THE NEXT STEP: ACCELERATORS VERSUS STORAGE RINGS

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The reason I was willing to accept the invitation of Dick Neal to come here was largely because of the opportunity it gave me to collectively pay homage to the community of accelerator scholars for all the magnificent instruments which have been provided to me and presumably the users I represent here. Perhaps he picked me because I am old enough to have been through so many of these things, starting with a Nevis Cyclotron, and progressing through the Cosmotron and the AGS, and recently, the ISR, and the Fermi National Accelerator Lab with even occasional forays to the Bevatron and PPA. The teamwork of the users and the builders is part of our profession. We work hard in mutual stimulation. You make the machines, and we try to use them so well that the need for more machines becomes self-evident. Both of us, of course, working so that Gell-Mann and his friends become more and more famous. (That is not really fair. It is really an unholy trinity, engaged in a more or less honorable endeavor, the significance of which for history and for the future we do not have to elaborate here.)

My subject is to put the thing in perspective, and I have a slide here for people who do not travel so much. This is a picture of the Fermi National Accelerator Laboratory (Fig. 1) where I have been spending most of my time. Let me just

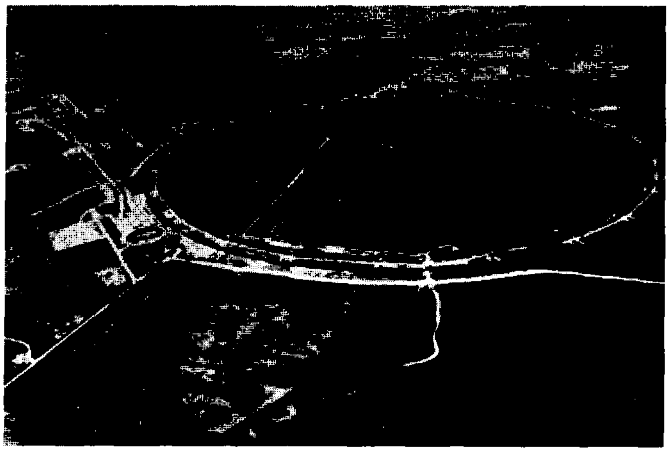

FIG. 1--Air view of the Fermi National Accelerator Laboratory.

say that I am very poorly prepared for these remarks, largely because of the fault of some of you here. I had planned to prepare this talk in some detail during the breakdowns of the FNAL accelerator, and for the last six weeks or so there have been so few.... It is really a thing of great beauty. We sit three kilometers from the accelerator and watch a beam sit steadily on a target which is only about 0.4 mm in transverse dimension, and with a duty cycle and intensity which is just positively embarrassing. This is, though, the last word in accelerators, and I always wondered what future archeologists might make of this — and to illustrate that I have another slide, which might surprise you (Fig. 2). It is clear to me the mystery of Stonehenge is solved! Some of you might even appreciate the next figure (Fig. 3), which shows how this was built. You see your progenators, the accelerator builders of an early day — I do not know if you recognize anybody.

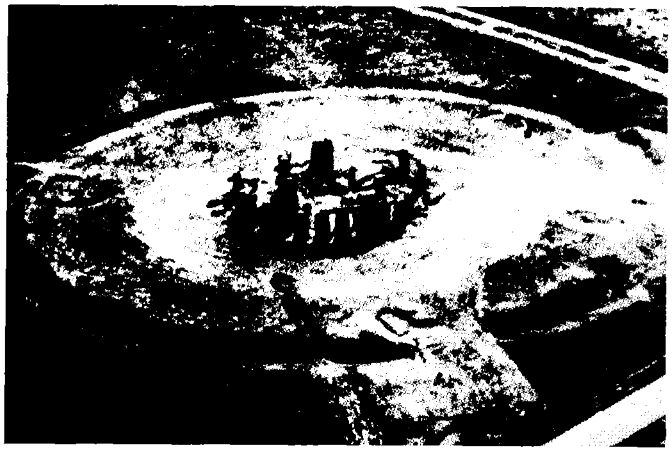

FIG. 2--Air view of Stonehenge.

