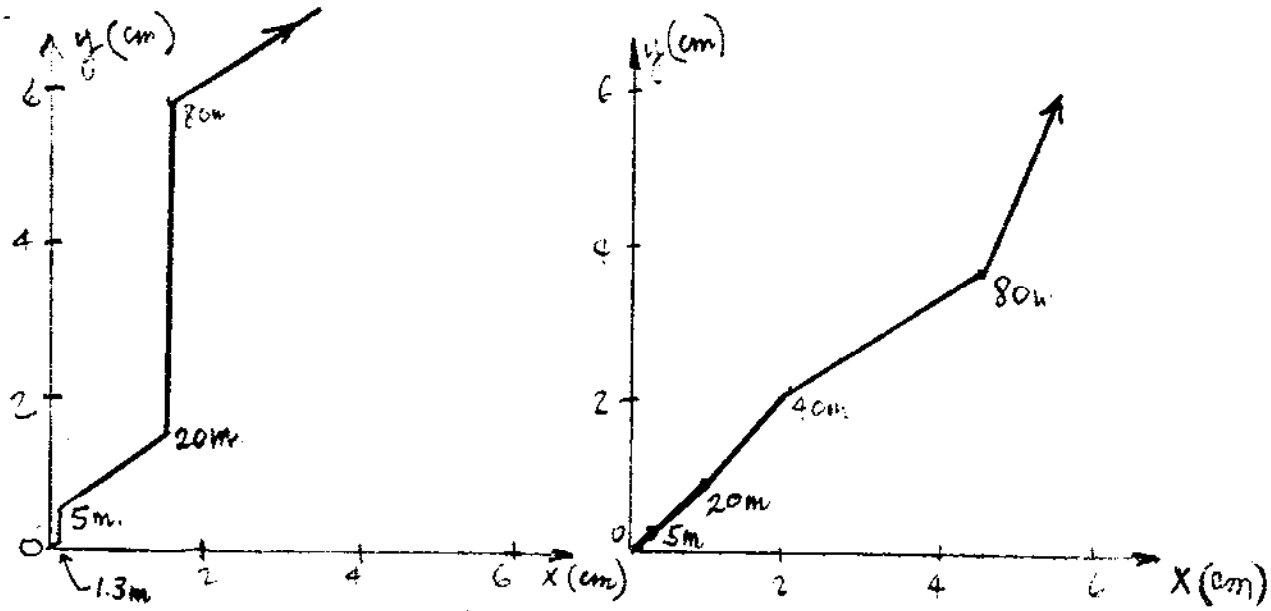


Figure 5.4. Beam's eye view of trajectories of particles through (a) a sequence of dipole magnets, and (b) a sequence of quadrupoles. We have chosen $\eta = 8$, $p_t = 1$ GeV, and $p_\ell = 1.4$ TeV.

because of the very strong sweeping. There, of course, the use of quadrupoles is mandatory, except for far downstream, where the circulating beams are split apart by a dipole bend. A few more words on this appear in subsection 5.6.

Strong sweeping, either with quadrupoles or dipoles, may be a useful feature in other modules as well. If almost all particles of generic p_t are swept to the side without hitting the calorimeter walls, the occupancy of the hadron calorimeter will stay low (neutrals plus high p_t secondaries, mainly), and the fan-shaped beams exiting from the sides (in this case the typical multiplicity per fan is 2 or 3) might be transported into external Cerenkov detectors, etc. for further measurement before being abandoned.

Finally, there is the question of the central barrel. Here again there is the possibility of an unconventional architecture. Perhaps the barrel should be cut in half at $\eta = 0$, with readout from the interior of the barrel flowing upstream to 90 degrees and then outward through a gap. There is in this low mass-scale detector nothing sacred about rapidities near zero, and the loss in rapidity per unit laboratory angle is minimized at 90 degrees. So also is much of the local

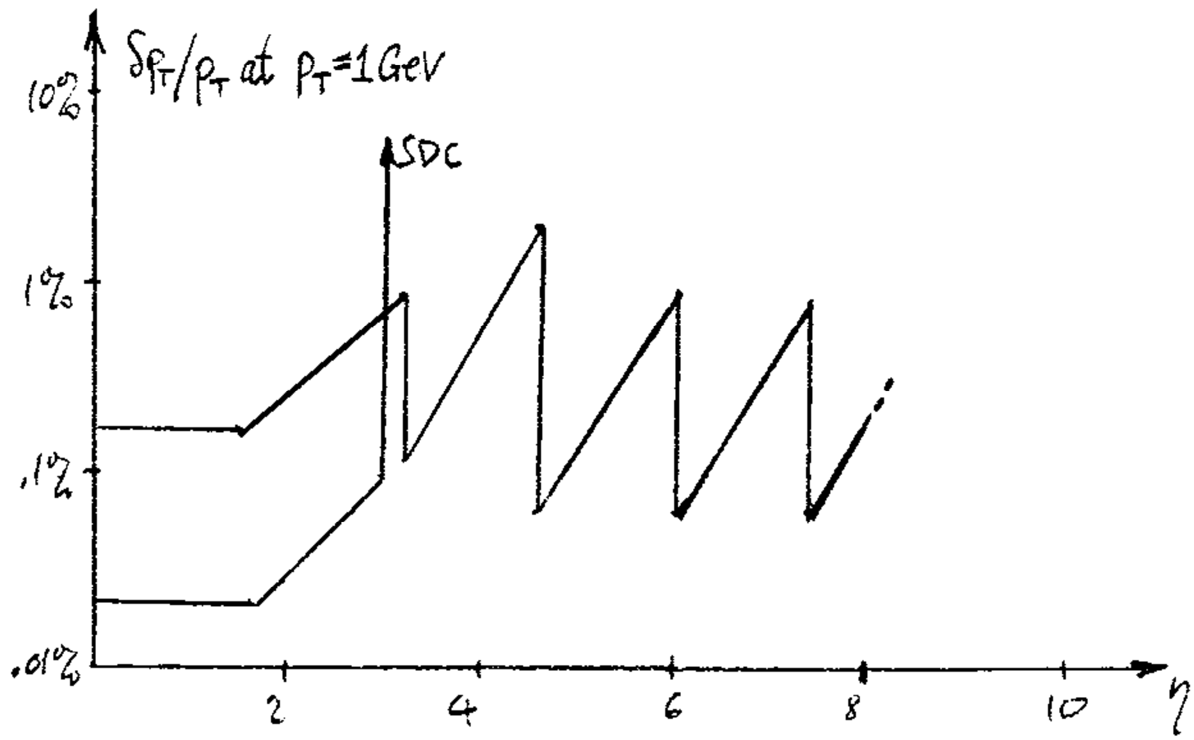


Figure 5.5. A guess at resolution versus rapidity with quadrupole magnets chosen instead of dipoles.

background, I would think. Also the hardware for the gas-jet target is most easily dealt with in such a solution.

Even more unconventional would be consideration of a large-aperture, barrel quadrupole magnet for the central tracking. Most of those soft tracks which curl up in annoying ways in solenoids are here swept outwards in the horizontal and vertical planes.

All of these comments are meant only in the context of suggestions for study and evaluation; none of them as advocacy. I detect a tendency for spectrometer designers to simply try to scale up what was done at lower energy without starting from scratch in their thinking. SSC conditions are sufficiently extreme compared with the AGS or CERN PS, where I see the historical origin of most contemporary fixed-target spectrometer architectures, that maybe a fresh look is in order.

3. Data acquisition and event selection

While my ignorance of this subject is profound and almost complete, this does not stop me from rendering a few opinions and attempting a few estimates. One reason that I am so emboldened is that the apparatus is so long that just the finite speed of light appears to create some constraints on the data-acquisition architecture.

Consider a double diffraction-dissociation trigger, for which one demands hits on the 1 km endwall and no hits in the central part of the detector. Then an event occurring at time zero registers on the endwall 3.3 microseconds later. At least another 3.3 microseconds is needed to send this information back to the central detectors. Those detectors will be queried as to whether they saw an event during the $t = 0$ beam crossing. They must be able to supply the evidence that the answer was an unequivocal no.

Therefore it seems that the minimum time interval for a Level I trigger decision is 7-10 microseconds. During that period all information in all modules for all beam crossings must be retained. In other words, there must be massive pipelining and buffering, along with local preprocessing, at the very least to zero-suppress and compactify the data before it enters the pipeline.

I guess event sizes (per module) as follows:

	Bytes/channel	Occupancy per event	Channels	Bytes/event
Tracking	10	1%	40K	4K
Calorimetry	20	5%	30K	30K
Silicon strips	5	0.3%	500K	8K
Total				42K

This wild guess (which uses in part a few inputs from a Cornell B factory proposal[33]) is based on a rapidity acceptance of 1.4 units. Comparisons with other detectors that I could find (which cover 3-6 units of rapidity) include 200K for the CERN B-experiment of Schlein *et al.*,[34] 100K for the CESR B-factory proposal, and 20K for the SFT.[35] So my numbers appear to be in the middle of the range.

The only possible unique feature for this detector is the long elapsed time before the Level I trigger occurs. Here the SDC EoI provides a useful comparison.[36] Its basic front end architecture seems to be quite similar to this spectrometer. There the Level I decision occurs at 1.5 microseconds. But since there is an event every beam crossing and the event size is 1 Mbyte, the front-end buffer has to have a bandwidth of 10^5 Gbytes/sec, compared with this spectrometer's 600 Gbytes/sec (we generously assume 15 modules for the full detector). In other words, the front-end pipeline is 6 times as long but has 150 times less area than the SDC's.

I found another comparison in the CERN-LHC B-physics initiative of Schlein *et al.*[34] It has 100-200 Gbyte/sec into the pipeline, with a Level I trigger occurring 15 microseconds after the event, with output of 1.6 Gbyte/sec.

I conclude that this device does not involve any data acquisition problems not addressed by others. Since the filtering is at most "only" a factor 10^4 (10^9 events/year recorded out of 10^{13}/year acquired), I assume the problem of creating appropriate—and highly flexible and adaptable—event selection algorithms is feasible. And in parallel with the Level I decision process of the spectrometer as a whole, individual modules can preprocess data, share data with neighbors, and create their own semilocal (in rapidity) data-analysis channels, including local permanent storage, consistent only with the requirements of the experiment as a whole. This might include data samples in which the experiment as a whole has no interest. This possibility of local or semilocal "autonomy" seems to me to be a virtue of the modular nature of the detector and therefore should be encouraged whenever possible in the design of the overall data-acquisition architecture.

