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A FULL-ACCEPTANCE DETECTOR FOR THE SSC[★]

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I. INTRODUCTION

During this year, I have spent most of my time thinking about and promoting a detector for the SSC program which would emphasize physics at the lower—rather than the higher—mass scales. I believe this to be an important supplement to the generic program of TeV mass scale physics that dominates everyone's attention. I decided that the best way to approach the issue was to submit an expression of interest to the SSC Laboratory, detailing what is on my mind. This has been done. It is available as a SLAC document [1], and was submitted as EoI–19 to the SSC. The Laboratory and its program committee reviewed it this summer and, while noting that a single-author document is not a proposal and that a single theorist is a poor substitute for an experimental collaboration, concluded that it still appeared to make sense to enlarge one of the collision halls so as to accommodate such an initiative in the future. The next step will be creation of a real proposal by the end of 1993.

The detector concept will be described a little more in the next section. For here, let it suffice to say that the device is meant to be a survey instrument, with full acceptance for charged particles and photons: the "bubble-chamber of the SSC." Its strength would be the acquisition of a maximum amount of information per event, along with the greatest possible flexibility and adaptability in choosing what to do with that information. Because full acceptance implies the ability to see leading low-p_t particles, the detector must look something like two 20 TeV fixed-target spectrometers face-to-face. It is therefore very long, of order 1 plus 1 km, and modular—many magnetic stages.

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This modularity allows a staged approach to building the detector. The complete device is expensive, and it is totally impractical to believe there are resources available to have it ready for first collisions. However, a Stage I detector—for example, one that only covers the extreme forward-backward directions—is not so expensive, of the scale of a (reasonably big) fixed target experiment at Fermilab or CERN. Were such a detector approved and funded, I have no doubt that other groups, perhaps most likely from abroad, would come forward with enough manpower and resources to instrument at some nontrivial level the rest of the phase space.

What are the problems? I think the main problem is the perception that there is not enough physics to justify such an effort. I do not understand why this is so. It is not only the conventional minimum-bias menu (inclusive spectra, multiplicities, correlation moments, intermittency, etc.) that is available for study. There is a big program of diffraction physics (single, double, triple ...) available, including studies of "hard" diffraction (jets present in the final state). This subject is of great interest and has been neglected up to now, although experimentation at HERA should stimulate interest in it. The questions of saturation of parton distributions at small longitudinal fraction x can be addressed via forward dilepton, direct photon, and jet production down to values of x below 10^{-7}. In that kinematic regime, novel, unanticipated phenomena may be expected as well. Also, cosmic ray observations suggest the possibility of interesting leading particle physics at very low p_t, under 100 MeV. It is conceivable that such phenomena might lead to new ways of investigating the structure of the strong, QCD vacuum [17]. Also, perturbative QCD might be studied in new ways, for example via initial state gluon bremsstrahlung, or via looking for patterns in the event structure of multijet final states. New particle searches at and beyond the 100 GeV mass scale might be improved in sensitivity by exploiting underlying-event signatures such as rapidity-gaps, tagging jets, and/or initial-state bremsstrahlung. In general, while putting together the EoI, I was very impressed at how just the thought of observations of events free of acceptance cuts created new ways of thinking about a wide variety of topics, all the way from diffraction physics to the Higgs sector. I now have a lot of new theoretical physics topics to work on.

And this physics menu is not limited to programmatic physics; I think there is a great deal of discovery potential as well.

So I am here to encourage you to pay some attention to this idea. I do not know whether all this is an optimal match to the hadron spectroscopy which is your primary interest. Certainly this detector would do an extremely good job on charm and bottom physics. And it would at the very least illuminate a variety of issues which bear on hadron structure in fundamental ways, hence to spectroscopy as well.

In the next section, I will describe very briefly the cartoon of a full-acceptance detector as presented in the expression of interest, along with some of the basic features and technical difficulties. In Section III, I will describe some theoretical-physics spinoffs emergent from the preparation of the EoI, which bear on general issues relevant to hadron spectroscopy. In Section IV, I review very briefly the capability of the detector for spectroscopy per se. The final section is devoted to concluding remarks, which can be summarized as follows:

I need help!

