SLAC-PUB-5927
September 1992
T/E

FAD: A Full-Acceptance Detector for Physics at the SSC*

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Invited talk presented at the VIIth International Symposium on
Very High Energy Cosmic Ray Interactions,
Ann Arbor, Michigan
June 21-27, 1992

* Work supported by the Department of Energy, contract DE-AC03-76SF00515.

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WHAT IS FAD?

For high energy pp collisions, the concepts "4π" and "full acceptance" are distinct. At the SSC, the appropriate variables for describing phase space are the lego variables: pseudorapidity η and azimuthal angle ϕ. While most of 4π is covered by pseudorapidities less than 3 or 4 in magnitude, at the SSC there is very interesting physics out to η's of 9 to 12. For over a year[1] I have been attempting to encourage an initiative at the SSC to provide a detector which could cover the missing acceptance of the two big detectors, which in particular have no appreciable charged particle tracking with good momentum resolution beyond rapidities of 2.5 or so.

The nonnegotiable criteria for an FAD are for me the following:

1. All charged particles are seen and their momenta measured well, provided p_t is not too large.
2. All photons are seen and their momenta are measured well.
3. The physics of rapidity-gaps (*e.g.* inelastic diffraction) is not compromised.

This means angular coverage from 90° down to tens of microradians. The above criteria cannot be met on day one of SSC commissioning with the amount of funds available. But I believe a staged approach is feasible, with a lot of interesting physics available along the way. We shall return to practicalities later in this talk.

The basic philosophy underlying the FAD idea is that it should first and foremost be a survey instrument, sensitive to almost everything, but optimized for almost nothing. Its strength is in the perception of complex patterns in individual events, used as a signature of new and/or interesting physics. Examples of such patterns will be given later.

The main reason behind this philosophy is to provide a detector which has good capability to see the unexpected as well as the programmed, engineered discoveries (such as W, Z, the standard top quark, or the standard Higgs boson). There are many examples in the past, where the general purpose full-acceptance devices using advanced technology made important non-engineered discoveries. Three of my favorites are the Berkeley bubble chamber, which was built because it gave a superior look at the structure of collisions involving multiparticle production, even before there was a clear strategy of what would be learned. It was responsible for the discovery of many hadron resonances, but that was not, I believe, anticipated in advance. Mark I at SPEAR likewise was the first serious prototype of modern generic 4π collider detectors, and its discoveries, *e.g.* quark jets, were again not anticipated in advance. Less spectacular but important to me was the Pisa-Stony Brook full-acceptance detector[2] at the CERN ISR, which mapped out the fundamentals of multiparticle production at high energies, using a simple technology (scintillator) together with full acceptance. Also important for the same reason were the bubble chambers at FNAL and CERN, which for both hadroproduction and neutrino reactions served a similar role. All modern Monte-Carlo programs which underlie so much of modern ideology can be traced back to such experiments.

While the present climate is not as conducive to exploratory ventures as was the case in the past, there are a number of reasons why an FAD now makes good sense. Among them are:

1. Physics at large Feynman x is essentially unexplored at hadron collider energies. It is more than a little arrogant to assert that learning how the valence degrees of freedom break apart in a central collision at extreme energies is not of interest.
2. The physics of very small x, which is also found in the far forward direction for kinematical reasons, is of specific theoretical interest, because perturbative QCD goes out of control.
3. The physics of rapidity gaps (diffraction) has been largely neglected. Large $\log s$ is essential, and the hadron-collider milieu is a great opportunity for advancing our knowledge of this important and difficult set of processes.

There are also practical reasons for an FAD now, having to do with technology. New technologies are really what drive scientific advances. And the most rapidly changing high-energy physics technologies are arguably those connected to the information industry: data acquisition, data processing and storage, and data analysis, including pattern-recognition techniques. In my estimates for the FAD prototype I sketched last year, 80% of the FAD cost was in these technologies.

What has been happening? I began working on this idea in January of 1991. On April 1, I made the first presentation of the idea at the SSC, and by May an expression of interest[1] (EoI-19) was submitted to the SSC Laboratory. This was reviewed by its program advisory committee, which saw enough merit in the concept to encourage the laboratory to consider the provision of the necessary physical space far upstream and downstream of one of the collision points, so as not to preclude any such initiatives. (There was of course no explicit commitment to FAD *per se.*) On December 7 at SLAC, the first meeting of the FAD Working Group was held. Since then there have been two more meetings, one in Dallas and one in Madison. The idea is that the working group is a collection of interested parties and only that, organized to explore the physics case and the technical design challenges. By now our membership exceeds 120. If all goes well, a collaboration should be formed in about a year from now, with a leadership which is committed to actually building the detector, running the experiment, and becoming rich and famous. When the collaboration forms, I would like to step to the side, remaining at most in "godfather" mode at the pleasure of the collaboration.