In general, I bias toward a data acquisition and event selection system as sophisticated as possible consistent with a prudent budget. One of the most important features of this spectrometer is the very large amount of information per event acquired. Therefore the processing power applied to the data set should be maximized.

4. The beam pipe

As far as I can see, the most troublesome problem with this detector has to do with the beam pipe. With typical angles in the submilliradian range, a 1mm beam pipe presents more than a meter of material to particles going through it at grazing incidence.

An immediate question to address is how thin the pipe can be made. For no particular reason I first assumed that it must be of a diameter between 5 and 20 cm in order to satisfy the accelerator specifications (high vacuum, reasonable impedance presented to the beam, physical aperture at least that in the normal machine cells, etc.) Otherwise it might be tempting to go even bigger and evacuate the whole spectrometer, with a 1m diameter beam pipe. Indeed this option may still be of use if one were led to a very thin beam pipe, too fragile to withstand atmospheric pressure on the outside. Another constraint is that the pipe have a conducting inner layer to provide acceptable impedance to the circulating beams. I do not know the correct thickness, which must be of order the skin depth, but take it here to be 20 microns of aluminum, an aggressive choice.

Alternatively, in the Berkeley '87 B-detector study,[37] a 300μ beryllium beam pipe of 2 cm diameter was chosen, with all tracking on the outside. This has many advantages, but at least one disadvantage: the charge determination of beam-jets, possibly important for electroweak physics and low-p_T physics, is probably more difficult in that case. In what follows, I assume (without prejudice) the option of a bigger beam pipe and Roman-pot silicon microstrip tracking within.

There is some R&D on thin beam pipes under way in connection with B-factory designs. There exist very low density foam materials such as silicon carbide or boron carbide with densities of order 3 percent of normal densities and with robust mechanical properties. So it is thinkable that the pipe could consist of this material, of order a millimeter thickness, with the aluminum conductor on the inside and some similar material (thinner?) on the outside, making a sandwich sufficiently strong to withstand atmospheric pressure. (I am indebted to Steve Shapiro and Wayne Vernon for informing me of these developments.)

Hereafter we assume for this option that the pipe thickness is 1 mm of C at density of 0.03 that of graphite, with a 20 μ layer of Al on the inside. This gives an interaction probability of 15% for a 1 mrad angle of incidence. While the interaction of the charged hadrons with the pipe appears to be the worst problem, we begin with the question of photon interactions.

There are a large number of candidate pipe geometries to consider. A variety of these are documented in the Berkeley '87 B-detector study. It seems that if n experimentalists get together to decide what to do there will be at least n options debated. Here we choose uncritically a set of truncated cones as shown in Fig.

5.6. The vertex of the cone is at the collision point, and the calorimeter edge gets shadowed by the cone.

The chance of a photon hitting a cone is (assuming one truncated cone per module)

$$1\,\mathrm{mm}/(2.5\,\mathrm{cm} \times 1.4) = 3\% \ .$$

When it does, it will convert in the first radiation length (10 cm in the Al, 5m in the foam). But unless the electron energy is very high, the multiple scattering will be large enough that the electron and positron will exit within the first radiation length. Therefore below that energy the cascading is suppressed, and there will be relatively little soft charged shower debris. This has been checked with an EGS run (I thank Ralph Nelson for his generous assistance). Essentially what happens is that the critical energy of the cascade occurs at about 15 GeV instead of 15 MeV. Assuming incidence of the primary photon onto the upstream edge of the pipe, and photon direction parallel to the pipe surface, the number of electrons emergent from the beam pipe is about 55/TeV, and the number of photons is 70/TeV. Their distribution in angle and energy are shown in Fig. 5.7. We see that the products are collimated within a few milliradians, so that it may even be possible to salvage some information by examining the debris on the calorimeter wall.

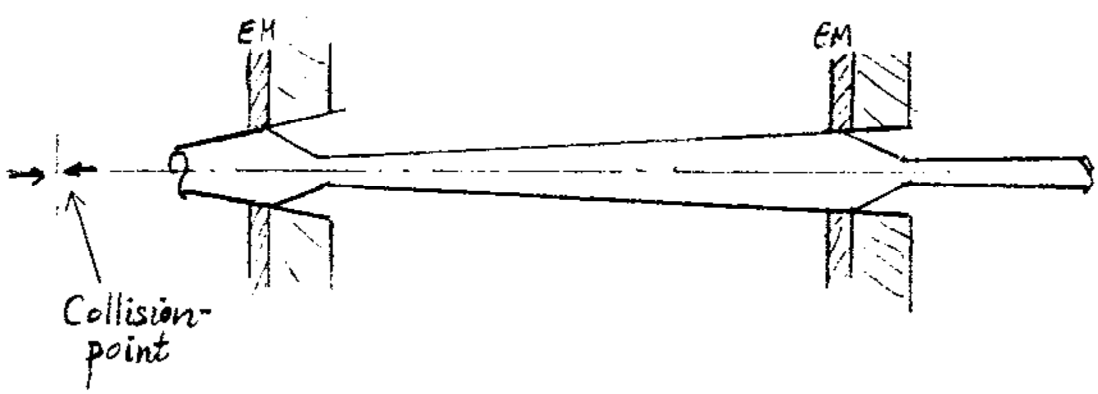
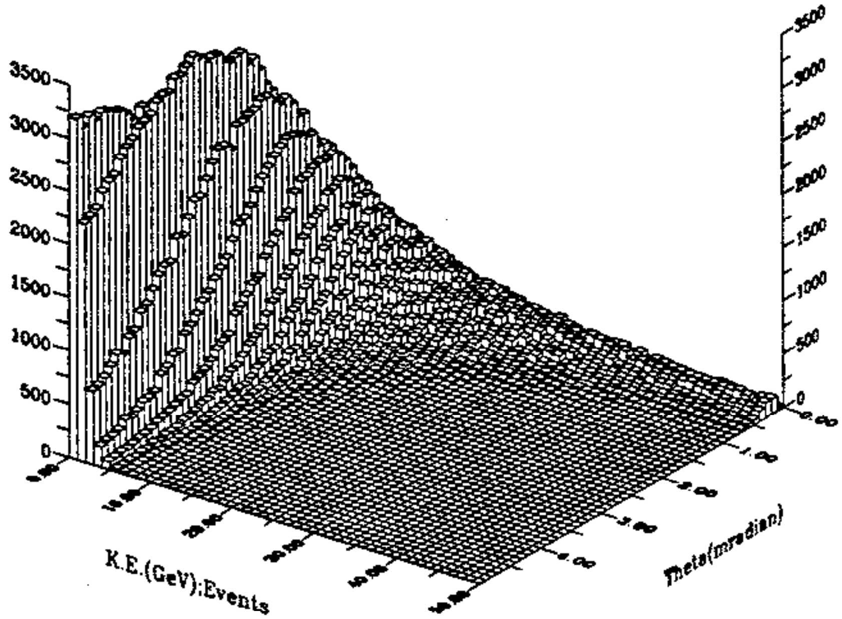


Figure 5.6. A candidate beam-pipe geometry.

ELECTRONS



PHOTONS

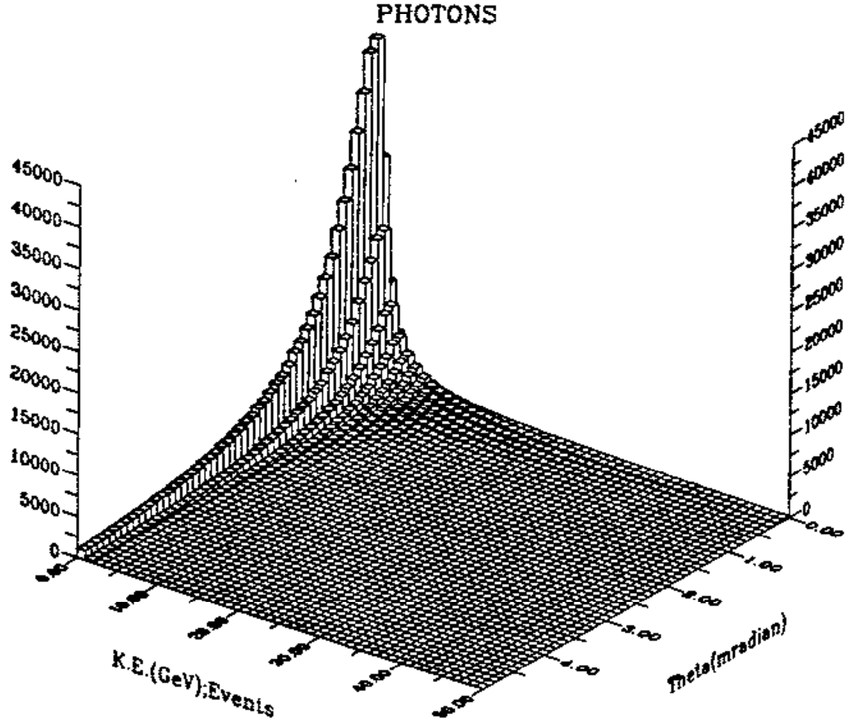


Figure 5.7. Energy and angle distributions of electrons and photons emergent from a 1 mm layer of C (3% normal density), with 20μ Al on one surface, when 1 TeV photons are incident on the middle of the edge of the layer (and parallel to it). The vertical scale is arbitrary.

In any case there is on average one problem per 5 modules per event, with this most likely being a photon loss mechanism and not the creation of unacceptable background.

Dealing with the charged particles is more difficult. But when charged particles hit a beam pipe, all reaction products escape (to a high degree of probability) without reinteraction. Furthermore, where the problems occur, the spacing of tracking planes is so sparse that on average most reaction products escape detection completely. So the phenomenon is again most likely a loss mechanism, rather than a source of unacceptable background (in the sense of creating too much confusion for pattern recognition to succeed).

