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What Lies Ahead?*†

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1. Introduction

I begin this talk with a self-introduction. I am a person often asked to give talks with titles like the above. In checking out the record, I found the rate over the last decade is somewhere around two a year. I frankly have gotten a little tired of giving them, and even more tired of having to listen to others give them. You are forgiven if you feel the same way about this one. But this talk may be a little different, just as the SSC experience is a little different from the experiences in physics that have preceded it.

Another part of my history I want to mention has to do with SSC. I am one of the rare birds in U.S. high energy physics that did not support the SSC initiative back in 1983–1984. And here I am, not only giving a closing talk at an SSC jamboree, but also involved in a nontrivial way in expressing interest in its experimental program. So I feel the need to explain myself in a few words as to where I was then and where I am now. The history of those years do condition my present viewpoint, which is after all what I was asked to present.

In 1983 I was advocating a site-filling collider at Fermilab as the next step in the U.S. program, to interpolate between Tevatron and the LHC, with an SSC or something else to come later. The idea went nowhere, although in Europe I find to this day some sympathy for it. My concern about the SSC initiative was not that it was scientifically wrong—on the contrary I have always felt that it is scientifically superb—but that such a large next step was a very high risk to take, and that many years would be required to get the SSC off the ground, with a high probability of failure of the whole venture.

I am happy to admit that my political judgments were off the mark. The SSC initiative has consistently progressed more rapidly and successfully than I guessed it would. The public, be it the grass roots, the state of Texas, the Congress, or the administration, has been supportive to a most impressive degree. And I see this support to large extent as originating from the right reasoning—not from considerations of the quark-barrel or spinoffs, but in terms of fundamental values and the major advances in basic science that the SSC should provide.

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So I stand here persuaded that the SSC will happen. There may be delays and obstacles and no shortage of difficulties, but someday those 20 TeV beams will be there colliding with each other. With this conviction comes a recognition of the overwhelming obligation the high energy physics community has in following through in every way possible to help make those collisions occur. And before that happens there are sure to be some pretty tough times.

2. Discoveries

I do not want to dwell on those difficulties, especially since my morbid thoughts regarding SSC in the past were somewhat ill-considered. Rather I prefer to concentrate on the reasons why I have always been highly enthusiastic about the physics of the SSC. They go well beyond the generally cited Big Three of Higgs, supersymmetry, and compositeness. My enthusiasm is based simply on the general, very familiar line of argument for the intangibles; that the increase of a factor 20 in cms energy and 1000 in luminosity from what we now have is an extraordinary advance in sensitivity to new—and old—phenomena. The usual arguments regarding the TeV mass scale are, at the least, strongly suggestive that there will be really new physics available in this new parameter domain. And all the "old" physics, be it bottom quark or W physics or whatever, will be present in much greater abundance at the higher energy. One must keep in mind that thorough study of old physics can be a major avenue to big discoveries. And there are new frontiers at low mass scales as well which cry out for incisive exploration at the SSC. This subject is my preoccupation nowadays and I will return to it later. Putting together all the various opportunities, I believe a well-conceived and well-supported SSC physics program could have the breadth and diversity represented by, say, the entire Fermilab program, fixed target plus collider.

This point is to me central. Hadron-hadron collider physics is really different from electron-positron physics. The SSC program should be qualitatively different from the LEP program. There the event rate is so low that there is only one winning strategy, namely to accept every event and examine it with the best 4π detector one can make. True, the highest-mass scale SSC physics bears similarity to this. But even in that case there is, for the pp collisions, useful supplementary information in the beam directions to be had, possibly demanding novel strategies and unconventional extensions of generic detector design. And there is the ubiquitous problem of event selection to deal with—is the right physics being chosen? Ultimately the answer is that the whole spectrum of SSC physics at all mass scales deserves a thorough examination. This is a big order and may well require a set of detectors with as much variety as one finds in Fermilab/CERN fixed target experiments. For this reason I put the scientific longevity of the SSC as very large, at least 50 years.