II. THE DETECTOR

As mentioned above, the detector is essentially two 20 TeV fixed target spectrometers face-to-face, with the circulating beams going through the center of them. I take as basic, nonnegotiable specifications that photons and charged particles shall be seen, with momenta well measured, in all of phase space. At the SSC, this means pseudorapidities up to about 11 should be covered. The architecture is then largely fixed by the requirement of seeing the photons. Several annular calorimeter walls must be provided to catch them, together with endwall calorimeters at a distance of about 1 km from the collision point—not at all too far away. Behind each annular calorimeter wall can go an analyzing magnet for the charged particles. Special consideration must be made in the far forward/backward regions where the focusing quadrupoles for the circulating beams do double duty as analyzing magnets for the TeV-scale charged secondaries. This increases their apertures, length, and distance from the collision region. They naturally will be from 100 to 300 m from the collision

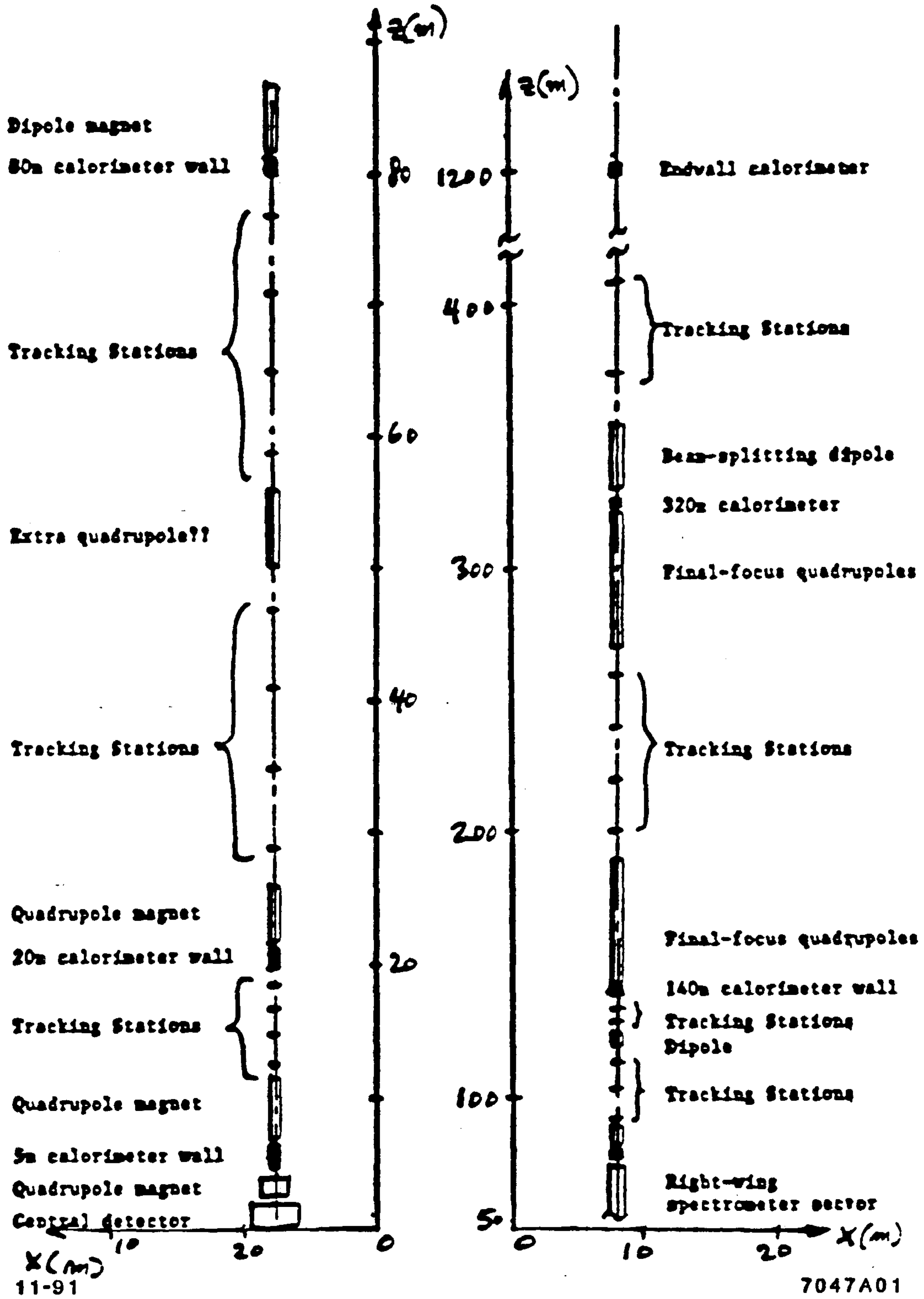