To study this problem properly requires a choice of magnetic-field architecture and tracking studies with realistic production spectra. I have made a few hand calculations to try to get a feel for the nature of the game. The strategy I assumed was, assuming a large beam-pipe-diameter, to bend as strongly and as soon as possible to sweep particles out of the pipe region before they get too far downstream.

The example taken was the original cartoon (Fig. 5.2), with each magnet (2m, 5m, 20m, 80m) taken to be a dipole with a p_t kick of 1.5 GeV. I then followed the motions of an ensemble of particles with p fixed at 100, 200, 400 GeV, ... 12.8 TeV and p_t restricted to be less than 700 Mev. From this one obtains the distributions in Fig. 5.8. These numbers to me look encouraging. The existence of "good" and "bad" momentum bands emphasize the importance of serious tracking studies, as well as the importance of physics inputs. One must know which momentum bands are the most important to optimize, because it is likely to be at the expense of some other band.

I have not repeated this exercise using quadrupole optics. It should be done. I expect the result to be worse because of the weak bending at small impact parameters. But that might be compensated by choice of stronger fields. The exercise also needs to be repeated using a 2-4 cm diameter beam pipe.

My conclusion from all this is that while the beam-pipe problem is a heavy one, with no clear design choice favored (at least to me), there appears to be more than one viable strategy for handling the problem.

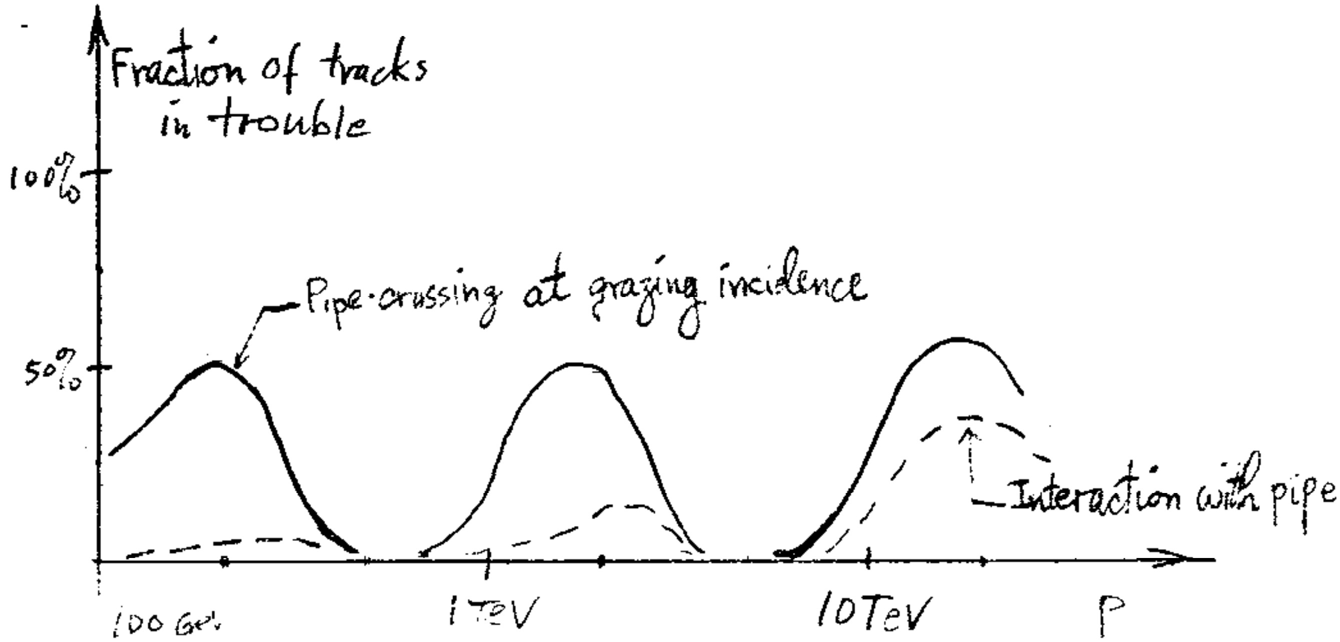


Figure 5.8. The fraction of particles of given momentum which cross the beam pipe at grazing incidence (solid line) and interact in the beam pipe (dashed line). The architecture of Figs. 5.1 and 5.2 is assumed, with dipoles of $\Delta p_T = 1.5$ GeV behind each calorimeter wall.

5. Backgrounds and radiation damage

Other than the troubles with the beam pipe, candidates for background problems include

a) Beam-gas interactions within the detector.

b) Particles, especially soft neutrons, electrons, and photons, created by beam-beam collision secondaries hitting calorimeter walls and apertures.

c) Albedo from collision products which strike the general environment (walls, floors, etc.).

d) Beam halo (especially elastically scattered protons and muons) interacting in the detector.

I have no idea of the importance of items c) and d), although they must in some sense scale with the other two.

I estimate the beam-gas background by assuming the vacuum quality within the detector is no worse than the average vacuum in the ring as a whole. Then the rate in the detector can be estimated from the beam-gas lifetime of the machine, taken to be 300 hours. With 1.3×10^{14} protons in each ring and counting any interaction of either beam within 400m of the collision point as dangerous, the rate is

$$2 \times 800\text{m} \times (1.3 \times 10^{14})/(83\text{km} \times 10^{6}\text{sec}) = 2.5\text{Mhz} \ .$$

This problem is therefore serious, but does not look deadly. Careful study is in order.

The second background source, the emission of junk from calorimeter faces and apertures, may be the worst problem; it certainly attracts much commentary from experienced experimentalists with whom I interact. As we discuss further below, the scaling law for mean energy deposition onto a calorimeter face a distance z downstream is, per inelastic interaction,

$$\frac{dE}{dA} = (100 - 200\ MeV)\ \frac{z}{R^3}$$

where R is the distance of the element of area from the beam axis. Most of the energy is dumped onto the inner edge, and the amount increases linearly with distance. There are two kinds of background. One class consists of hard energetic particles which are peaked forward in angle. These I ignore on the grounds that just from energy conservation they are relatively few in number. The soft, isotropic neutrons, gammas, electrons, etc. are on the other hand most abundant.

I am not competent to estimate the magnitude of these. However the problem seems to be easier the further downstream one goes. Assume again the scale invariant geometry (Fig. 5.1). Then, for example, the tracking element nearest the 80m wall will be 4 times further away than its corresponding tracking element at the 20m wall. While the 80m wall emits of order 4 times as much isotropic junk than the 20m wall, the solid-angle factor is 16 times smaller than at 20m, leading to a problem that scales inversely with increasing distance downstream.

Therefore one should expect the front end to have the biggest problem. But for that there are already studies, as well as working experience. For example (Fig. 5.9), at Fermilab the direct photon experiment E706[38] runs at 0.6 Mhz interaction rate with a magnet at 2m with aperture in the vertical of 20 cm, silicon

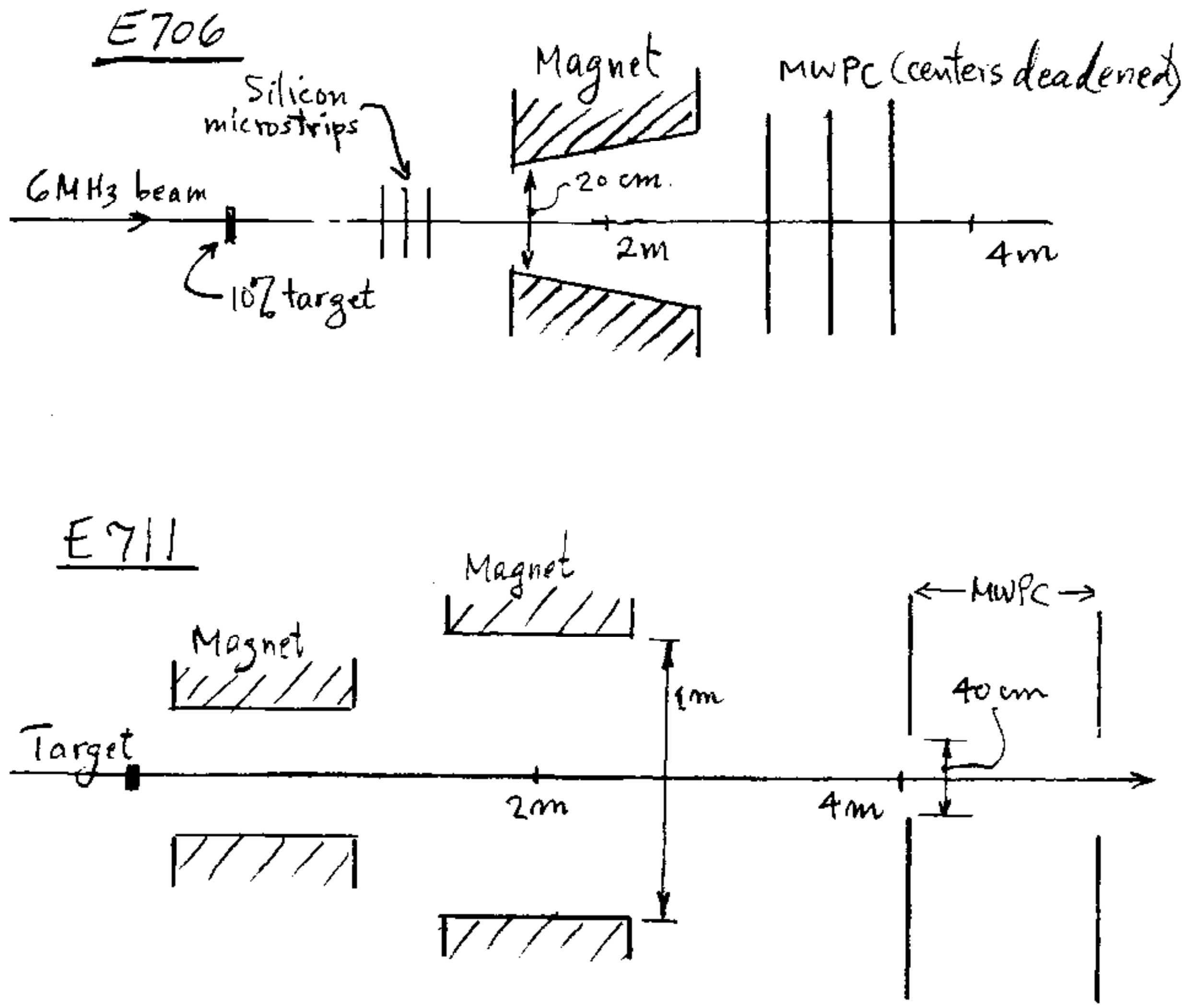


Figure 5.9. Layouts of two high-rate Fermilab experiments: E706 and E711.

microstrips just upstream (which survive) and MWPC's just downstream. (They will be replaced by straw tubes; I thank George Ginther for informative discussions). Another experiment,[39] E711 (dihadron production), successfully ran at 2-5 Mhz with a two magnet system 1-2m downstream of the target, with MWPC's about 4m downstream (central wires deadened). The rapidity-distribution at SSC is about twice as large as at Fermilab, so that these examples indicate that backgrounds at SSC will not be an easy problem in the front part of the detector, but that the state of the art is not too far from what is required.