The critical-path items on our agenda for the coming year are the creation of the detector architecture, background studies, and the detailing of the physics menu addressed by the experiment. While the order of importance is the reverse of that enumerated above, I shall briefly describe in the stated order the status of our thinking:

A. Detector architecture:

To see all the collision products, the detector is essentially two 20 TeV fixed-target spectrometers face-to-face, with a generic central barrel detector linking them. A natural scaling rule is that the length of each arm be proportional to the beam energy. Therefore a rough estimate of the length is given by multiplying the length of a Fermilab 1 TeV fixed target spectrometer by 20. This gives a length of order one kilometer per arm.

There are some differences. In the collider case, the circulating beams must go through the center of the detector, together with an annoying beam-pipe. And the final-focus low-β quadrupole magnets are within the detector, serving as analyzing magnets for the leading particles of laboratory momentum in the multi-TeV range,

This is not the place or time to go into the details of the detector conceptual design. As of April of this year, the situation is something like what is shown in Fig. 1. The free space between the low-beta quads is somewhere around 100 + 100 meters: no machine elements are in that region. The machine elements downstream of that free space will probably look quite similar to what is found in the SSC conceptual design book[3] for an intermediate-β collision region. We expect a luminosity around $10^{32}\text{cm}^{-2}\,\text{sec}^{-1}$.

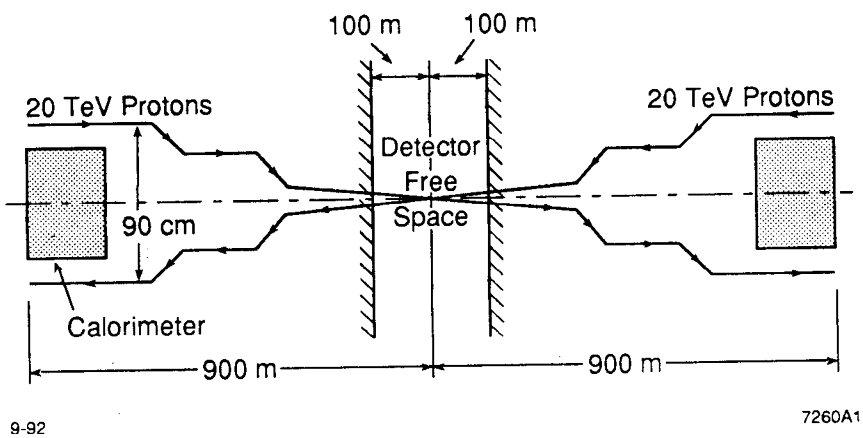

Figure 1. Schematic layout of the FAD collision region.

The portion of the detector downstream of 100 meters must see leading charged particles and leading photons and neutrons. While a length of 500-1000 meters seems dauntingly large, the actual situation is that the detector fiducial volume is in fact very small. Many of the detector considerations (*e.g.* transverse resolution, multiple scattering, distribution in p_t and Feynman x of the leading particles—and perhaps even some of the background problems) are boost invariant. So one can view the process in a reference frame which is Lorentz contracted by a factor 2000 (Fig. 2), corresponding to a 10 GeV proton colliding with a 40 PeV proton, with the 10 GeV products going into the forward spectrometer. Assuming Feynman scaling, it should be easy to visualize what kind of products actually go into the acceptance: obviously very few per event, just by energy conservation. While the transverse space is quite cramped, I think there is probably enough to do the necessary measurements. A working subgroup is now being organized by John Venuti to deal with the parameter choices and detector architecture in this region of the phase space.