Fig. 1. One-half of the full-acceptance detector (FAD): (a) front end, 0–100 m; (b) downstream detector, 100 m–1 km.

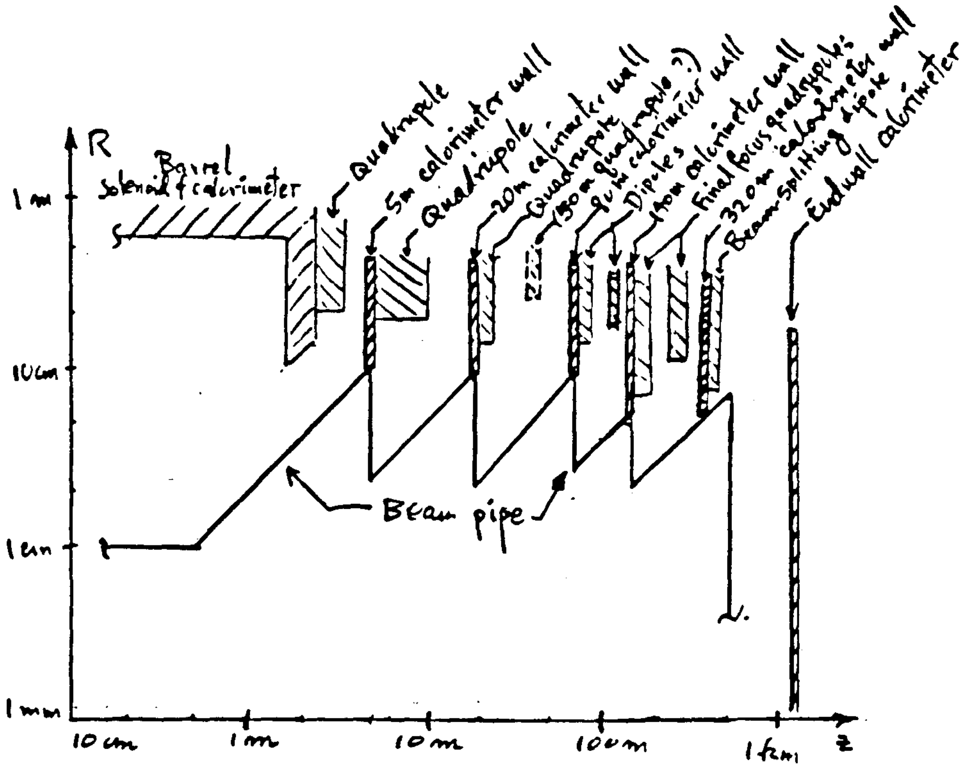

Fig. 2. The FAD in log-log coordinates. Tracking (not shown) fills the empty regions rather uniformly.

point, reducing the luminosity to a few percent of what the generic, high-p_t detectors receive.

A cartoon of what such a detector might be is given in the EoI and exhibited here in Figs. 1 and 2. It is essentially an oversize beam line, with the density of detection elements inversely proportional to the distance away from the collision point. The modularity should be apparent. Different modules (magnetic stages) of the detector can be labeled by the rapidity interval to which they are sensitive. Ideally, a given module should be largely blind to what goes on in the other regions of phase space. This feature may allow different parts of the experiment to be semi-autonomous. If so, I would think this could be a very powerful feature in allowing risk-taking in instrumentation and choice of physics goals, within the overall architecture of the experiment, without compromising the overall goals of the complete experiment.