We should note that the background downstream of a calorimeter wall may be easier than these cases because of the small aperture and high density of the wall (ideally 1.5m of tungsten), which makes it look like a collimator. However the small aperture makes the upstream neutron albedo problem potentially worse. We may compare that problem with what is faced by the SDC. Their 5m calorimeter wall has a 90 cm diameter aperture, while we chose 20 cm. The total energy deposition per collision onto that wall scales inversely (more or less[40]) with the diameter. Thus with 100 times the luminosity, the SDC problem is 20 times worse than what is faced by this spectrometer.

Finally, we consider the question of radiation damage. The biggest dose of ionizing particles to detector elements occurs at shower maximum in electromagnetic calorimeters. To get a feel for the problem we estimate the dose in the endwall calorimeter and then use scaling arguments. We assume a Feynman-scaling flux of pions

$$x \frac{dN}{dx\ dp_t^2} = 1.5(1-x)^4 f(p_t)$$

with

$$f(p_t) = 12.5 \exp -5p_t \ .$$

This assumption is pessimistic. If the cosmic-ray data mentioned in subsection 3.6 are right the yield is lower. But assuming this and putting the endwall at 1.3 km gives a mean energy deposition at zero degrees of 30 GeV/cm^2 per interaction. (Recall that 10^{13} interactions/SSC year is assumed.)

Calculating the deposition at large angles from the above formula gives the rule of thumb already quoted, where one should use 100 MeV at the forward angles and then increase it by the amount the rapidity plateau rises as one goes to larger angles.

The energy deposition on the endwall is shown in Fig. 5.10. There is a severe problem for radii less than 30 cm or so. A 320m calorimeter wall likewise has problems at the inner edge. The 140m calorimeter wall, described in the next subsection, with its small, 10 cm diameter aperture, likewise has an equivalent problem. Walls at closer distances probably are all right. And in any case the problems are localized to such small areas that there are probably a variety of

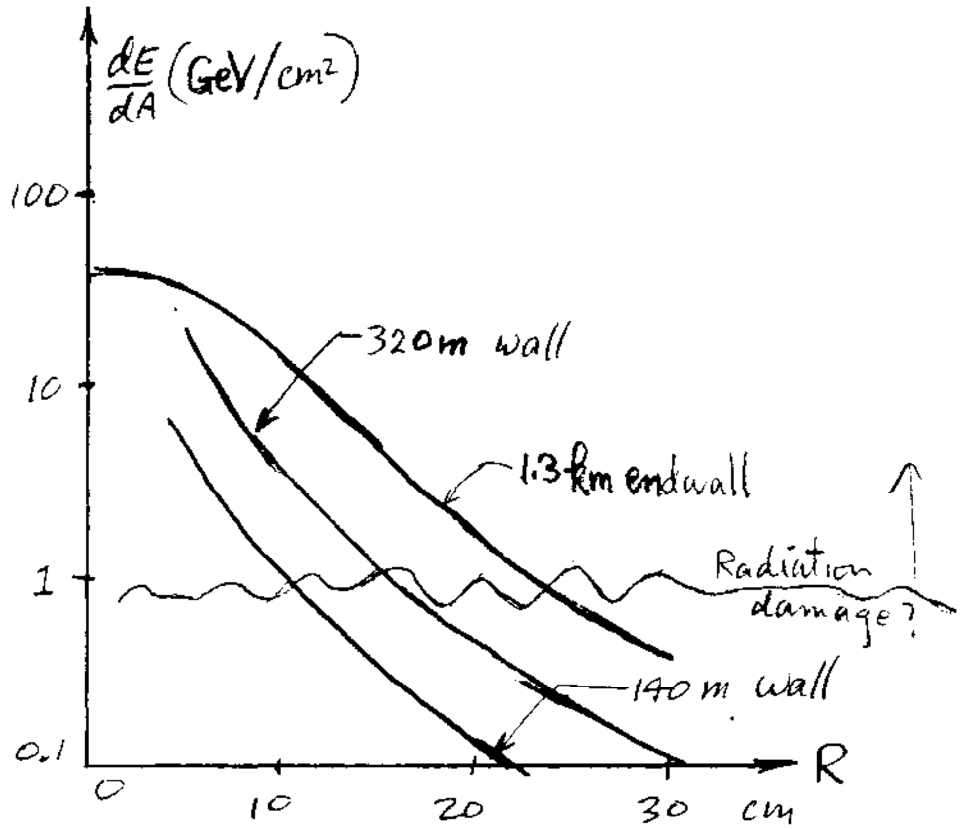


Figure 5.10. Rough estimate of energy deposition per event on a calorimeter endwall located 1.3 km from the collision point. Also shown is the deposition on a 320m wall and a 140m wall.

solutions that can be found. They also occur in areas of phase space with quite low occupancy, so that a preradiator which locates shower cores followed by a radiation-hard coarse-grained detector (liquid scintillator?) should suffice. There is also the problem of neutron damage, about which I know not what to say.

6. Interaction of the detector with the SSC machine lattice and the physical environment

As we have already mentioned, the radical-right/left sectors of the spectrometer not only dominate the real-estate, but also are intimately related to the SSC machine lattice. There are quite a few critical issues to handle. In this section we describe one cartoon of what such a sector might look like.[41] But I have no confidence that this is very near to what more considered thought and wisdom of experienced designers would give as an optimal solution. However it may serve to highlight some of the problems and exhibit some of the critical parameter choices.

We start our considerations at the 80m calorimeter wall, which we leave alone, with an aperture of 20 cm diameter. However, downstream of this we use a pair of strong dipoles (of opposing polarities) to sweep as many charged secondaries into the tracking system before they reach the final-focus quadrupoles. One reason this is of value is the beam-pipe problem as discussed above in subsection 4. Another is that the cost and length of the final-focus quadrupole system rapidly increases with aperture. There is therefore a strong bias toward accepting no more secondaries, charged or neutral, into the aperture of the final-focus system than necessary.

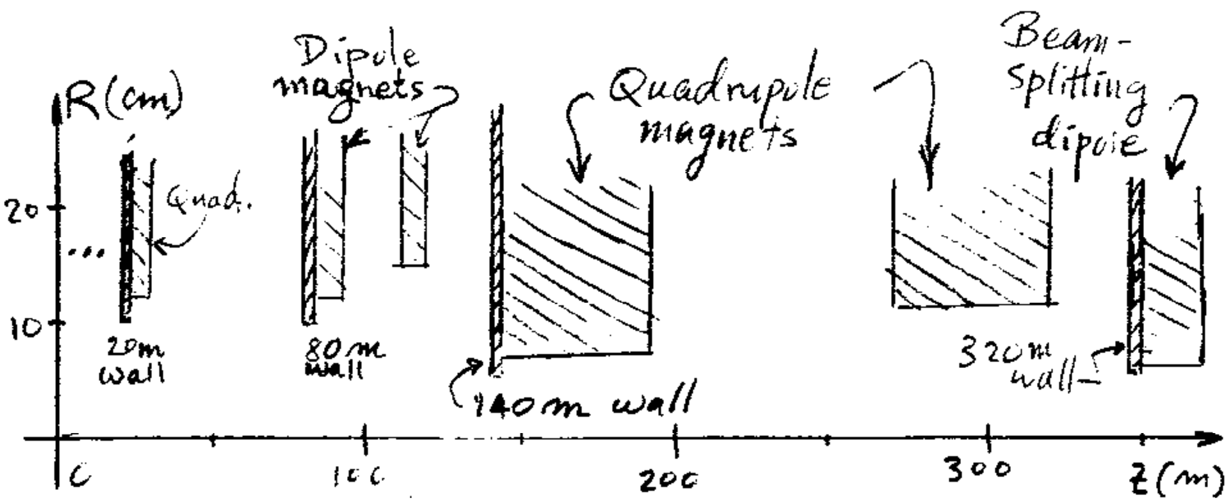


Figure 5.11. A cartoon of the front end of the radical-right sector of the spectrometer.

A sample layout of this sweeping module is shown in Fig. 5.11. Its endwall calorimeter has an aperture of only 10 cm diameter, and is located 140m downstream of the target. Therefore the rapidity coverage of neutrals in this module is actually $\Delta\eta = 1.3$. The radiation damage problems are comparable to what exists on the endwall calorimeter, and there will have to be extra care taken with respect to spatial resolution of the showers.

The p_t kicks of the magnets (2.4 GeV and −1.6 GeV) were chosen to bring the two circulating beams onto the axis of the quadrupole string, given a standard value of the crossing angle of 75 microradians. It turns out to be a quite reasonable choice, since to good approximation the only secondaries which enter the quadrupole apertures have momenta in excess of 2-4 TeV.

Immediately downstream of the 140m wall begins the quadrupole string. We choose a gradient of 0.15 T/cm. The first string is 50m long, and after an 80m drift space comes a second string, again 50m long. This by chance puts us at the 320m endwall. Behind that goes a strong dipole to split the circulating 20 TeV beams and to initiate their transport into the SSC lattice.

The first 50m of quadrupoles suffice to sweep away all the secondary charged pions of 2-10 TeV into tracking elements, and the second 50m system plus dipole sweeper should allow the secondary diffraction-dissociation protons to be measured somewhere nearby without too much trouble.