Whatever the mass scale, it is the discovery physics, not the programmatic physics, that provides the primary motivation for most experimental physicists to undertake the great hardships involved in mounting experiments. It is interesting that nowadays, with our standard-model conditioning, the very concept of discovery seems increasingly circumscribed. It is the class of discoveries that may be called "engineered" which seems to dominate present thinking. The discovery of the W and Z, as well as the anticipated discoveries of the top quark or standard-model Higgs, fall into this category. One knows enough about the properties of the hypothesized particle or phenomenon that a well defined search strategy can be defined. With the success of the standard model this mode of discovery becomes increasingly the dominant one. We see large tomes written for the Higgs hunters or SUSY hunters, detailing in advance how to make the Great Discovery. But one thing for sure is that almost all the words in the Higgs Hunter's Guide[1] on how to find a nonstandard Higgs are wrong. There are so many mutually incompatible scenarios that generally what is on one page contradicts what is on another. Is it reasonable to average over a large number of wrong strategies in order to increase the odds of finding the right one?

The earlier history of particle physics abounds with examples of another kind of discovery, which can be called serendipitous. The discovery of strange particles or the muon are examples. The observation of the many hadron resonances in the early 1960's is another. CP violation lies on the edge. More recently I think the J and ψ arguably fall into that category. All these discoveries arrived largely without advance billing, with theoretical prophets having little if any significant part of the action.

In those simpler days the justification for the instrumentation which was responsible for those discoveries was much simpler. I am told that the proposal for the BNL AGS was a model of brevity. I know not whether there was even a proposal for the big Berkeley bubble chamber. But in any case I am pretty sure that it would not contain any simulations of the physics expected to occur. And even if it did, it is pretty certain that it would not have had much to do with the important results. Big bureaucratic SLAC did create fat documents. But again what went into the physics projections for the linac program, and later for SPEAR, bore little resemblance to what turned out to be the memorable experiments and discoveries.

Even some facilities have been in a sense serendipitous. The SPEAR storage ring did not emerge out of the national master plan, but in spite of it. The same can be said for the Cornell storage ring CESR. I know; I was on one of those committees that recommended against it.

Is there a future for the serendipitous discovery? I think the answer is affirmative. The problems of the electroweak sector are ripe for that kind of occurrence. There is much talk about the importance of WW scattering, just as there was much talk about $\pi\pi$ scattering in the early 1960's. The key to strong interactions was to be revealed by study of that process. But it was other degrees of freedom-quarks, gluons, strangeness, charm, ...—which were the real teachers of what goes on. The four Higgs degrees of freedom may well again be only indirect, relatively uninteresting of the ones underlying really behind the origin of particle masses.

It takes very little to change the phenomenology a lot. Yukawa predicted not only the existence of the pion but also its decay mode into electron and neutrino. If only the muon were to weigh twice as much as it does, he would have come out ok. Certainly Yukawa is not to blame for the bad prediction. It is this kind of thing that makes me indifferent to SUSY phenomenology. Imagine someone telling you that there are three generations of quarks and leptons and that they interact via standard model couplings, with only the information on the mass matrix not given. I think it would be hard to reconstruct anything like the phenomenology we have. The details are everything.

I stumbled on an interesting, if morbid, example while working on my expression of interest for an SSC experiment.[2] I call it the dark Higgs sector.[3] Take the usual four Higgs fields and replicate them with four more which also undergo spontaneous symmetry breaking with similar self-interactions. However, the new particles have no charge or electroweak couplings whatever; they are "dark". So there will be three massless (or nearly massless, if there is some breaking of the new $O(4)$ symmetry) "dark" Nambu-Goldstone bosons I call k, as well as a new massive Higgs-like boson called S. The new "dark" states can interact with the usual Higgs degrees of freedom. The most important effect is the mixing of the heavy S with the usual Higgs. As a consequence of this, not only will the S decay into two k's, which most likely are as invisible as neutrinos, but so also will the ordinary Higgs. For a large range of parameters, this kk channel can overwhelm the usual Higgs decay channels. And were this the case, the usual SSC discovery modes of the Higgs particle would disappear, although the missing mass techniques used in the electron-positron world would still be available.