There are plenty of difficult technical questions to be addressed. The beam-pipe design is very tough; a particle going through a 1 mm pipe at a not atypical angle of 1 mrad sees a meter of material. Backgrounds created by particles hitting the inner apertures of upstream calorimeter walls are a serious worry. The detector is compact transversely and may be in trouble from beam halo. There are radiation damage problems far downstream, even at this reduced luminosity. Some discussion of all of these problems can be found in the EoI. And I think they can be overcome. But obviously, a lot of study is needed to find out one way or the other.

In the EoI, I envisaged microvertex detection being provided in the phase-space regions appropriate to charm and bottom production. As for Čerenkov detectors, I left that question unconsidered, out of ignorance on my part. There are certainly regions of phase space where Čerenkov detection can be included, and others where it may be impossible. The apertures of the detector were chosen so that there would be, in principle, good efficiency to see vees and kinks from decays of low-p_t K's and hyperons.

III. SOME PHYSICS SPINOFFS

The topics in this section need not be directly linked to the physics justification for the full-acceptance detector (hereafter, FAD). But there is some connection, since they have been stimulated by thinking about it. The main issue is centered around what the valence degrees of freedom of a proton look like on arrival at the SSC collision point. For a variety of reasons to be touched on below, I think it is reasonable to view them as three constituent quarks each of rather small size, i.e., with radius of 0.2–0.3 fm. This picture is strongly suggested by the success of the additive quark model for total hadron-hadron cross sections [2]. And it is also expressed in the chiral-quark picture of Manohar and Georgi [3], an approach revived and extended these days by Weinberg [4]. If this is so, then the beam's-eye view of an incident

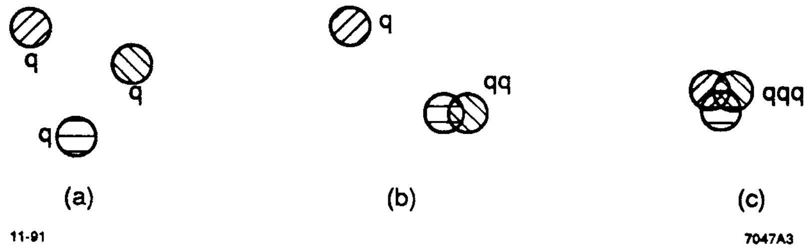

Fig. 3. Beam's-eye view of protons.

proton can be either three distinct quarks, or a quark and diquark, or a triquark (Fig. 3), depending upon the amount of shadowing present. It follows that there are a dozen or so distinct collision configurations possible, ranging from no quarks colliding directly ("string-string collisions"), to single-quark collisions with the remainder as spectators, to triquark-triquark collisions. It seems to me to be important to learn theoretically how the structure of individual events depends on the initial configuration, and whether these distinct configurations can be distinguished experimentally. Almost certainly the important information—if any—will be found in the forward-backward directions. But the signatures may not be so easy. For the no-quark or one-quark collisions, obvious candidates are presence of leading baryons, of low multiplicity, and/or of rapidity gaps. Likewise, absence of *any* leading hadrons and/or presence of high multiplicity may be signatures for triquark-triquark collisions and the like. Better signatures would be very welcome. Doing something about this theoretically probably requires working directly in the impact-plane variables, namely, transverse space coordinates (impact parameters) rather than their conjugate transverse momenta, as is usually done. The impact parameters of constituents carrying large amounts of momenta are constants of the motion—essentially classical angular momenta. It may be profitable to find ways of exploiting such conservation laws.

For me, thinking about the impact-plane descriptions mainly lies in the future. For now, my attention has been focused on whether this small-constituent-quark picture is reliable. It has plenty of implications by itself. The essence of the Manohar–Georgi picture is that, at distance scales of 0.2 to 1 fm, the important degrees of freedom are the constituent quarks, with mass of 350 MeV (for u and d), and the pions. The latter degrees of freedom must be there, if the constituent-quark mass is a consequence of the spontaneous chiral symmetry breaking, because of the Goldstone theorem. The gluons must also be included, but they are not viewed as very important; the effective coupling α_s is estimated to be about 0.35.