FIG. 3--Construction of Stonehenge.

In considering my assignment, The Next Step, I decided to be as general as I could, considering configurations that are thinkable in my lifetime as a physicist, which might not last more than another fifteen or twenty years. In reviewing the history, there were always two arguments used for a new accelerator. One was to answer existing questions of the kind Gell-Mann reviewed for us in beautiful detail, and the other was that the accelerator should permit the kind of exploration that has always led to totally new questions — even undreamed of, like CP violation, or strangeness, or some of the many other discoveries that were made in a totally surprising context. And both these steps are quite important. I might give an example of something which is a little bit out of context of here, but something I would like to bring to the attention of this gifted group. Some of us, a few years ago, wrote little notes on the interest that might accrue to collisions of high energy uranium with uranium. I wrote such things, stimulated by some work we did with antiproton production far below thresholds, a nuclear physics question, and then I read a note by Francis Farley, who took off on some speculations of Cocconi about the virtues of complexity. These are highly speculative things, and therefore there was no dream that one should actually spend real money on accelerating uranium to very high energies. But recently, starting from very abstract theoretical ideas, Lee and Wick have produced a nuclear physics theory which has greatly stimulated the idea of going to many GeV per nucleon. It is just these kinds of connections — remote connections — which have made our subject so interesting.

Let me now talk about more relevant things. I was asked to talk about storage rings versus accelerators. And much to my surprise, I found that the ISR has been, in a sense, too successful. Because if one looks at the current listing of unfunded projects, one finds six storage rings — that is something people are thinking about — five are electrons, one is a proton-proton storage ring, and no conventional accelerators! How could this be? What happened to the conventional accelerator? Is a new, conventional accelerator unthinkable in the time scale of the next ten-fifteen years? Thus, in the time I had to prepare this, I considered whether this is a wise thing — is it wise to forgo the opportunity for conventional accelerators?

In Fig. 4, we have a review of all possible experiments, trying not to omit anything. We have lepton-lepton, lepton-hadron, hadron-hadron interactions. We write "storage" where it seemed obvious that a storage ring was the right thing, and "accelerator or storage" when that is possible, and accelerator clearly when you require secondary beams. Now it could be that $\bar{p}p$ could also go in storage rings, but that is somewhat of a quibble. Here is then an almost complete list of the kinds of experiments you can do unless somebody finds something which is different from a hadron or a lepton. (I teach physics for poets, and I got an examination paper last week in which a hadron was defined as "a no longer active particle physicist.")

lepton-lepton		lepton-hadron		hadron-hadron	
e^+e^-	S	ep	A or S	pp	A or S
e^-e^-	S	μp	A	$\bar{p}p$	A
		νp	A	πp	A
				Kp	A
				Yp	A

S = Storage
A = Accelerator

But there are alternative reactions, e.g.

$$\left.\begin{aligned} \mu^+ + Z &\to \mu^+ + \mu^- + \mu^+ + Z \quad \text{(tridents)} \\ &\to e^+ + e^- + \mu^+ + Z \\ \nu + Z &\to \mu^+ + \mu^- + \nu + Z \end{aligned}\right\} \quad A$$

or

$$p + p \to \ell^+ + \ell^- + \text{junk} \qquad \text{A or S}$$

FIG. 4--All possible experiments to do at accelerators and storage rings.

There are alternative reactions (Fig. 4) sometimes, which are not totally obvious, but which permit the study of lepton-lepton scattering using nuclei as observers — this works if the incident energy is high enough. There are also some interesting experiments certainly, like neutrinos going to muon pairs, and other experiments which involve lepton-lepton interactions, as we will see, where the leptons come out of some complex and maybe not so pretty initial state.