Yes, this is a priori an unlikely, contrived scenario. But the S and k were carefully named. WW scattering would reveal not only the usual Higgs resonance but also the S, just as $\pi\pi$ scattering reveals not only the σ but also the $S^*(975)$ resonance, coupled strongly to those "unexpected" Nambu-Goldstone modes, the K's. And the presence of an extra $O(4)$ symmetry for the dark sector, which commutes with all known symmetries, is maybe made more credible when one reflects on the fact that the key to the strong interactions as seen in the 1960's lay in a symmetry (color) which commutes with all symmetries known at that time. So the dark Higgs sector is no less incredible than what actually happened for the strong interactions. That does not increase the odds for this specific model, only for the notion that simple twists of the present thinking can change phenomenology and search strategies in a big way.

It is not only at the highest mass scales that the discoveries can occur. The J and ψ were for their time discoveries well below the energy frontier, as was the τ lepton. The b quark discovery was ideal for the Cornell electron-positron machine, much less so for its higher energy cousins at PEP and PETRA (although they made essential contributions to b-physics as well.) For the future, axions and SUSY particles are only two obvious classes of particles which might show themselves somewhere other than at the highest energy scale.

And the new low energy phenomenon need not be a particle discovery. A theorist friend of mine, Vadim Kuzmin, provided an extraordinary example of one to me.[4] Ever since the 1967 Sakharov paper on the cosmological baryon asymmetry, Kuzmin has explored various aspects of baryon-antibaryon mixings and oscillations. According to him, there could still be a baryon-number violating coupling originating in particles of electroweak masses and coupling strengths, provided all three generations are simultaneously involved. Therefore he exhibits special interest in the Ξ_b (bus) baryon, which according to him might undergo a lot of mixing with its antiparticle. (See Fig. 1 for a little motivation for this craziness.) Yes this is wild, but think how it would change the wonderful world of b-physics were it to occur. Electron-positron b-factories running at the $\Upsilon(4S)$ would be out of it, as well as (probably) Z-factories. A high-powered dedicated b-experiment at a hadron collider would (probably) offer the best chance.

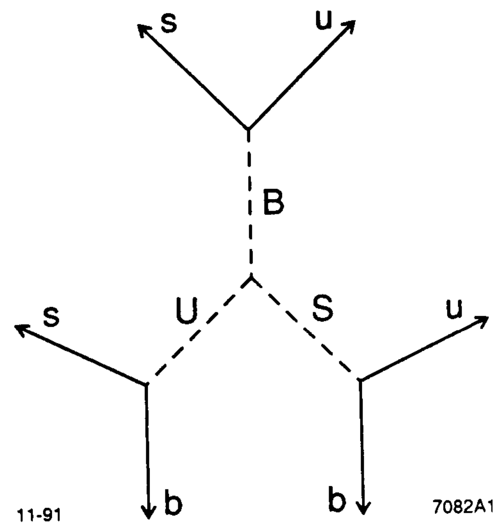

Figure 1. Kuzmin's Krazy Koupling. The dashed lines are color-triplet scalar fields; the solid lines are right-handed quark fields.

Of course there is also the present search for discovery physics in high-precision tests of the standard model. No new particles or striking new phenomena need be seen, only the disagreement of an accurate prediction for a number with an accurate measurement. These methods have been and are now truly beautiful to behold. But ultimately there is no substitute for seeing more directly the reason for a disparity—or for that matter an agreement—if that is what it comes down to.