The presence of the pions in the effective interaction has, by the way, a spectroscopic implication. The Manohar–Georgi pion is not the $^1\mathrm{S}_0$ hyperfine partner of the rho, but a distinct, essentially massless collective mode which mixes with it. After mixing the conventional $^1\mathrm{S}_0$ state is driven upward in mass, into obscurity. The spectroscopic issue is how obscure? Is the 0^- $\pi(1300)$ state under the $\mathrm{a}_1(1260)$ [or the corresponding strange state under the $\mathrm{K}_1(1400)$] a candidate for this state?

There are other arguments for this picture of small constituent quarks. Weinberg emphasizes, in particular, that the axial coupling and magnetic moment of a constituent quark to good approximation is that of a pointlike Dirac particle, and that there is no evidence for excited states of constituent quarks. This suggests a lack of large-distance structure. In addition, the violation of the deep-inelastic sum rules of Ellis and Jaffe [5] (spin crisis) and of Gottfried [6] (isospin crisis) again allows an easy interpretation in terms of pion and kaon clouds around compact constituent quarks [7].

This whole subject is—for me, as a theorist—best addressed in terms of recent developments by Isgur and Wise [8] in heavy flavor physics. They have found great simplicity in the description of weak form factors of charm/bottom hadrons in the formal limit of heavy-quark mass tending to infinity. There is conceptual simplification to be had for pure strong interaction physics as well. Indeed, one may simply *define* a constituent quark to be the B meson when the b-quark is disregarded. Since in the Isgur–Wise limit, the dynamics of the b-quark is trivial, this means the strong dynamics of a B meson is essentially that of a single constituent quark [9]. For example, the strong quark-quark total cross section is the same as the B-B cross section. Electromagnetic and weak form factors of the light quark can be defined as well. The formalism is not quite conventional. For example, make believe that the *b*-quark has spin zero (in the infinite-mass limit it does not matter), so that the B meson has spin 1/2. Then the electromagnetic and weak vertex functions of the B will have the usual structure familiar from study of the nucleons, except that in the Isgur–Wise limit the initial and final four-velocities are constrained to be identical. Also, M/E $(= 1/\gamma)$ normalization must be used for the wave functions, and the four-momentum transfer q, which remains nonvanishing and finite in the limit, is orthogonal to the four-velocity: $q.v = 0$. The main point is that B mass and B momentum cannot appear separately in the vertex function—only the four-velocity appears. With similar kinematic modifications, the strong scattering of single constituent quarks from each other, inelastic as well as elastic, differential as well as total, can be described.

Alas, it will be a long time before colliding beams of B mesons are prepared. Nevertheless, the theoretical exercise of imagining what would happen and providing the formalism to go with it may lead to a useful way of thinking about the heretofore poorly defined concept of the constituent quark. The infinite-mass limit in heavy flavor physics provides a very simple and precise starting point. I like to use it in thinking about the Pomeron in particular [10]. The nature of the Pomeron is not yet well understood, and the FAD would be a superb instrument for study of that question. Two extreme views of the Pomeron are suggested by thinking about heavy flavors. In B-B scattering, the Georgi–Manohar picture invites a view of the process as the interaction of the pion clouds (chiral condensates) surrounding the constituent quarks. This, in turn, suggests predominance of the Amati–Fubini–Stanghellini Pomeron [11] built of ladders whose elements are mainly pions and constituent quarks, decorated perhaps with a small amount of glue and sigma fields. On the other hand, a naive view of high energy upsilon-upsilon scattering would be in terms of the interaction of two small color dipoles. This, in turn, suggests a Low-Nussinov or Lipatov Pomeron [12], with the ladders built primarily from gluons. But are there really two Pomerons [13]? A lot more theory and experiment will be needed to resolve such issues.

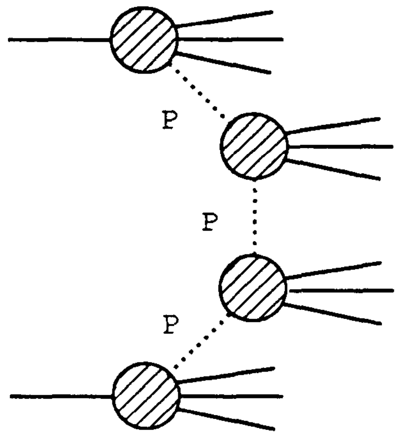

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Fig. 4. Pomeron–Pomeron diffraction dissociation.