In looking at these experiments, one must remember that there are deep interconnections — for example, there are three which study hadronic electricity: the e^+e^- goes to hadrons, which is of enormous current interest today — it is worked on at SPEAR; the deeply inelastic scattering; and perhaps electron pairs coming out of what you have in the way of hadrons, say proton-proton collisions. If you want to test the complete theory of the electrical structure of hadrons, presumably you need information on all three kinds of reactions. And then, again, looking at e^+e^- goes to hadrons, the data look very much like pp goes to hadrons. The hadronic things coming out look so much like the yields we see at the ISR, that clearly there is an important connection, and that further investigations of both will have to go hand-in-hand.

I will now survey briefly a set of experiments which one would like to do. Most of these are extrapolations of currently active experiments. The extrapolations of expected cross sections are made here in these next few charts, on the basis of data at lower energies, on the basis of models, and sometimes dimensional scaling, and then maybe sometimes on the basis of just nothing at all. In contrasting storage rings and accelerators, I take 5 TeV as a thinkable accelerator, largely because it fits on the Fermi National Accelerator site — and has roughly the same relation to a thinkable storage ring (and here we have had a great deal of thought) as ISR and FNAL, which are the complimentary accelerators we are working with today. And clearly, as we study FNAL and ISR, and the relationships of the two kinds of experiments, we will learn more about the validity of the kind of comparison I am doing now. So my thinkable accelerators from the point of view of hadron collisions are a storage ring of at least 200 GeV against 200 GeV, for Super ISR, and say roughly 5 TeV for Super FNAL. These are arbitrary numbers, if you like, but thinkable in the time frame of the next ten or so years and at costs which very probably do not exceed the annual construction costs we have already inflicted on the U.S. taxpayers. I have made up a report card, Table I, (this being the end of the semester),

TABLE I

Report Card

	Super ISR (SISR)	Super FNAL (SNAL)
• $\sqrt{s}$	≥ 400	~ 100
• usable $\sqrt{s}$	depends	$\to 100$
• usable luminosity[a]	$\to 5 \times 10^7$	10^{12} [b]
• dwell time[c]	B-	B+
• experimental area's flexibility, cost	A	C-
• particle identification	B-	A?
• backgrounds	D	D
• secondary beams	F	P
• interaction of expt. and machine	C-	A-
• sociology	B	A+

Table I (cont'd)

(a) But don't forget Fermi motion.

(b) Only for pp experiments.

(c) Compromises — energy, intensity, special tricks, unequal energies.

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where some entries are in the form of numbers and some are just letter grades. In some cases, we use the pass-fail option. The first entry is $\sqrt{s}$ which is a ridiculous notation for the energy available in the collision. Super ISR is listed as more than 400 GeV and Super FNAL (5 TeV) about 100 GeV. But I also made a note here, "Do not forget about Fermi motion", because for some special experiments 100 GeV can go surprisingly high paying a price in luminosity. Another entry is usable energy in the center of mass. Usable energy is not always all of the available energy and this depends, again, very much on the kind of experiment you do, and also on what comes next, which is luminosity. I think it is fair to say that a conventional accelerator can probably exploit the full energy available, 100 GeV. It has not yet been established at FNAL, but we will see that it is not terribly far away. For the ISR, whether you can use the full square root of s depends upon what you are talking about. Certainly for the exploration of totally new physics, production processes, it does not seem as if you will ever use the full energy unless luminosities are much, much higher than are being talked about. On the other hand, for tests of scaling, where you have various scaling parameters, for example $2p/\sqrt{s} \equiv x$, you want to make this vary over a very large range — then the mere fact that energy is available is useful.

The next entry in Table I is luminosity and I took for this what might be plausible, namely a beam of a few 10^{13} ppp which gives $\sim 10^{12}$ interactions per second for SNAL. For the Super ISR, I assumed $\sim 10^{33}\,\text{sec}^{-1}$, which gives 5×10^7 interactions per second. That is luminosity. On the other hand, usable luminosity is a different thing. Usable luminosity for a conventional accelerator is relevant to experiments using primary protons which, if we look at FNAL, is a small fraction of all the experiments. Most of the luminosity at NAL is being used to generate secondary particles. That is how you can compensate for the fact that you have to build a 5 TeV machine to get a mere 100 GeV available. You make lots of secondary particles. Of course at the ISR you use the full luminosity — to date only $\sim 5 \times 10^5$ interactions/sec. Experimental techniques for surviving at the projected SISR rates are in principle on hand but have yet to be proved practical.