Finally, there is the possibility of discoveries which do not go beyond the standard model but could be fundamental and interesting in their own right. I came across a possible example when working on the expression-of-interest, and am now exploring the issues together with Marvin Weinstein at SLAC. The measurement is as follows: Select events for which the multiplicity of produced particles is as large as possible, but where there is no significant jet activity. Now plot the distribution in transverse momentum. At low transverse momentum, say 100 MeV and below, Weinstein and I predict a pion excess. So of course it will be there. Cut on that excess; it should exhibit Centauro and anti-Centauro behavior. That is, there should be large fluctuations in the charged-to-neutral ratio of the anomalous component. There should even be some events where all the anomalous excess is charged and there is no neutral component, and other events where all the anomalous excess is neutral and none is charged.

The idea was motivated by cosmic ray data,[5] although this is not the place to elaborate how that happened. And there are some accelerator experiments which point to phenomena something like this,[6] although I would not cite the evidence as anything except nonnegative "support". A good experiment is clearly not easy. Neither low-p_t charged particles nor neutral pions are easily seen in a generic central barrel detector, although in principle it can be done. It is not easy to dismiss the idea (or support it) on the basis of existing evidence.

What is the physics? The picture is that in a high energy collision with high associated multiplicity the spherical debris-shell emerging from the collision point is relatively thin, perhaps a fermi or so thick, and propagates outward at the speed of light. Eventually this shell turns into the dilute gas of hadrons expanding outwards and into the detector elements. But before the critical distance and/or time of hadronization ("decoupling", "freeze-out") one has an inner volume that has relaxed back to something approximating the vacuum, separated by the debris-shell from the outer vacuum. But the QCD vacuum is approximately degenerate because of the approximate chiral symmetry of the up and down light-quark system. So the chiral orientation of the vacuum, usually in the σ direction (we use the language of the linear σ-model), need not be the same in the volume interior to the debris shell, provided the surface-density of energy in the shell is sufficiently high. The actual orientation will be influenced by details of the specific collision at early times. Suppose the interior-vacuum orientation is deflected in the π^0 direction. Then, when at late times (of order the time of hadronization), the interior vacuum sees the exterior vacuum again, it will relax back to the "correct" vacuum by coherent emission of only neutral pions. Let f be the fraction of anomalous low-p_t pions which for a given event are neutral. Then, assuming that the direction in which the vacuum is disoriented is a random variable, the expected distribution of f should be inverse square-root, very different from what a statistical picture would give.

While this experiment does not require an SSC, it would be best done there. The reason is that the effect is anticipated to be more prominent the higher the associated multiplicity. The higher that is, the larger will be the hadronization radius R. The usual multiplicity of hadrons should scale as the area of the debris shell at decoupling, R^2, while the anomalous, coherent piece scales as the volume within the debris shell, R^3. Also, the quasi-macroscopic semiclassical arguments used to bolster the theoretical picture are better justified the larger the space-time distance scales involved. In addition, it may be experimentally advantageous to choose a large mean rapidity for observing the low-p_t particles, because they then have a larger laboratory momentum.

This picture of disoriented chiral condensate is quite speculative. But I think it credible enough to merit experimental consideration. Were one able to see such a thing, one might learn useful things about the nature of the QCD chiral vacuum, a subject which is not at all in good shape theoretically at present. And this in turn might lead to useful spinoffs for technicolor theories. Without asking for it this subject therefore could be relevant to electroweak physics beyond the standard model.

3. Some General Concerns

The above examples do not qualify as candidates for serendipitous discoveries, because as soon as they are written down, an engineered search can be defined. On the other hand, had they not been written down, and if by some fluke one or another of them actually turned out to be true, they would qualify as candidates. But it is not so clear to me how well a generic physics program such as one finds in the big design books would do in making the actual discovery.

There is to me a big challenge of a new type facing this and future generations of experiments and machines. One cannot easily defend an expensive machine or detector on the basis of the physics of intangibles and serendipity. There must be an underlying warranty of productivity, not just for programmatic physics, but in fact for programmed searches for "engineered" discoveries. Otherwise the expenditures will not be deemed cost-effective. This in turn influences the design of machines and detectors, quite possibly in ways which inhibit their potential for stumbling on the unexpected.