The effect of these quadrupoles on the circulating 20 TeV protons is to refocus them 400m or so downstream of the target. Here there is a machine-lattice exercise to perform, one I have not attempted, to get an acceptable matching of this insertion into the regular lattice. In the CDG designs, which is all that I have looked at, the final focus is a quadrupole triplet which effects essentially point-to-parallel focussing, with the remaining matching quadrupoles far downstream, of order 1 km away.[42] Therefore I have reasonable confidence, perhaps foolishly so, that the amount of large-aperture quadrupole focussing needed for a match is not badly estimated here.

In examining the properties of the existing low-beta and intermediate beta designs, I estimate that a beta function for the matched (as yet nonexistent) insertion will look something like Fig. 5.12. The main presumption is that $\beta_{\max}$ in the quadrupole system will not be large compared to 8 km, as is the case in existing low-β designs, and that the beta just upstream of the quadrupoles will again be the generic value of 1 km. If this is the case then the ratios of luminosities will

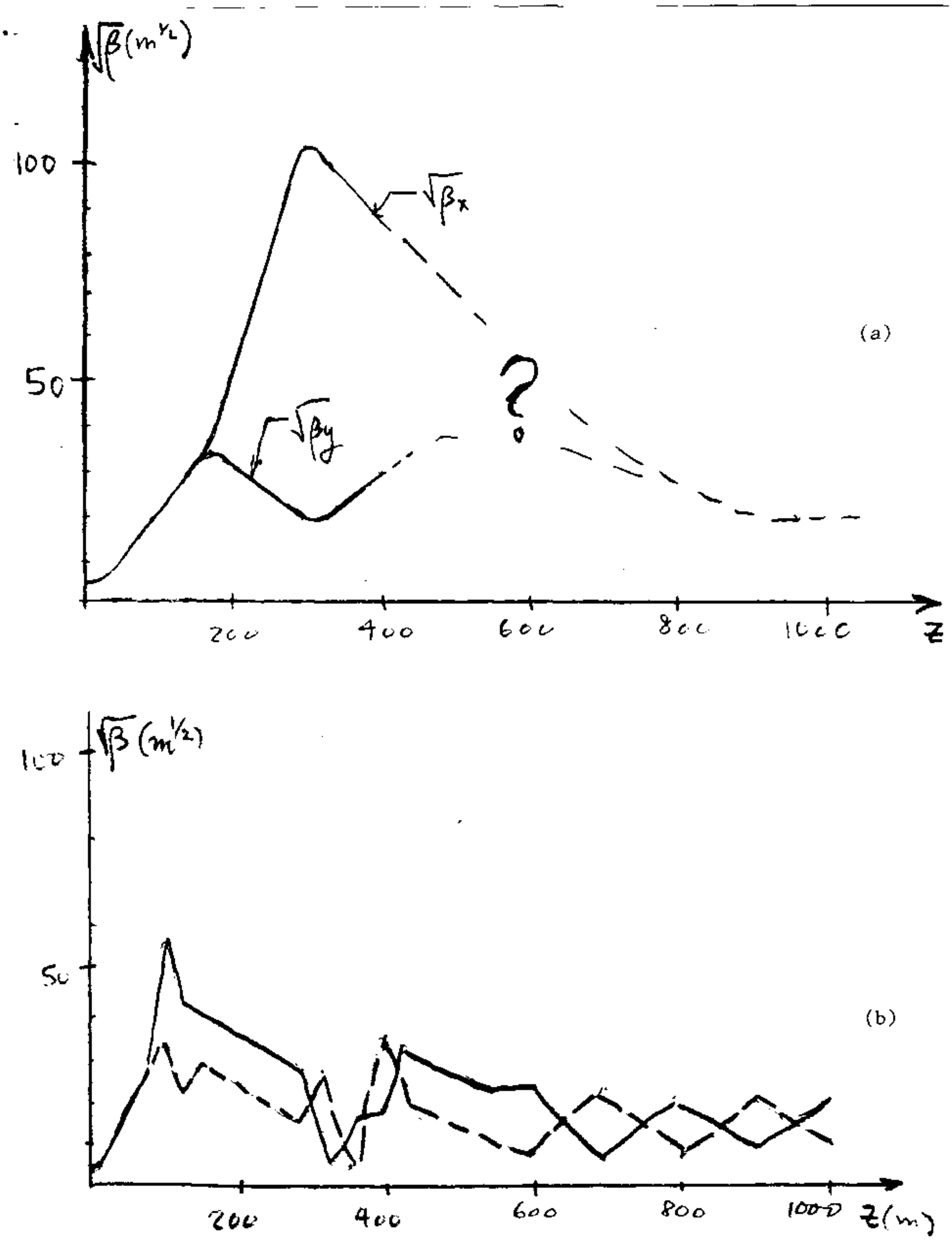


Figure 5.12. An estimate of the β-function for a matched (as yet nonexistent) design of the final focus quadrupole system (a), compared with the standard intermediate-luminosity design (b). A β^* of 2.5×10^{31} cm^2 was assumed, and $\beta(z)$ was then calculated from $z = 0$ outward.

just go as the square of the amount of free space, in comparison to the 20m of the low-β design. This would leave for this spectrometer about half the luminosity of the "standard" intermediate-luminosity collision region, or $2.5 \times 10^{31}\text{cm}^{-2}\text{sec}^{-1}$.

Relative to the standard CDG intermediate-luminosity lattice design, the circulating beams stay together longer before being separated (cf. Fig. 5.12). Here the separation between beams should be made large as far upstream as possible in order to leave a central free space for those photons en route to the endwall calorimeter. This would be best done by bringing the beams directly out to their final separation of 80–90 cm from the $z = 350$m splitting dipole, unlike the existing collision-region lattice designs.

In the collision-hall conceptual design available to me[43] there is no provision for such radical right/left spectrometer arms which extend beyond 100m or so. It is essential that extra space be provided. I estimate the diameter of the quadrupoles to be 50-70 cm, so that this part of the spectrometer, as laid out here, is again quite compact. It actually physically fits in the standard SSC tunnel, but only by inches. However, extra transverse space *must* be provided, as well as ways for the data to be transported to counting-rooms, etc. and necessary services brought in. And there might be more demands for transverse space were the design to mature, especially given my perhaps irrational bias toward compact transverse dimensions. Also, there may be a need for shielding walls around the spectrometer, implying that personnel and equipment bypasses around the spectrometer area need to be provided.

VI. Is All This Practical?

1. Modularity and its social implications

The practicality of this idea rests in part on the modularity of the spectrometer. Each portion of the detector in real space sees only a portion of the event in rapidity space. This in turn leads to the notion that the detector may be regarded as an assemblage of quasi-independent detectors, and that the experimental group itself might also be similarly regarded as an assemblage of quasi-independent subgroups. This allows a staged approach to the building up of the detector and therefore the possible practicality of the idea in the face of seemingly impossible budget confrontations with high-priority generic detectors. But before getting into the questions of costs and staging, it may be worthwhile to mention some other aspects of this feature of modularity. All this is more than a little romantic in nature. But one of the reasons I revived this idea was the belief that this device admits a much less rigid social and managerial structure than the generic detectors. And I think that, if this feature is really true, it is very important for the physics.

For example, it gets harder and harder for large collaborations to engage in risk-taking. This can occur already at the design level: does one dare to invest in a risky technology when the lead times are of order a decade, when the costs are enormous, and when the consequence of failure is the loss of many years of productivity? In highly integrated detectors such as generic central detectors, the answer is pretty clear. However in this essentially one-dimensional spectrometer it is more thinkable to take such risks, because the lead-times and turnaround times should be much shorter. And the innovative design ideas need not be applied to the detector as a whole, but only to a module or so at a time, so that any losses which are incurred will only affect a limited portion of the total phase space observed.

Another level of conservatism is in the choice of physics to emphasize. We see an enormous focus of effort in the direction of the Higgs search, for good reason to be sure. But the example of the dark Higgs sector in Section IV.6 shows how even that could backfire—although the general goals of doing an optimal job on physics at the highest p_t scales can hardly be faulted. But this spectrometer more easily allows innovative small pieces of physics to be initiated by a relatively small subgroup of the full experiment without disturbance of the remainder. Even physics ideas which require some modification or augmentation of the detector capability might be able to be implemented in a timely way.

Therefore I think that modularity, flexibility in the approach to the physics, and rapid adaptability to change are part of the design criteria to be applied to the spectrometer. This should occur in the managerial structure, and continue in the delegation of responsibility of subgroups to the detector; they should be as local in rapidity as possible. For example there is the temptation, because of the approximate boost-invariance, to build all the calorimeter walls with a common design, most likely with the same people doing them all. Much better to me is to allow different technologies to be applied to the various walls, with local responsibility for the choices made (but also local responsibility for failures-followed of course by global responsibility for improvements). Indeed there can be internal (friendly) design competition between left-arm and right-arm modules of the same rapidity.

Modularity as a design criterion also evidently applies to the architecture of the detector as well as to the society of physicists using it. The modularity was caused in the first place by the multiple calorimeter walls creating the individual "rooms" in which subgroups "live". But in the charged particle tracking system, modularity could mean that track coordinates be able to be processed and the particle four-momenta determined locally (more or less). Just as a possible example, this might preclude use of upstream dipole magnets because of the difficulties created for modules far downstream (cf. subsection 5.2, Fig. 5.4, for what I mean here).

And presumably the data acquisition system can be made to enhance modularity, with lots of local processing power-including local permanent storage of data, and local analysis pathways for local physics goals.

What are the candidate "modular groups" and how big are they? We mentioned in the beginning that the detector naturally divides itself into the central barrel region, the left and right wings (5m to 100m downstream), and the radical-right and radical-left sectors containing the final focus quadrupoles at their front ends. This could easily go to six were it decided to slice the central barrel at $\eta = 0$, as mentioned in subsection 5.2. In each sector there is a further natural subdivision in terms of those who are dependent on an upstream (or downstream) neighbor, and those who are not. Here it is less at the physicist level and more at the apparatus level that there is likely to be a strong distinction. But if these subdivisions were put in, the number of "modules" so defined could be as large as 16. So in a practical sense the modularity is somewhere between 5 and 16, not so different than the estimate just based on the number of "rooms" created by the calorimeter walls.

Hereafter we simply revert to the calorimeter-wall definition for the estimation of size and cost of a module. In the next subsection we will make a rough generic guess of the cost of a module. That rough guess is compatible with a group size no larger than a typical Fermilab single-stage open geometry experiment, say 20-40 physicists.