Again, even in pp experiments, it is not at all clear that the full luminosity at NAL is usable. At NAL recently, when the beam intensity reached 10^{13} particles, it was at times embarrassing, because if the proper experiments, for example neutrino experiments, are not running, even though the beam is split many ways, there are many experiments that cannot use the full luminosity. The apparatus just does not work. It is not easy to make full use of primary protons at the full intensity — you have to use tricks. However, it has been done in special cases and the important thing to note is that the intensity is there.

There are other entries like dwell time — how long do you sit to do an experiment. You sit as the ISR a long time because there are lots of compromises. Since you have many experiments around the ring, if one experiment wants high energy, and another experiment wants low energy, clever as the people at the ISR are, they have not yet been able to change the energy as one goes from one crossing region to the other. (I have a lot of confidence in them, and I expect that one of these days, they will solve the problem.) But until they do, it is a problem and so you have meetings at which one has to make a compromise, and at the ISR it is usually what Carlo Rubbia wants. To get your particular spectrum of conditions you have to wait a long time. Someone every once in a while comes in and he says, "I only want 1 A today." If he is convincing, we have to wait; there's the usual compromise. Now at NAL, for example, there is the new technique of the front porch, where several energies might be available at one time; this can certainly be a great help, and source intensities are no problem — many more groups can more or less control their own intensity, although that still needs some improvements. SNAL gets good grades for this.

Then there are things like experimental areas and flexibility, which are really quite a problem at a conventional accelerator. The bigger the accelerator, the bigger the problem. The experimental areas are complex, and costly, and not nearly as flexible as people hoped, certainly for NAL. Whereas relatively, I think, the experimental areas at the ISR are fairly simple — they are much smaller, physically, and more easy to rearrange. They also cost less.

Particle identification in doing experiments is a problem. Using a many TeV beam, if one scales Cerenkov counters, for example, one finds enormous lengths. A several mile long Cerenkov counter boggles the mind a bit. On the other hand, there may be other techniques that are, or may become much more useful at SNAL, e.g., the relativistic rise in the ionization loss, or perhaps transition radiation; there are techniques which are being developed and there is clearly a lot of interaction between instrumentation and the exploitation of accelerators. Particle identification at the ISR is easy in the forward direction because you can use Cerenkov counters of moderate length. Here, what is relevant is that at SNAL you are dealing with particles of many TeV, whereas at SISR one deals with particles of merely 100 GeV; now we have learned how to deal with those particles. On the other hand, the need to exploit the limited luminosity at ISR requires large aperture equipment and this means that it is much more difficult, say, at 90°, to do particle identification; most of the good particle identifiers have small apertures.

Backgrounds: Well, I find everywhere I go there are lots of backgrounds. I do not give good marks to any machine. This is probably my problem. Maybe electron machines are better. On the other hand, most of the cross sections I will show you that are of interest at NAL, or SNAL, are a much smaller fraction of the total cross section than they would be at SISR; so just the energy gives you a better grade for ISR's.

There is one thing that gives ISR types a bad grade, and that is the interaction of the experiment and the machine — what I call "real life at the ISR". You are very much coupled in with the machine at an ISR. It is well known that you cannot keep these clumsy experimentalists away from the machine, and every once in a while they knock something over, and the machine goes out for months — that is hard to avoid. Whereas at NAL, close contact between experimentalists and the accelerator proper is at a minimum.

Sociology in Table I just means how many physicists can you employ, and it is amazing how well the ISR does. But that is also because it is in Europe, where large groups were pioneered. We once counted 200 Ph.D.'s working at one time, and I do not think that NAL can beat that, although in principle it should. So I gave that a better mark anyway.