IV. THE FAD AND SPECTROSCOPY

It seems like overkill to go to the SSC to do conventional spectroscopy. It, in fact, almost certainly is. But there may be a place for the use of spectroscopic information to learn about strong-interaction dynamics relevant to the superhigh SSC energy scale. Perhaps the nature of resonant structures seen in the beam fragments can help to elucidate some of the impact-plane questions introduced in Section III. And perhaps the states produced in diffractive processes can aid in determining the nature of the Pomeron. For example, one may study Pomeron–Pomeron diffraction dissociation (Fig. 4) into low-mass and not-so-low-mass systems, greatly extending the studies initiated at CERN [14]. Are such states rich in glueballs? Is there an enhancement of heavy flavor production? Or are the final states mundane, as suggested by the Manohar–Georgi picture of an AFS Pomeron?

However, there is certainly a place for charm spectroscopy at the SSC. Vast amounts of charm are produced. When working up the EoI, I made my own crude estimate of inclusive charm production, scaling up in a reasonable way from present energies. It is reproduced in Fig. 5. I do not guarantee the quality of this estimate. But no matter what, the conclusion remains; if charm can at the SSC be seen ef-

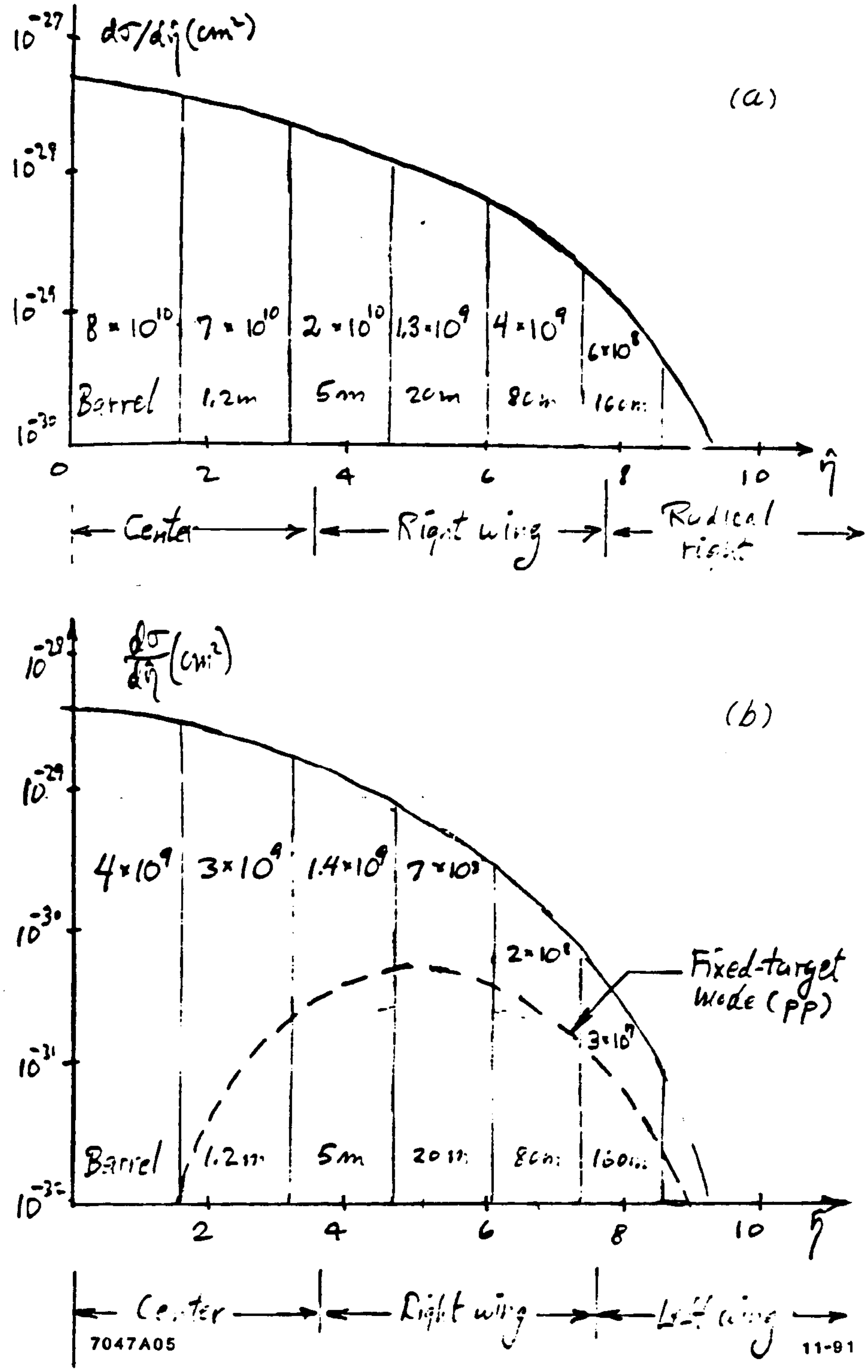

Fig. 5. (a) Rough estimate of charm production at the SSC. (b) Rough estimate of bottom production at the SSC, in both collider and "fixed-target" models.

ficiently, there is a considerable advance possible beyond what is expected in the next decade from Fermilab and tau-charm factories. They promise of order a million reconstructed charms per experiment. My guess is that at least another factor of 100 to 1000 is, in principle, available at the SSC with an FAD.

There are both challenges and opportunities in using an FAD for charm physics at the SSC. The choice of which rapidity interval to emphasize may well favor relatively forward directions, where the detector architecture is planar, and all tracks of interest are normal to the silicon planes. As in fixed target work, many silicon planes per track may be used, something difficult to accomplish in collider barrel geometry. On the other hand, the presence of the circulating beam in the middle of the detector, along with the beam pipe, creates challenging obstacles. And there will arise the question whether such an ambitious program is worth it. I think that that question will be best defined by the charm physics yet to come. Certainly there will be frontiers, such as the search for and study of exotic charmed baryons like the ccu or even the ccc [15]. But one may demand more than that.