A given "modular group" could actually find physics to do all by itself in its 1.4 units or so of η: inclusive distributions of almost anything. But once it collaborates with nearest neighbors there is an enormous body of measurements which opens up, because after all the rapidity coverage of almost all detectors built doesn't exceed that by very much. And many generalizations to larger rapidity intervals are available. And while these dreamy words exaggerate and oversimplify what would really happen, I still hold to the bottom line which underlies the whole discussion: the advantages of modularity are so large, from so many points of view, that it should be respected as an important design criterion.

2. Costs

This section is intended only to provide the roughest guidance on what the cost of this spectrometer might be, to identify any singular big-cost items, and to get some feeling for the overall distribution of costs. No independent costing has been attempted,and the unit costs have usually been guessed by normalizing to numbers in other proposals or EoI's. The costs will be estimated per module, with no contingency applied until the very end, where it will be taken to be 30%.

A) Magnets:

The magnets behind the generic calorimeter walls were taken to have a rather small aperture (20 cm diameter) and a field integral of perhaps 5 T-m. I guess no more than $1M (conservative?), a number small enough that it doesn't control the overall cost much at all.

The central barrel magnet is more costly. We take a radius of 60 cm, a length of 2.5m, and a field of 3T. Cost: no more than $5M?

The 100m string of final-focus quadrupoles in the radical right/left modules dominate the magnet costs there. The magnet outer diameter is of order 60 cm and a rough guess of the cost, made by comparing the tonnage of iron and superconductor (we assume the quadrupoles are superferric) to the SDC magnet, gives a cost of roughly $10M.

B) Electromagnetic calorimetry:

Each calorimeter weighs only 15 tons, and we assume the cost is dominated by readout. Taking $\delta\eta = \delta\phi = 0.03$, and counting each pixel as a channel (conservative!) we get 30K pixels/wall. Taking $250/pixel gives $7.5M/wall—and a very good calorimeter.

I checked this number against other quotations by normalizing to the 1.4 units of rapidity in these modules. The results were \$6M (SDC), \$20M (Berkeley '87), and \$3M (SFT).

C) Silicon microvertex system:

The SDC measures the cost of their system by area, quoting \$1M/m^2 including electronics. Their system has 10^7 channels and 32 m^2 of Si, implying a unit cost of \$3/channel. I happily buy that! Covering everything out to a radius of 10 cm with of order 16 double sided planes/module gives an area of a mere 0.5 m^2/module, and an estimated number of channels of 500K/module. There are more channels/m^2 here than at SDC, so I guess the cost/module as somewhere between \$2M and \$5M.

D) Outer tracking system:

This is the system which covers the 10-40 cm region. Candidate technologies include more silicon, a la SDC, who go all the way to 60 cm, or a gaseous silicon microstrip detector,[44] or scintillating fibers, or conventional MWPC's, or straw tubes. I guess 40K readout channels per module, with the cost dominated by the readout electronics. For that I take a cost per channel of \$125, giving \$5M/module as the bottom line. This seems large to me—but I'm engaging in wild guesswork here.

Again I compared the cost per 1.4 units of rapidity quoted by others. The results are \$4M (Berkeley '87), and \$1.5M (SFT). SDC comes out at \$18M, but that is not a realistic comparison.

E) Miscellaneous costs:

I am aware that these can sometimes bite, but am helpless here. Items that do come to mind include the exotic beam pipe, the vacuum system for the beam, Roman pots for all those silicon systems if they are inside the beam pipe, a big 1m diameter vacuum tank and ancillary complications if the thin beam pipe needs protection or if interactions of secondaries with air are troublesome, Cerenkov counters, TRD's, other bells and whistles appended to the apparatus, the central computer, general overhead assigned to the experiment, etc. ... Just to put down a number, I'll guess \$5M/module.

Summing up these numbers gives a cost of \$23M/module, which I suspect is conservative. I assign an extra $2M to each half of the central barrel, and an extra $10M for the quadrupole string in the 80-320m module, but assign only $5M to the endwall module. Adding all this up gives $132M, to which is appended $40M for contingency. Doubling this for the second arm gives the bottom line of about $350M.

It is no surprise that this is a very big number. However, we must remember that this detector is very stageable. We now turn to a possible scenario.

3. Staging scenarios

We now address the question of practicality. It is in my opinion near impossible that this detector could prevail in a direct first-round competition with the second generic detector or even a dedicated B-physics detector. An alternative strategy would have the only SSC commitment to the proposal be provision of the collision hall and services, provision of the final-focus optical system, and support for instrumenting the radical-right spectrometer arm. I do not know the cost of the collision-hall extensions, but the other costs listed above are of order $30M plus contingency. The scenario is as follows:

Stage 0:

A collaboration is formed which is big enough (20-40 physicists) to build and operate the radical-right portion of the spectrometer, and strong enough to intelligently design the appropriate growth potential into the overall detector architecture. It is this group that submits the proposal to the SSC and, of course, wins approval.

Stage I:

The following parts of the detector should be ready at the time of the commissioning of the first circulating beam of the SSC (We assume that one ring will be completed as soon as possible, with a delay of some time before the second one is finished.):

1. Good tracking (not necessarily silicon microvertex; just good tracking) and good electromagnetic calorimetry beyond the 80m calorimeter wall of the radical-right spectrometer arm.
2. The magnets behind the sundry calorimeter walls (e.g. 1m, 5m, 20m, and 80m.).

Only this part of the detector cost is to be borne by the SSC directly. The remaining items below are to be supplied by interested outside parties who bring in their own resources. The idea is that "for rent" signs are put out on the other modules of the spectrometer, and the collaboration, in conjunction with the SSC Laboratory, entertains any and all outside proposals to help instrument them. Not much in the way of resources is necessary to fulfill the remaining items on this list:

3. A gas-jet target
4. Simple tracking beyond the 1m wall (and none penetrating the beam pipe).
5. Coarse-grained calorimetry on the downstream walls (beyond the 1m wall), for example, $\Delta\eta = \Delta\phi$ of 0.3.

No central barrel detector is suggested at this point, although there may be a lot of salvage solenoids by then. And probably one will not want an exotic beam pipe either, making a splendid initial challenge for the experiment. With this much apparatus there is the opportunity to do some physics. We assume the SSC Laboratory is not so interested in running the first ring for physics. But even with the detector in a completely parasitic mode, it can learn very much about backgrounds, radiation damage, effects of the beam pipe, detector performance, etc. Also important is the commissioning of the data acquisition system and learning how to efficiently reject background at the Level I decision point. Hopefully by the time the machine has been run in, the experiment would be ready to request a small block of time to log up to 10^9 minimum bias events (At 10 kHz this is only a 24 hour run, so the request is dominated by setup time.). Some physics accessible is listed below:

Stage I physics program:

1. General survey of particle production at 200 GeV in the cms, especially in the far forward direction.
2. Study of unusual event structures.
3. Incisive studies of diffractive processes.
4. A-dependence studies?
5. High-p_t Pomeron physics.
6. Searches for unusual very-low-p_t phenomena.

7. Search for evidence of photon-exchange processes with presence of a rapidity gap.
8. If RHIC is running by then, responses to observations made in that program.

Stage II

Stage II begins with commissioning of the SSC collider. In the interval between the Stage I run and Stage II, graduate students write theses, postdocs write papers, and hopefully more support flows in from the outside to further instrument the detector. Again not too much in the way of incremental funds is needed to add the radical-left arm and the simple tracking and coarse calorimetry in the rest of that arm commensurate with the sophistication (or the lack thereof) attained in the right arm. And of course there will be modifications, retrofitting, and probably just plain new ideas emergent from the Stage I operating experience.

It would be nice to have the barrel region instrumented to the same level of sophistication: magnet, conventional tracking, coarse-grained electromagnetic calorimetry, and muon coverage. This is a little more pricey. But the reservoir of people experienced in these techniques is very large. And since there will also be a natural (perhaps irrational) interest in "capturing the center", there might well be some bidders with enough independent resources to do it.

In general, I would guess that there will in fact be no shortage of bidders proposing to fill up all rapidities with detection elements. These may even include groups associated with SSC generic detectors who would like to put some of their R and D developments (especially ones not chosen by their collaboration) in an SSC environment early on. It may be that the real problem will not be in finding physicists and equipment, but in quality control. The modules should not be instrumented with junk provided by inferior experimental groups, which once in place becomes difficult to get out. It will not be easy to keep the long-range goal of a well-integrated, quality instrument in the forefront during this stage of the evolution.

The Stage II physics program might begin with another short gas-jet run with beams not in collision, to exercise both the left and right arms before the main experimental program begins. But the physics menu during the first real collider run, assuming no more resources available than the "minimal" scenario described above, includes the following:

Stage II Physics:

With only a few days of running in minimum-bias mode, a lot of exploratory physics already becomes accessible:

1. General survey of minimum-bias particle-production phenomena.
2. Study of unusual event structures.
3. Search for the anomalies reported by cosmic ray experiments.
4. Comprehensive study of low-p_t Pomeron physics.
5. Study of quark-quark collisions using leading baryon tags.
6. Study of events containing both jets and rapidity gaps.
7. QCD minijet studies, and the extension to an initial study of multijet production.
8. Study of events with very high multiplicity.
9. Study of very-low-p_t phenomena.

In the first dedicated run (say, one SSC year at 10% of design luminosity), the menu would expand considerably:

10. Small-x physics, in particular study of forward dilepton production.
11. High-p_t QCD, with and without rapidity gaps in the final state.
12. Observation of W and Z production.
13. First look at processes involving electroweak boson exchanges (with rapidity gaps), with an eye toward Higgs searching in future runs.
14. Intermediate mass scale physics of contemporaneous interest.

It is no use to try to anticipate what would happen beyond Stage II. Indeed it is arguable that it is foolish even to anticipate this much of a scenario. The main point is that there is a guaranteed scientific payoff to this program even were it to go no further than what is sketched out above. And the growth potential and flexibility in how the growth actually would take place is enormous.