Well, now I want to sort of back up this report card with some information. The effect of Fermi motion, if you like, or essentially the nuclear physics boost you get from hitting a nucleus instead of a free proton, scaled up from the work we did at PPA and the Bevatron to NAL, is given in Fig. 5. In one collision out of 10^5, the 28 GeV that is usually available from a 450 GeV accelerator goes way up to 40 or 50 GeV. This is not an unmixed blessing, It is interesting for certain explorations if you have a good signal you can then ask about a particle whose mass is 40 GeV. The same considerations will enable SNAL to go to ~ 140 GeV.

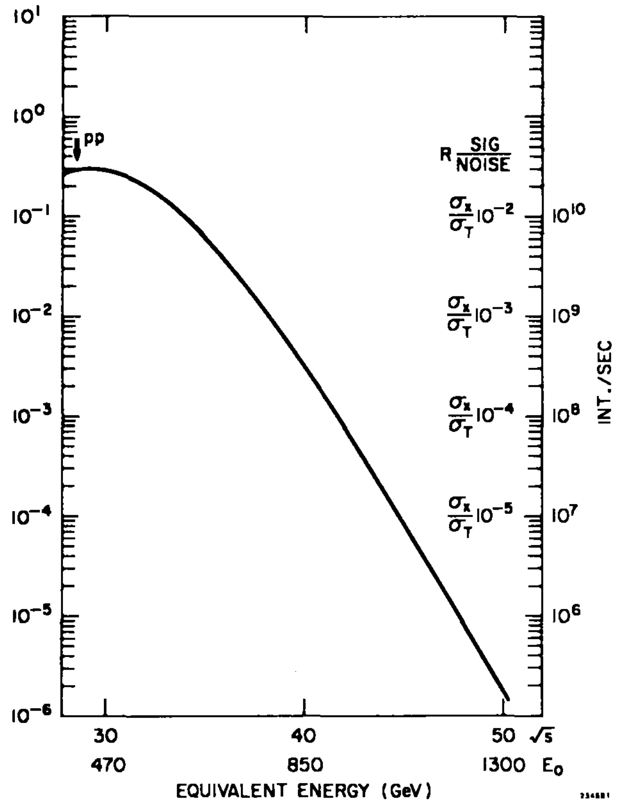

FIG. 5--"Fermi motion boost" at NAL (450 GeV).

I talked about usable luminosity, or using luminosity that is available. Let me go through a quick history of the ISR to illustrate this. Shown in Fig. 6 is a charged particle spectrometer, the Saclay-Strassbourg spectrometer, which has produced some nice results on charged-particle yields at 90^0. You see the crossing region and you see the spark chambers and the magnet, more spark chambers, and other identifiers. The total solid angle of this apparatus is of the order of 0.01 steradian. I would like to contrast it with another approach at the ISR, which was the so-called CCR experiment, which insisted on having a very large coverage, about 1.0 steradian on each side. This experiment was interested in large, transverse momentum π^0's, and tracked a yield curve out to a transverse momentum of ~ 9 GeV. It stopped running at the end of 1972, and here it is in 1974 and the spectrometers, of which there are several working at the ISR (one is shown in Fig. 7), have still not really gone beyond about 4 or 5 GeV, to my knowledge. So the use of the luminosity is not a trivial thing, and one pursues this problem.

Here is a picture of what our groups hopes to do next at the ISR, which is essentially a 2π spectrometer (Fig. 8). This is a now super-conducting magnet, with spark chambers inside, and glass outside to look at γ rays, and a wall thickness of only ~1 radiation length, so that one does not disturb the photons too much. This would help the ISR in achieving luminosity times solid angle, which might go up by a factor of ~ 50 or so above the previous generation of experiments.

The same sort of thing happens at NAL. The next figure, Fig. 9, is useful for comparing SNAL against the SISR. Here is a Cronin-Piroue data on production of pions. Note that they have, in fact, a rather small aperture but still they are able, with some trouble, to achieve cross sections of the order of $< 10^{-37}$. In terms of exhausting the kinematic limit you see data out to $x_\perp$ ~ 0.7 where the maximum value of the accelerator is 1; you see they are not too far away.