Even more serious in my mind is the change in attitude this can engender. I see less rugged individualism amongst the newer generations of experimental physicists. This may be a false perception, because it is hard to see into the inner workings of big collaborations and how tolerant they are to those who would venture into unconventional directions. But there are certainly institutional inhibitions that have to be overcome. For example, high-risk innovative instrumentation is hard to incorporate into the large monolithic collider detectors, with their half- to full-decade lead-times and turnaround times. The possibility of failure is unacceptable. I have seen remarkable conservatism in the running of experiments, where not even a small fraction of time at the end of a run was allocated to lower priority triggers or speculative searches. Even in analyses of data there are pressures toward conservatism. In the early Rochester conferences one finds data presented of appalling quality by present standards, but which helped provide leads for further experimentation. Nowadays it is dangerous for a collaboration to exhibit some wild crazy phenomenon even at the 3 and 4 σ level, because so much hoopla is created by the theoretical community, not to mention the press, that when the effect eventually goes away it can reflect negatively on the reputation of the collaboration.

4. EoI-19: A Full Acceptance Detector

The feelings expressed in the previous section became motivation this year to revive some old work on full acceptance detectors. I decided that the best way to try to attract interest in the approach was to submit an expression-of-interest to the SSC, detailing what is on my mind. It is not appropriate in this talk to go into detail on this enterprise; that was not my charge. But it is difficult for me to ignore it here, since that EoI is a specific expression of what I have tried to say in this talk in more general terms. So in this section I report briefly what the detector is, what has happened to the initiative at the SSC Laboratory, and what the general response of the community has been.

The full-acceptance detector (FAD) is intended to supplement the generic program of high-mass scale physics, by concentrating on lower mass scales, especially the physics in the forward-backward direction. Full acceptance here means good coverage and momentum analysis, for charged particles and photons, over the entire lego plot of $\pm$ 12 units of rapidity or so. A detector which does this is going to look roughly like two 20 TeV fixed-target spectrometers face-to-face, with the circulating beams going through the middle of them, and the final-focus quadrupole magnets integrated into the detector, serving as analyzing magnets for the fast forward charged-particle secondaries. Simple and general considerations then lead to a detector of order one plus one kilometers long, with a width of order meters. A cartoon of a possible detector architecture is exhibited in Fig. 2 and 3 taken from the EoI submitted to the SSC.

The detector is essentially one dimensional, looking something like a beam line. As such, it exhibits a high degree of modularity. Different portions of the detector are labeled by the pseudorapidities to which they are mainly sensitive. I find this feature singularly attractive. It means that the big experiment naturally is subdivided into a set of subexperiments which each enjoy a significant level of autonomy. This happens all the way from the technical level, e.g. the architecture of the data acquisition system, to possibly the social organization of the experiment itself. It means that for many purposes the interdependence of the sundry detection elements is lessened, and therefore there can be more risk-taking in the technologies used in individual detection elements, and faster turnaround times to replace or upgrade those portions of the detector that don't initially work like they are supposed to. If these utopian ideas on modularity have any truth to them, I think that they could be very powerful in allowing the experiment to adapt and respond to changing trends of what is important to measure, with relatively short lead-times.