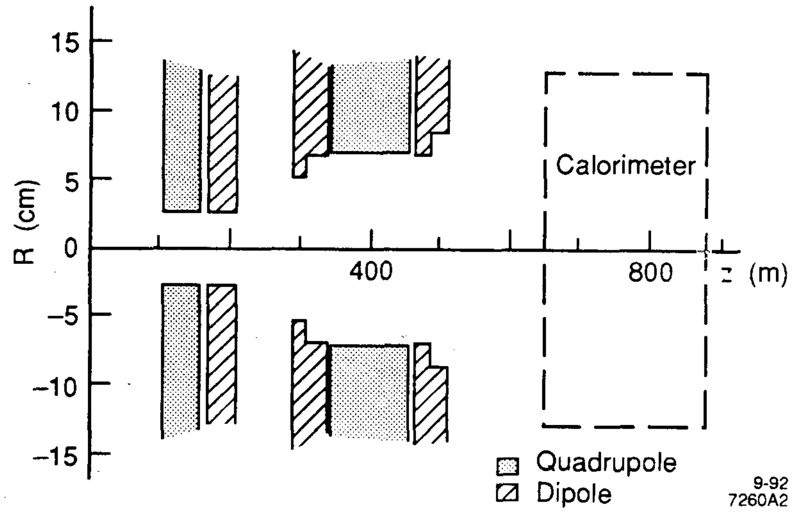

Figure 2. Fiducial volume (Lorentz contracted) of the downstream portion of the FAD detector. Most dimensions are still uncertain to a factor 1.5–2.

The conceptual design(s) of the central portion of the detector (± 100 meters) are just beginning. There should be a variety of these in place by midsummer, in order to provide enough time for the detail work in the following year, as well as to provide a basis for examining the compatibility of an FAD with detectors optimized for B-physics. The issue here is whether the specifications for doing B-physics in the rapidity region of η no larger than 5–6 compromise the downstream physics at larger η. While there has been an initial dialogue of the FAD group with the b-physics community, there must be more homework done, mainly by FAD, before these questions can be meaningfully addressed.

And it must be kept in mind that most of this work has to do with an ultimate, costly FAD, not with an affordable Stage I version. The reason for considering all this now is to ensure orderly growth potential; a Stage I version should not compromise the ultimate device, but be the first step in its implementation.

B. Backgrounds

Even within the ± 100 meter region of the detector, there are likely to be a number of magnetic stages and/or annular calorimeter walls. The inner apertures of these annular walls, as well as the beam pipe, are potential serious sources of background. This possible bad news is mitigated by the fact that beam-halo is likely to be less of a problem than in any other environment, simply because there is no energy being put into the circulating beams and because the beam lifetimes are long (provided only that the machine works as advertised!). Thus energy conservation alone therefore provides a strong argument that halo-induced background will be small. On the other hand, the backgrounds associated with the secondaries from the collisions of interest, as well as from beam-gas interactions, are more serious. On the basis of hand-calculation estimates, even these look manageable. However no one will be persuaded of this–including myself–without a lot of Monte Carlo simulation. That work has just begun[4], and much more needs to be done.

C. Physics

The physics menu of a full acceptance device is by definition vast. In the interest of providing focus, the FAD group has tentatively chosen a short list of "flagship topics". These exercise the design specifications of the detector in unusual directions, as well as provide novel physics ideas in themselves. The short list as of now, still rather tentative, is as follows:

1. Rapidity gaps and jets as a signature of new physics, as well as a study of diffraction in strong interactions.
2. The cosmic ray connection: leading particle physics.
3. Quark-quark interactions at fixed large t and $s \to \infty$; these are, according to perturbative QCD, supposed to get strong!
4. Study of vacuum structure by observation of low-p_t phenomena at high associated multiplicity.

The first item emphasizes the acquisition of patterns of jets and rapidity gaps over a large region of phase space in a single event. The next item requires very high quark-quark cms energy at moderate t (100-1000 GeV^2); namely, observation of jets produced at milliradian angles to the incoming beam. These are found on the 100–meter calorimeter wall in front of the downstream detector. The third item is the physics of the 100 m–1 km region of the detector, and also includes physics seen on the 100 meter wall. And the last item is demanding, from its requirement to see event-by-event low-p_t (50-300 MeV) charged hadrons and photons simultaneously and efficiently in an environment of high associated multiplicity.

None of these items are in the established menu of SSC physics, and all are presently in a state of evolution. I think there is a good chance that they all may turn out to be of considerable importance in the long run.

We now turn to a more detailed discussion of these physics items.

A. Rapidity Gaps and Jets

Diffraction physics may be defined as the study of hadron-hadron collisions containing rapidity gaps. Elastic scattering and single diffraction are well studied, although much remains to be done on double-diffraction and beyond. In addition, there are only the beginnings of experimental studies of processes containing both rapidity-gaps and jets[5].