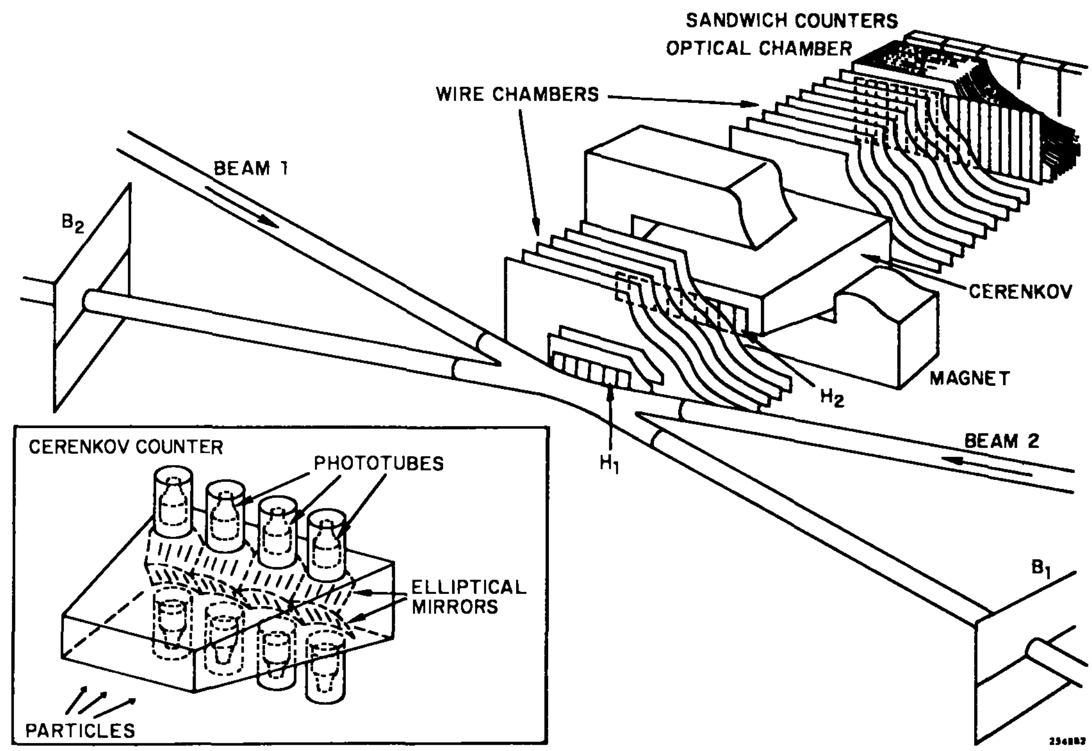

FIG. 6--The Saclay-Strassbourg Spectrometer at ISR.

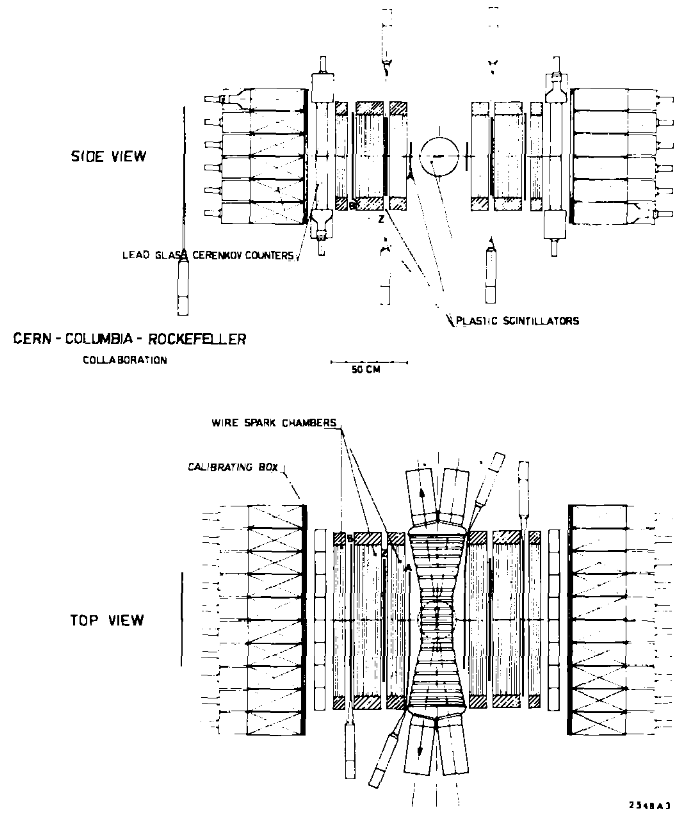

FIG. 7--The CCR Experiment Spectrometer at ISR.