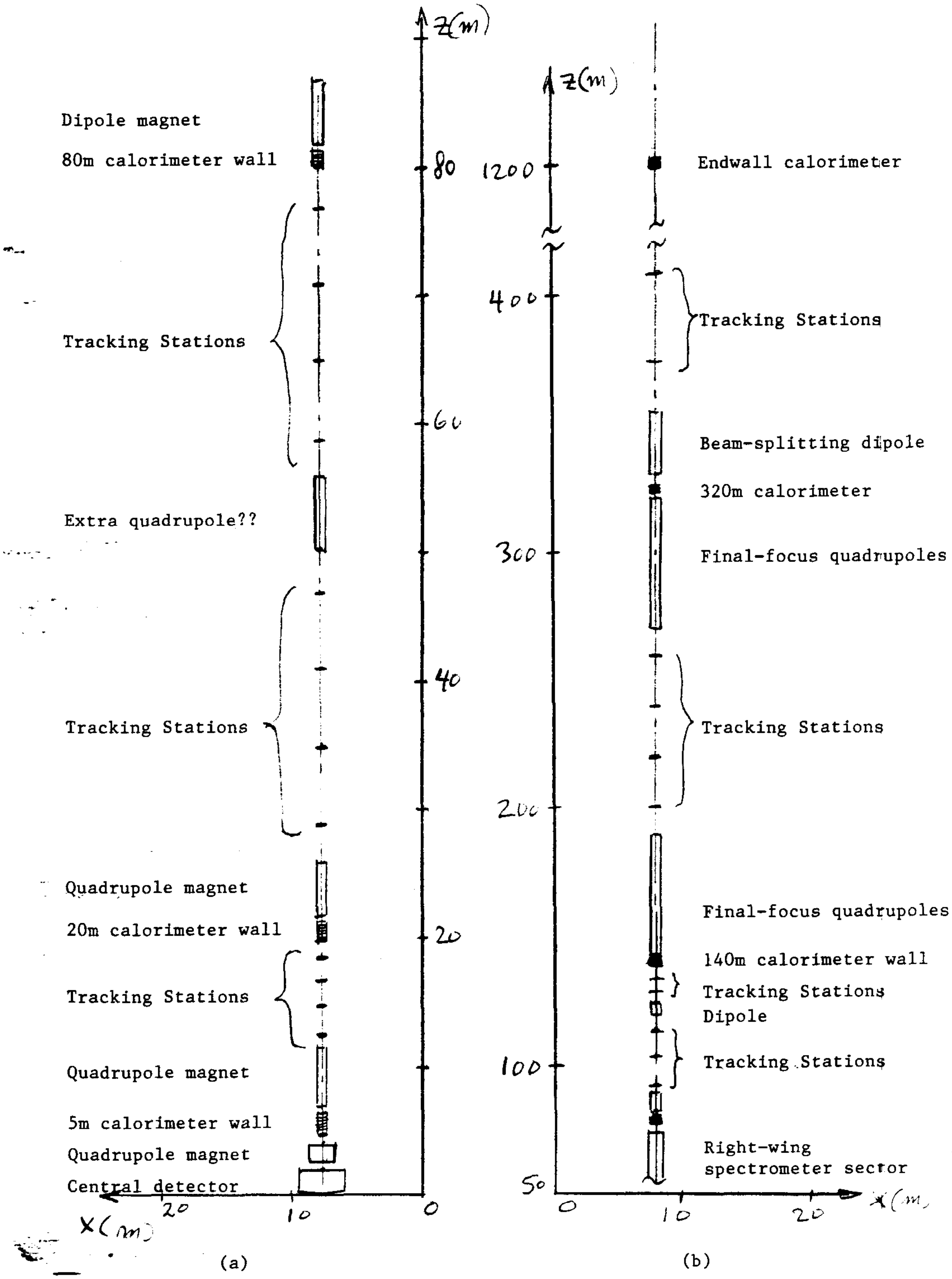

Figure 2. A cartoon of a possible FAD. Only one half is exhibited.