Finally, the FAD is quite a good detector for bottom physics. The relevant part of the detector is the portion upstream of 50–100 m or so. The question of B physics at the SSC has been extensively studied [16], and I have not much to add here to it. The physics is clearly superb. I would only comment that the FAD is not optimized for B physics. And since B detectors are engineered for the very important CP violation physics, it is unclear that the compromises required to allow detection of leading beam fragments are compatible with the mission of a B detector. In particular, there is a limit to the luminosity of the FAD because of the relatively clumsy and bulky final focus quadrupole magnets. And the upstream magnetic architecture of the FAD is constrained by the requirement that the forward charged and neutral particles are to be seen downstream and measured. However, at this stage of development, it is not clear that there is incompatibility. And there may be advantages for the B detector from the extra acceptance and information per event provided by the FAD. The question needs study from both sides.

V. WHAT COMES NEXT?

The main problem now with the FAD initiative is simply that there is a lot of work to do, with so far very few people coming forward expressing interest in doing it. The SSC Laboratory thus far has been as receptive and encouraging as can be expected. The next formal landmark will occur at the end of 1993 or early 1994, when proposals for "small" SSC experiments are due. I personally wish to remain a theoretician. But I believe in this idea and will stay aboard at least until proposal time. At present, I and those experimentalists who have expressed interest in working on this idea are beginning to get things together. We hope to have a first organizational meeting before the end of the year. The idea is to form an FAD working group, with no formal collaboration created until near the proposal due date, when the real leadership (not me) is created. Meanwhile there is a lot of work to do. The topics include the physics menu, the detector architecture, the problems of backgrounds, beam pipe, radiation damage, etc., as well as the data acquisition and analysis techniques needed to deal with and exploit the large amount of information per event. An important first question will be what portion of the full detector is best suited to be the Stage I.

I think the FAD represents an opportunity to do exciting physics at the SSC within a modest sociological scale, comparable to what exists nowadays in the fixed-target world. I very much hope that there emerges enough interest that this opportunity is not lost. If you find this interesting, please get in touch with me; I am BJORKEN at SLACVM.

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