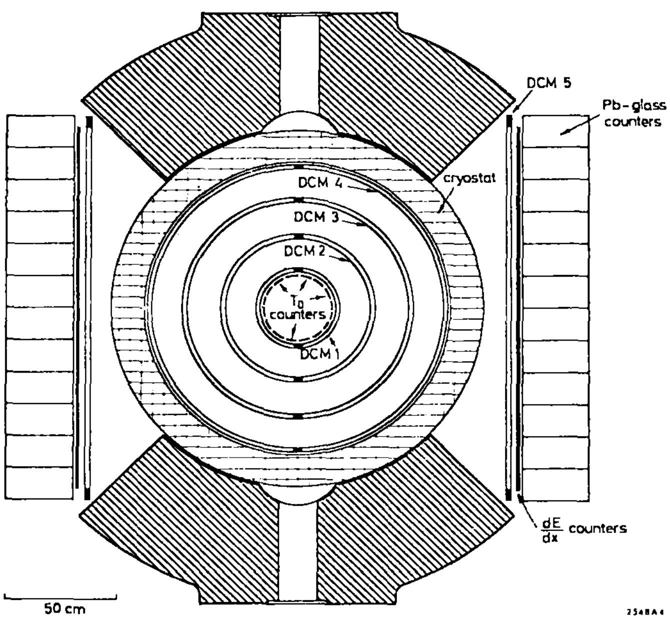

FIG. 8--A planned 2π Spectrometer at ISR.

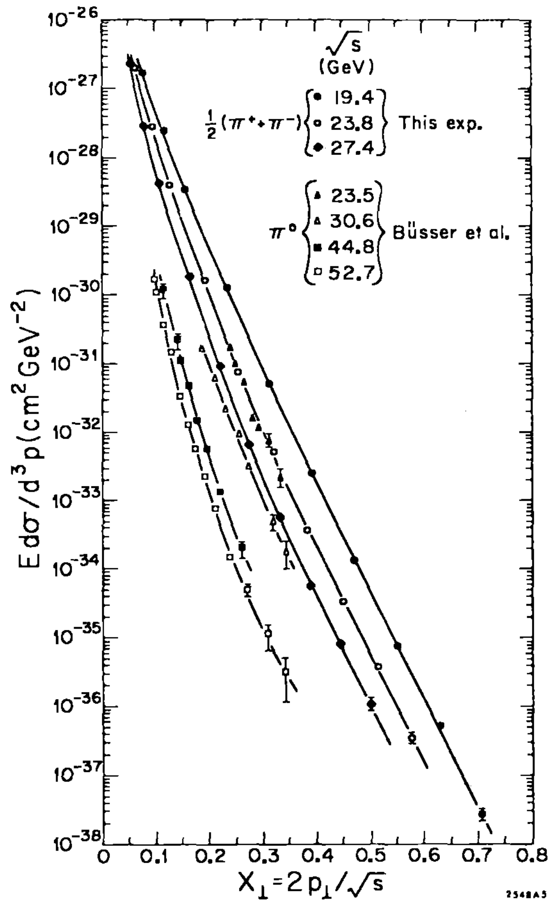

FIG. 9--Cronin-Piroue data on pion production.

And this is a very early NAL experiment. The comparison with data taken at ISR is given in Fig. 9, where even though the ISR energy is much higher, the luminosity is such that with a reasonably good solid angle, one could not go above $x_\perp \sim 0.4$. This is a good example. Energy you have, but limited luminosity prevents you from exploiting it fully. Of course, that was one stage. One hopes one will be able to go on, in both experiments. We will always be more inhibited at the ISR. This is the luminosity price.