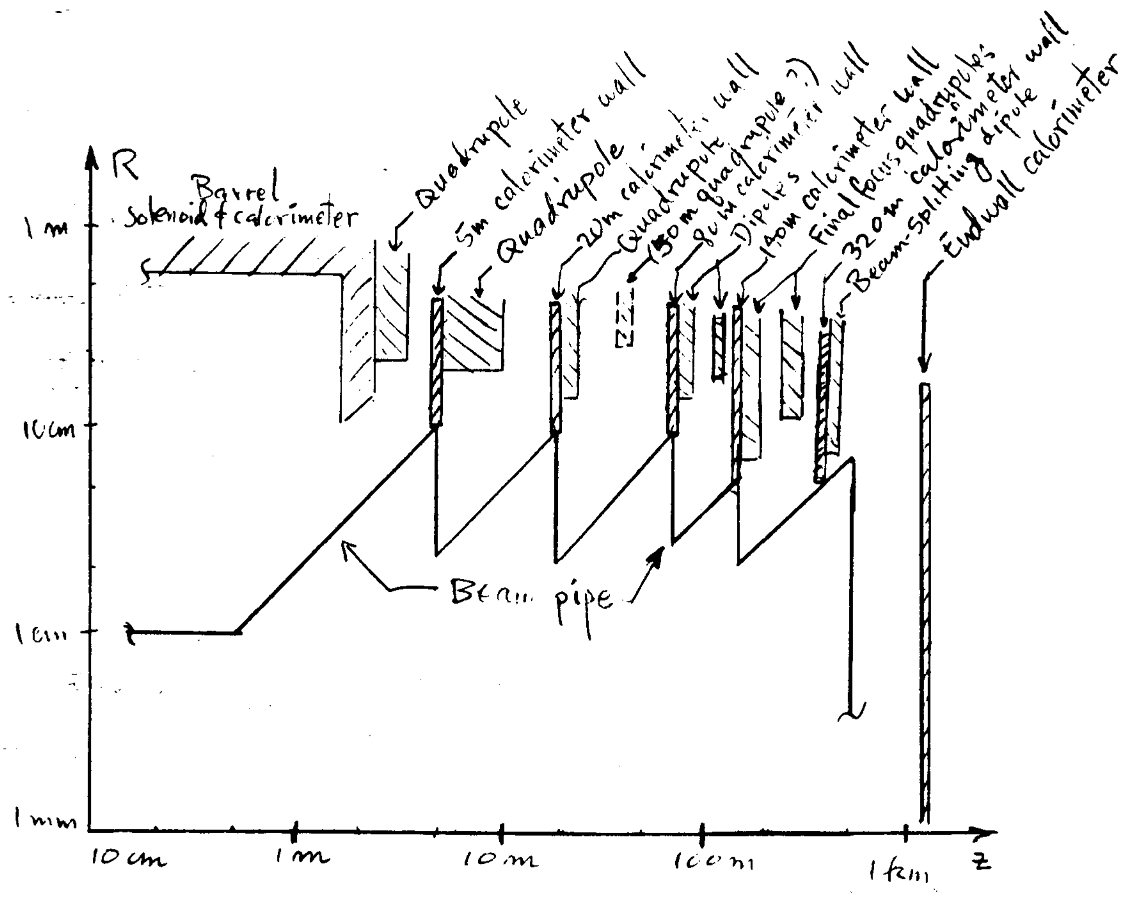

Figure 3. The same as Fig. 2 in log-log coordinates.

This modularity also is important with regard to the practicality of the idea. The complete FAD is obviously expensive, and as a lower-priority initiative it is clear that the whole thing will not be supported out of the SSC budget. However, because of the modularity it can be staged. One natural scenario is to start with the outer rapidities (greater than about 7). This part of the detector, whose front end is of order 100m away from the collision point, is not especially expensive, of order the cost of a single-stage FNAL/SPS fixed target experiment. It does define the architecture of the final-focus system and therefore the nature of the machine-lattice in the collision region. It also defines the nature of the collision-hall extensions in the beam directions as well.

I believe that if the outer rapidities are instrumented (and it is important to instrument both directions in order to do a good job on diffractive processes) then various institutions, perhaps predominantly non-U.S., should find it attractive to bring in enough resources and manpower to instrument at some level the remainder. In my mind the problem is not whether that will happen, but whether quality control can be maintained, as well as compatibility with satisfactory operation of the outer detectors.

The physics associated with such an initial stage program will be dominated by (but not limited to) the lowest mass scales, e.g. diffraction and minimum-bias phenomenology. There are hard technical problems having to do with backgrounds from beam pipe interactions and other interactions originating from edges of upstream detection elements or other materials. But the biggest problem I see is not these but rather whether this kind of initial physics is worth doing. To me the answer is evident; to most others it seems not to be.