A very interesting set of processes containing both rapidity-gaps and jets are those induced by electroweak-boson exchange[6]. This includes production of the Higgs boson by W-W fusion. This process has been discussed in some detail[7] elsewhere, and we will not repeat that discussion here. Suffice it to say that the pattern shown in Fig. 3, which I estimate to occur in a few percent of all Higgs-production events, is a very strong signature, arguably background-free. The essential feature of the signature is the *absence* of an underlying event: only the products of the Higgs-decay appear within the acceptance of a generic 4π detector. Even a Stage I FAD "toy" detector, discussed in Section 3, could arguably acquire and isolate this class of events.

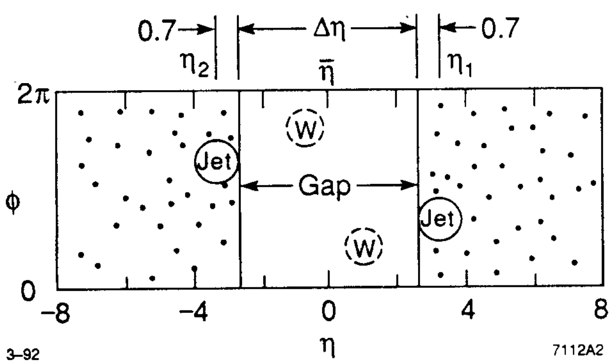

Figure 3. Pattern in the lego plot of the process $qq \to qqH$ via W-W fusion, with survival of the rapidity gap assumed.

There are many QCD issues raised by the consideration of the above strategy for finding the Higgs. Strong-interaction processes involving both rapidity gaps and jets need study. One direction is the study of the "structure-function of the Pomeron," initiated by Ingelman and Schlein[8]. Another is the search for "hard-diffraction;" namely, the existence of rapidity-gaps in final-states of multijet production events is hadron-hadron collisions[9]. I estimate[7] that between 10^{-2} and 10^{-3} of all generic coplanar QCD two-jet events will contain a rapidity gap between the jets (cf. Fig. 4), and that this fraction should not depend strongly upon the width of the gap (at least for large gaps, where the rapidity difference of the final-state jets is large) nor upon the p_t of the jets. This should not be hard to test experimentally, even with the 4π detectors of limited rapidity-acceptance which now exist. The generalizations to multijet and/or multigap processes are also clearly of considerable interest, and would be a very appropriate physics menu for a FAD, even in its Stage I incarnation. Work on this is in progress[10].

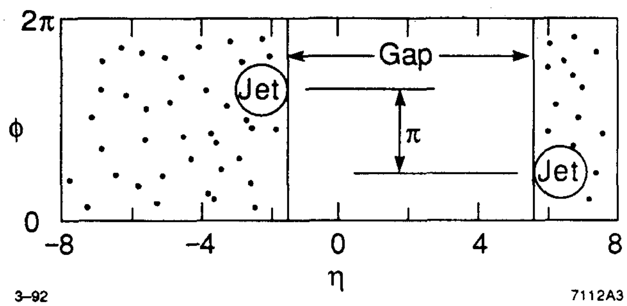

Figure 4. Event structure for hard double diffraction. The process with gap may be[7] 0.1%–1% of events with the same jet structure but without the gap.

B. Quark-Quark interactions at fixed large t and very large s

It is expected from perturbative QCD that, even at short distances, the parton-parton interaction gets *strong* at extremely large cms energies. This large-s problem, usually called the small-x problem[11], occurs because the perturbative single-gluon exchange is enhanced by multi-gluon emission, as described by the BFKL evolution equation[12]. Mueller and Navelet[13] have proposed two experiments which test this idea and which would explore the way in which the rapid growth with energy of this enhancement (roughly $s^{0.4}$) saturates. Both involve measuring the hard scattering of, say, leading quarks with $x \gtrsim 0.1$. One observes the secondary quarks with p_t, say, of 10–30 GeV. The cross-section could be enhanced relative to naive one-gluon exchange by two or more orders of magnitude. Because of the multi-gluon emission, the coplanarity of these two jets is expected to be washed out. However there should also be a 2-gluon, color-singlet, ladder exchange (the "hard Pomeron") of comparable magnitude present. This is the parton-level analogue of elastic scattering which in this strong, "unitarized" limit is arguably comparable to the inelastic contribution.

The typical production angles of these Mueller-Navelet jets are of order milliradians. Thus the jet cores are only of order 10 cm away from the beam axis when they strike the 100 m calorimeter wall. This should be quite observable in the FAD, although it does put strong demands on the quality and granularity of the calorimetry there. There are also problems of radiation hardness and good angular resolution which must be addressed.