What is the scenario from now until Stage I? Some of this will be discussed in the next, concluding section. But from the point of view of this experimental program, I do not see the need for long lead times-at least relative to the programs for the generic detectors. There are some advantages in short lead times as well. These include waiting for as many new Tevatron and LEP results (or others for that matter) and/or new theoretical developments to arrive before commitment of too much of the all-too-small resources that will be available. There is also the advantage of new technologies, especially in data acquisition, appearing. Others may come out of the SSC R&D program itself. So deferral of the major commitment to the detector may even be good science. On the other hand I should think that the full detector should be on line within five years of SSC commissioning.

The sense of urgency that I do have has to do with provision of the collision hall and services. If the SSC Laboratory deems this approach to be good physics, I believe that it should include an appropriate collision hall in its initial construction package, even if there is *no* response by the community to this idea. It will be very hard to turn things around if the experimentalists do knock on the door a few years hence.

VII. Request to the SSC and Concluding Comments

1. Brief review of the detector and its physics menu

If the reader behaves in a way anything like I do, he or she has first arrived at this section after reading at most the introductory section. Whether or not this is the case, welcome!

And whether arrival to this point has been via the long route or the shortcut, it may not be so easy to put together a concrete, concise picture of the detector from what is in the text. This stems in part from my own feeling that were there serious effort put into this general idea, the output would not likely look very much like what I lay out all by myself. Nevertheless, in Figs. 7.1-7.3 are put down cartoons of what one or two iterations of my own thinking produce. The detector naturally divides into the center, the left wing and the right wing and the radical-left and radical-right wings. In Fig. 7.1a, the central and right-wing spectrometers are laid out with length and width drawn to the same scale. This is not practical for the radical-right wing, drawn in Fig. 7.1b with transverse scale expanded by a factor five. Even so one sees that the essential nature of the spectrometer is its one-dimensionality. It is just a long broad-band beam transport system, which removes low momenta up front, removes higher momenta further downstream, and passes only the highest momenta to the 400m-1 km endwall module. In this respect it resembles the cochlea of the human ear, which receives a broad-band

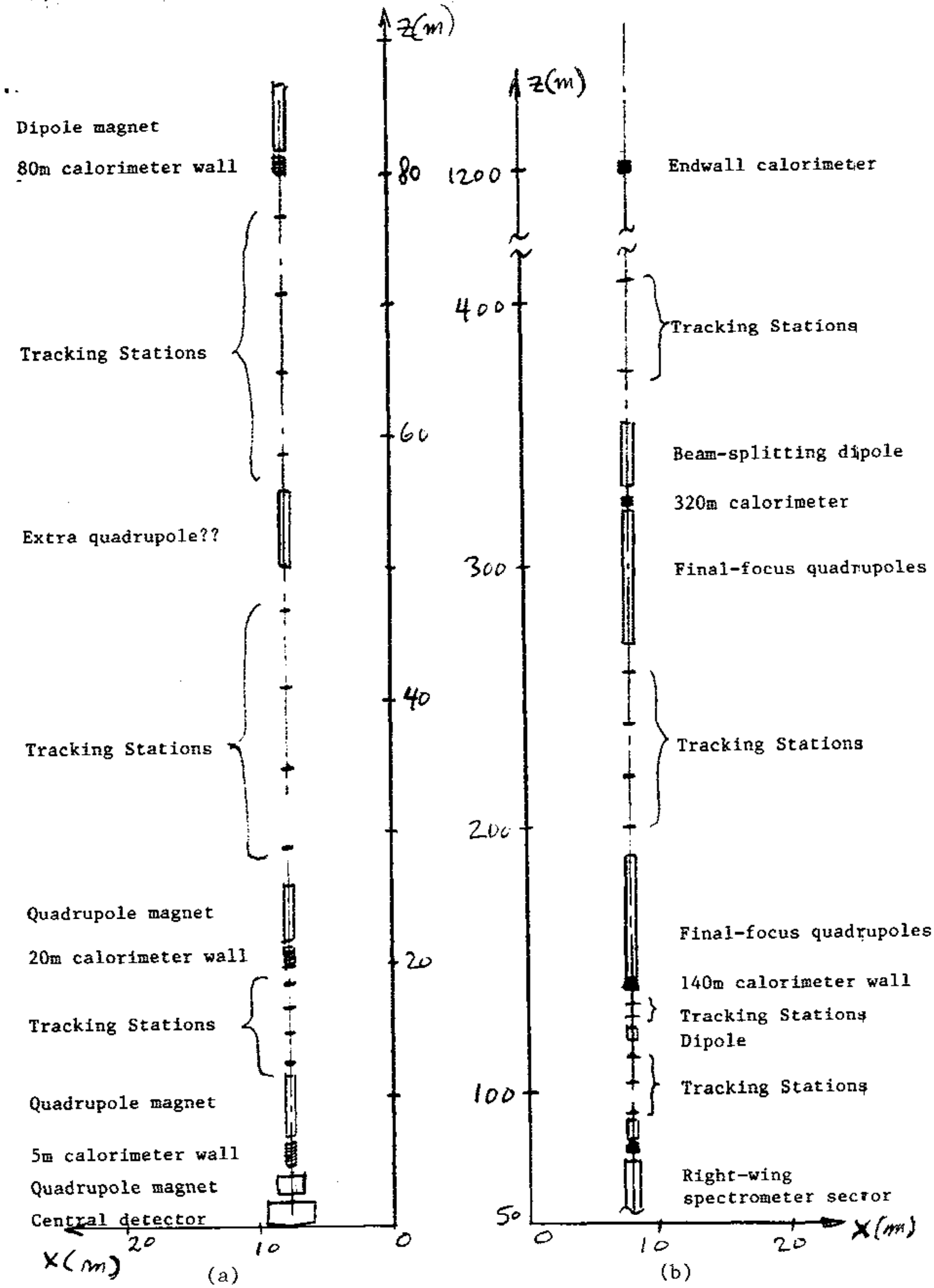


Figure 7.1. Layout of the current iteration of the spectrometer architecture: (a) center, and right wing, and (b) radical-right wing.

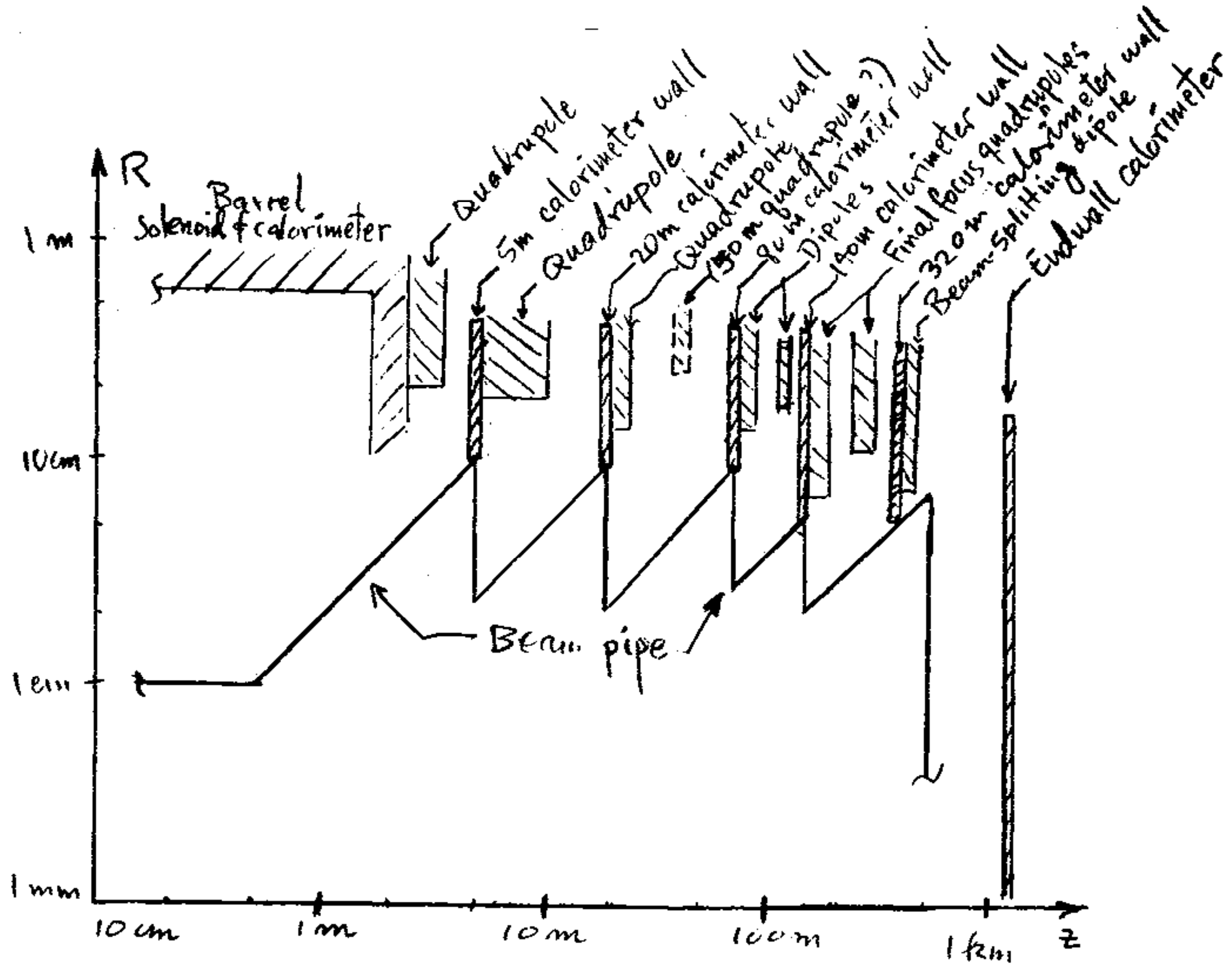


Figure 7.2 The detector architecture in log-log coordinates. In these coordinates, tracking elements should fill the region reasonably uniformly.

frequency spectrum of sound and filters out successively higher frequencies as the sound proceeds further into the ear.

In some sense the natural unit in such a detector is not meters or GeV, but db, and so the detector is redrawn to log-log scale in Fig. 7.2. In these coordinates the density of the charged-particle tracking elements (not explicitly shown) should be reasonably uniform.