What is happening? This spring I wrote the EoI and submitted it to the SSC Laboratory. It contained three requests to the SSC, all of which have been acted upon. First of all, the lab promptly reviewed it. Secondly, the lab has been most cooperative in helping out on the issues having to do with the interface of detector and machine design, namely lattice and collision hall. The most difficult request I made was to provide space for the detector even were there no community response to the idea. The rationales for this request are that the construction deadlines occur soon and that the opportunity could be lost, and that by providing the hall the laboratory could exercise leadership in stimulating this kind of initiative. Happily, the PAC in July has recommended to the laboratory[7] that $\pm$ 500m of tunnel be enlarged in one of the east collision regions. I am most encouraged by this response from the laboratory and its advisory bodies.

What about the response from the rest of the community? I have received warnings from experts that the detector is technically very difficult. I have also received casual encouragement from some of the theoretical and experimental community. But I seek more than that. In order to make this idea into a real initiative (or to give it a proper burial), individuals must come forward and be willing to put in real time in studying the physics issues, design problems, background simulations, etc. This is not an easy step for an experimentalist to take. At present there are of order ten volunteers, precious few. I would like to see a strong leadership for this initiative emerge. I myself do not want to become an experimentalist, but believe in the idea and intend to stay with it while it is getting off the ground. Once the enterprise is in motion, I would like to step to the side, and move into at most a "godfather" mode.

5. Concluding Remarks; What Lies Ahead?

One justification for discussing this EoI here is that I see what I am doing as something of a social experiment. In the past, an off-beat approach not in the main line of thinking could get started easier than now. This EoI represents a test of whether that is still possible within a laboratory as big as the SSC. Now using my experience with my own EoI to make more general inferences is sure to be very dangerous. After all, my perceptions are sure to be biased, since I am much less likely to see the deficiencies of what I am doing than anyone else. Maybe the physics is lousy. Maybe the detector can't be made to work. Maybe it is really politically impossible. Maybe I am not going about selling the idea in the right way. Maybe I am too impatient. But taking all that into account I still have days when I feel very discouraged. It is not that more people have failed to sign on. What is most discouraging is lack of feedback. I sense not opposition to the idea but indifference. There seems not the sensitivity out there to a new approach. Worst of all, there seems to be indifference to the physics. Half the EoI has to do with nonconventional physics topics, expressed in cryptic, telegraphic form. Even after oral presentation, I hardly ever get queries about what I mean by them. The other half of the EoI has some pretty unconventional ways of looking at detector design. These may indeed be lousy ideas, but hardly anyone is willing to offer an opinion why, no less defend it.

As I mentioned in the introduction, I am a person who is often asked to talk about the future. But it seems that this is not meant to include action on what to do about it.

What lies ahead? My answer is no doubt conditioned by reactions or the lack thereof to the EoI. But here they are anyway. We enter a period of extraordinary opportunity. Facilities like the SSC are not going to come our way very often. But the size of the thing makes it very hard to maintain the variety and creativity that has characterized our field in the past. The non-SSC base program is threatened. The scientific opportunities there and the importance of maintaining its health are enormous. And with respect to the SSC, its program appears to be nearly saturated by two not very dissimilar detector initiatives, no more dissimilar than CDF and D0 at Fermilab or Aleph and L3 at LEP. There are strong institutional pressures that push the field in this direction. But I share the worries of some of our critics that we are in danger of losing the essence of what makes science so exciting and interesting, as our approach to the physics becomes more and more homogeneous. I believe that for the base program and the SSC program there is no objective reason why this has to happen. The problem originates in the attitudes of individual physicists themselves. While diversity, and with it serendipitous discovery, is still very much a possible way of life in the future of high energy physics, I am fearful that the opportunity will be forfeited. I wish I could be more optimistic than I am.

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