Study of the multiple production of jets which underlies the Mueller-Navelet-BFKL physics is of comparable importance. The full acceptance of FAD is clearly of great value for such studies[10].

C. The cosmic-ray connection

Cosmic-ray air-shower physics in the energy range $10^{15} - 10^{18} eV$ is of great interest for its own sake as a probe of high-energy collision dynamics as well as for its astrophysical implications: the origins of such high-energy primaries are not understood.

An important ingredient in unraveling this physics is good understanding of the air-shower dynamics, especially in the early stages of evolution, where the leading-particle physics (Feynman x greater than 0.01) predominates. This region is essentially unexplored at contemporary hadron-collider energies. In the FAD detector, it corresponds to the physics at the 100m endwall and downstream of it.

There has been difficulty in accounting for the properties of air showers, as well as of individual events, assuming a smooth extrapolation of existing phenomenology upward in energy. This is true even after taking into account the expectations from perturbative QCD *a la* BFKL of extra energy dependence. And there remain the puzzling Centauro/Chiron event classes to explain as well[14]. A possible explanation, however, is suggested in the next subsection.

D. Disoriented chiral condensate and vacuum structure

Collisions with very high associated multiplicity produce "fireballs" which expand radially from the collision point at the speed of light, and which probably persist to a relatively large hadronization radius R, defined as that radius where the final-state pions are created as identifiable hadrons. Most of the energy and entropy of such a fireball arguably is found near its surface leaving a relatively "cool" interior. This interior region may be of special interest. Because of the approximate chiral symmetry of the strong interactions, the interior region may relax to a state in which the orientation of the chiral condensate in its internal symmetry space (σ, $\vec{\pi}$ coordinates in the σ-model description) is not in the σ-direction, but displace toward one of the π directions. The usual cost in surface energy existing in more gentle circumstances will here be negligible because of the large surface energy density of the fireball which isolates this disoriented vacuum from the true exterior vacuum.

The disoriented vacuum can survive only until the fireball-shell hadronizes, after which there is "penetration" of the exterior vacuum into the interior volume. During this period the classical field describing the difference of the disoriented and normal vacua will be radiated away. This is essentially a classical, coherent pion field with, for a given event a definite orientation of its (cartesian) isospin[15].

Some semi-quantitative studies of the nature of such coherent radiation have already been carried out[16]. In addition there are in the literature some closely related studies[17]. Because of the coherence of the source, this mechanism provides a candidate interpretation of Centauro cosmic-ray events (large fluctuations in the neutral-to-charged particle ratios) as well as the Chiron behavior (clusters of produced hadrons with unusually low relative transverse momenta) seen in the same event class[14].

A great deal of theoretical work can be done. Some is in progress. I hope that, at the least, working papers if not journal publications on this physics, as well as details of the experimental search strategies (including simulations), will be produced by the FAD Working Group in the next half-year.

E. Other physics

Much physics of special interest and appropriateness to a full acceptance detector such as FAD has been omitted in the physics "short-list." One may create very long lists of topics, which could form the table of contents for a conference on multiparticle production. Without going that far, we only mention a few other subjects where we expect active interest from others in the community.

One is heavy-flavor physics, not only bottom physics, but also charm physics. In both cases a goal for design of the ultimate Stage-N full-acceptance detector should be that this can be done with the highest standards of quality demanded by that community.

Another is the problem of physics of soft and semihard multiple production: minijets, multiplicity correlations, intermittency, etc.[18] Closely related is the issue of whether quark-gluon plasma is produced in high-multiplicity events.

Yet another is total and elastic-cross section measurements for meson-meson and meson-baryon collisions, using leading particle tags to isolate one-meson exchange contributions[19]. For example, the $\pi^-\pi^-$ interaction can be studied via observing two leading Δ^{++} baryons.

The process

$$pp \to \Delta^{++} + \Delta^{++} + X$$

can be extrapolated (Fig. 5) to

$$\pi^-\pi^- \to X \ .$$

In the same way various cross-section, *e.g.* πp, Kp, $K\pi$, $KK, \ldots$ might be studied. The formalism is the long-neglected "polology" of the 1960's and triple-Regge formalism of the early 1970's.

Given that the energy dependence of pp and γp collisions persists as a topic which attracts considerable theoretical and experimental interest, it would seem to make sense to extend in this way such measurements to a variety of other projectiles.