Because the final-focus quadrupoles have moved back to 140m, the luminosity suffers. I estimate it as (1–3) $\times 10^{31}\mathrm{cm}^{-2}\mathrm{sec}^{-1}$. The estimates of physics yields assumed an integrated luminosity of $10^{38}\mathrm{cm}^{-2}$/SSC year.

I reiterate, that as far as I am concerned, the three irreducible criteria for this detector are (1) full acceptance in phase space (lego variables), (2) accurate measurement of the four-momenta of charged particles and π^0's over the entire phase space, and (3) no compromise on the capability to do the physics of "rapidity-gaps" in this phase space (i.e. diffractive processes and electroweak-boson-exchanges). A rough estimate of how well this is accomplished is given in Fig. 7.3.

The physics menu addressed by such a detector is vast, and some is documented in the text. Here I again reiterate what I see as the most important feature of the spectrometer. First and foremost it is a survey instrument which can respond well to unexpected changes of physics emphasis, and which on its own has strong discovery potential. It is not optimized for "engineered" discoveries such as the W/Z discovery, the search for a standard top quark, or the search for the orthodox Higgs boson. It should be at its best for the serendipitous discovery of phenomena not anticipated in advance, the kind of discovery where the data itself speaks to the experimentalist, not the theorist. And since there has been no real survey instrument of this kind for hadron collisions since the era of the bubble chamber, there is a very broad range of discovery potential possible, both big deal and little deal.

I was especially impressed by this feature while preparing this document. Just thinking about the physics menu in the light of the possibility of making this class of measurements has provided me with new insights into both strong and electroweak physics. Even if this initiative goes absolutely nowhere I have several fresh lines of theoretical research to pursue. Since just the thought of such a detector can be such a stimulant, I am convinced more than ever that the existence of the real

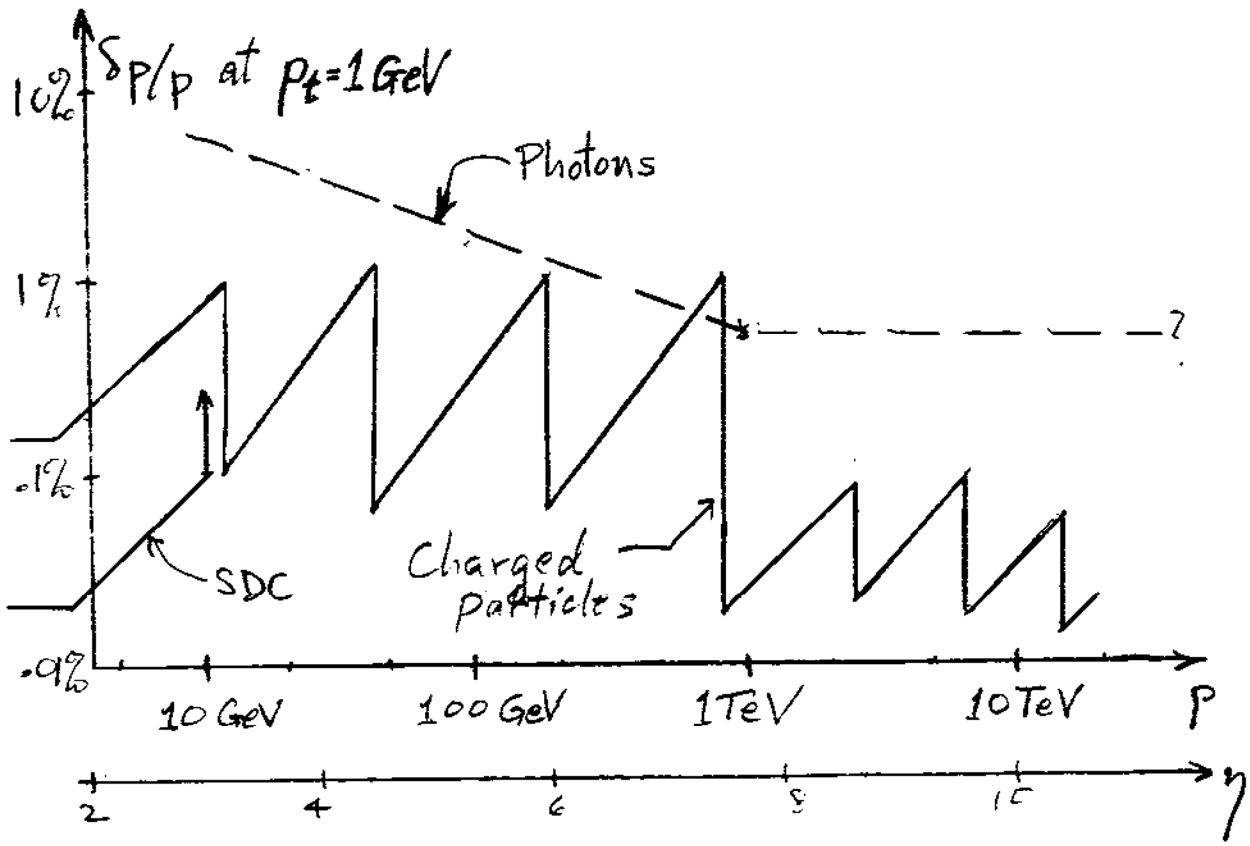


Figure 7.3. A rough guess of the resolution versus momentum (and η) for charged particles and π^0's of $p_t = 1$ GeV. The scaling law at fixed η for charged particles is $\delta p_t/p_t \propto p_t$, while for π^0's it is $\delta p_t/p_t \propto p_t^{-1/2}$.

thing would be incomparably better.

One of the most unexpected consequences was the implications for the Higgs sector. When I began this study I did not consider the possibility that this detector would have anything to say about that problem, because it was optimized for low mass scales, not high. After investigating the consequences of soft diffraction phenomena, I turned to diffractive final states containing jets. This led naturally to the realization that there were similar but even more striking event morphologies in processes such as photon or W-exchange. This in turn led to looking at the two-photon processes and in particular the process $W + W \rightarrow$ Higgs and other W-W interactions, as suggested by Khoze. In examining the signatures for that, it became interesting to question whether virtual W's of low mass, interacting to produce an invisible final state, might be an observable process via reconstruction of its missing mass. That thinking led in turn to the example of a "dark Higgs sector", a model which has two Higgs particles to discover, not one, but where, for a large range of parameters, both decay overwhelmingly into invisible final states. And while the example is unlikely, it bears a not inconsequential resemblance to what actually happens in the strong-interaction prototype of the Higgs problem. And were this scenario to occur, this detector might offer the *only* possibility of observation of the Higgs particle at the SSC.

I emphasize that I do not advocate this specific example as the reason to go ahead with the detector. It is an unlikely scenario, I have not accurately estimated cross sections and efficiencies, and I do not know the potency (background rejection power) of the event signature. The importance of the example rests mainly in illuminating how a simple twist of the theoretical situation can lead to a completely novel phenomenology. Any single example of how it can happen can be dismissed as far-fetched. But the overall odds of this kind of thing happening can be—and probably is—large.

2. Request to the SSC

If this idea is to go anywhere, experimentalists must come forward and be willing to do real work. But most everyone is quite busy, and the decision even to work for a while on this thing is a serious career decision, not to be taken lightly. Encouragement by the SSC Laboratory could be a very important factor in getting the ball rolling. Therefore I make the following quite specific requests to the Laboratory.

1. I request the Laboratory to review this document as soon as possible, internally and/or in conjunction with its advisory structures, and render an opinion regarding the physics value of this program, its technical credibility, and the technical feasibility of mounting it as a first-generation experiment.
2. If the response to the first question is encouraging, I request that the collision hall in an appropriate intermediate luminosity interaction region be extended sufficiently far to allow mounting the experiment (Work evidently needs to be done to pin down the enclosure dimensions.). This request is meant to be unconditional, in the sense that I believe it wise to do this even in the

complete absence of response by the experimental community before the civil construction deadlines occur. A revival of this initiative might occur later on, and the opportunity would have been lost. Furthermore this is the clearest signal the Laboratory can make that it is supportive of the idea.

3. Under the assumption that the next step does occur, it will clearly be important for proponents to maintain a close liaison with the Laboratory on design issues, most particularly interaction with the machine lattice, specifications with respect to the beam pipe, vacuum system, etc., and specifications regarding the collision hall and provision of services. Therefore it will be essential that the Laboratory express its willingness to contribute to the resolution of these design problems, even at the early stages of proposal preparation.

3. Request to experimentalists

Here the request is simple. I need help.

I think the list of questions which need work is pretty obvious, and I won't enumerate them here. But I estimate that the next step needs a critical mass of at least a half dozen persons putting in a number of weeks full-time to get things going. I especially invite anyone interested in participating at this level to contact me (I'm BJORKEN at SLACVM). I consider a reasonable deadline to be late this summer. If nothing happens by then I shall, with one exception,[#1] simply give up. If something does happen, I would not consider formally organizing the work until that time. And for those who cannot find the time, I still would greatly value any reactions and comments regarding this work.

As I mentioned in the beginning, I am not interested in becoming an experimentalist. But obviously I do strongly believe in the idea, and am willing to stay aboard until an appropriate organizational structure exists to carry the initiative forward, and in "godfather" mode thereafter for as long as it is appropriate.

#1 The exception would be to continue the effort to persuade the SSC to provide the collision hall: item 2, subsection 7.2. The deadlines are about a year away.

VIII. Acknowledgments

I have received much useful advice, criticism, and encouragement from many colleagues. I especially wish to acknowledge the help of Bob Cahn, Francis Halzen, Bernard Margolis, A. Lincoln Read, Enrico Predazzi, David Hitlin, W. Ralph Nelson, David Ritson, Steve Shapiro and Wayne Vernon.

I also thank Roy Schwitters and the SSC Laboratory, in particular Alex Chao, Don Edwards, and Ray Stefanski, for their warm hospitality in my first visit there, April 1–2 this year. I have every expectation that requests 1 and 3, subsection 7.2, will be acted upon, and am most encouraged by that. But Request 2 will be more difficult and there community response will be most important.

This work was initiated in large part and presented in preliminary form at the Sixth J.A. Swieca Summer School: Particles and Fields, Campos do Jordao, Brazil, January, 1991. I wish to thank the organization, in particular O. Eboli and A. Santoro, for making that meeting a most pleasant and fruitful one in all respects.

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