FUTURE PLANS OF THE FAD WORKING GROUP

There is a lot of work to be done in the next year. Thus far a start has been made on defining the problems of the far forward (100–1000 km) regions of the spectrometer, the regions most important to the cosmic ray community. There has already been a good initial connection with that community and I hope this will continue to grow in the future.

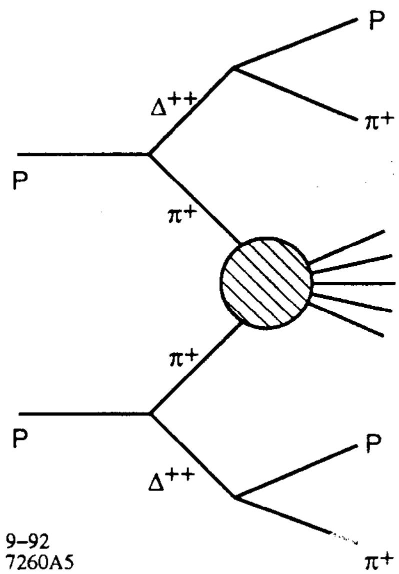

Figure 5. Double-pion exchange mechanism with Δ^{++} production in the final state.

In general, I would like to see considerable bottoms-up thinking on the architectural problems of this detector. I see this proceeding in stages, the first of which is to define the calorimetric architecture. Background studies are very essential in determining the amount of freedom of choice there is at this stage. Thereafter, the tracking architecture (without magnetic fields) might be the next natural step to be considered, with the magnetic architecture considered thereafter. As already mentioned, this conceptual-design phase of the study, which is where many choices of long-range importance will be made, should be in rather mature condition by the middle of the summer. At that time I hope there will be a variety of conceptual designs on the table, which can for a while be developed in parallel by their sundry proponents. Such design competition will be valuable in hardening the arguments for the ultimate design choices made at proposal time.

Important landmark events this year are this cosmic-ray symposium, where the far-forward part of the detector and the physics which goes with it can be further considered, and the Rocky Mountain Consortium meetings in Boulder during the month of July. These meetings are the one opportunity for an "FAD Snowmass" before proposal-writing time comes along. By the end of July a dialogue with the b-physics community ought to be initiated, so that by the fall, when there is a b-physics workshop at the SSC, there can be meaningful consideration of the compatibility and integrability of heavy-flavor physics initiatives within an FAD-type of detector.

Essential is the practical question of what defines a Stage I FAD. An obvious criterion is cost. At the December meeting we tentatively chose a value of \$(30 ± 10)M, roughly half that allocated by SSC to small initiatives. Also there was a tentative consensus that even at this stage one try to adhere to the full-acceptance philosophy, with a reasonably uniform distribution in investment as function of η. Since that time there has been some consideration of whether such a strategy is practical. This has led to a version of Stage I which might be called a toy detector.

The function of the toy detector is to acquire the coarse-grained pattern of individual events. The lego plot is divided up into coarse pixels of area 0.7×0.7, say. This amounts to about 200 in the full detector, excluding the special far-forward regions. The only information per pixel demanded is the charged multiplicity and the electromagnetic p_t, with hadronic p_t and isolated muons added in if it is affordable. Acquiring this information can be achieved with an elaboration of Pisa-Stony Brook[2] with essentially 1950's technology: a mosaic of scintillator telescopes, one for each pixel. With 20 panels of scintillator per pixel, and even an individual photomultiplier for each panel, this adds up to at most 4000 channels, an investment which may fit within the Stage I budget. Such a device standing alone could do a considerable amount of exploratory rapidity-gap-plus-jet physics, including arguably the observation at the SSC of the TeV Higgs discussed in the previous section[7]. And an R&D program with prototype versions of the toy in FNAL fixed target beams, as well as possibly FNAL and/or RHIC colliding beams, could provide an orderly pathway between now and SSC commissioning for testing the detection strategy, as well as for advancing the experimental status of soft, semihard, and hard diffraction processes.

In conclusion, I think the FAD represents a rather new detector concept which requires bottoms-up thinking in almost all its aspects. The next year should be filled with thinking more about fundamentals of detector design and various novel physics topics, and less about money and politics.

If FAD indeed succeeds, I think anyone involved will remember this experience with the greatest of pleasure. This kind of opportunity does not often occur.

Acknowledgements

Many members of the FAD working group, too numerous to enumerate here, have made and are making essential contributions to this effort, and it is a pleasure to acknowledge that here. I also wish to acknowledge the important support and encouragement of the SSC Laboratory.

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