Now let me go on to some other experiments. Figure 10 is the famous rising total cross sections. There is some bubble chamber data from NAL and there are the famous ISR experiments showing this rising cross section. New data from the total cross section group at NAL have experimental points which also show a rise with statistical errors in this region of the order of one or two-tenths of a percent. So here, again, you see a comparison between an ISR experiment where you have difficult experiments with fairly large error bars, but a big handle in s, as opposed to very precise experiments over a more limited range of s. What is even more interesting about the NAL approach is that they also have cross sections (Fig. 11, 12), for π's K's, and antiprotons, incidentally all of which look as if they were rising

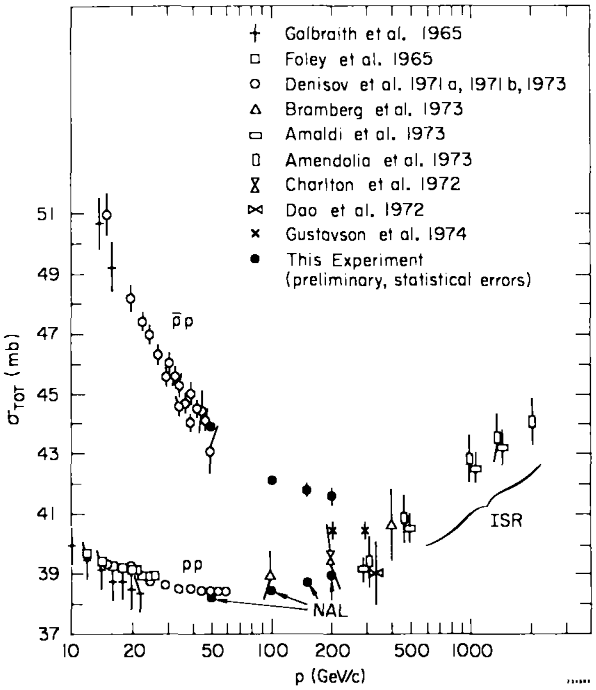

FIG. 10--The proton-proton total cross section.

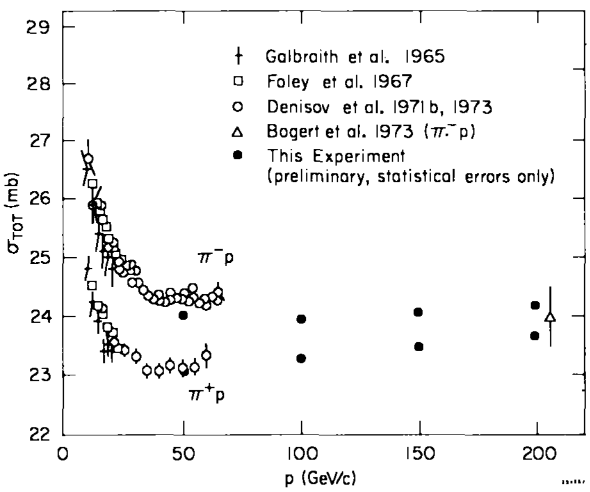

FIG. 11--The $\pi^{\pm}p$ total cross sections.

in this region. And it seems to me that in any incisive elucidation of this phenomenon, the need for secondary particle cross sections is also important. And perhaps I ought to leave this as a question: would theory be totally happy having only the proton-proton data?

The next Figure (13) deals with an extrapolation of total cross sections under various current theories and some possible SNAL and SISR "results". Following this (Fig. 14) is a multiplicity extrapolation and then (Fig. 15) a slope parameter in elastic scattering. The "stretch" in rapidity for ISR, SNAL and SISR in two current models is given in Fig. 16. Here was a clear ISR break since the NAL parameter s cannot extend far enough — only the ISR could discover the "central plateau".

Let me go on to some other experiments, and extrapolations thereof. Figure 17 is a strong interaction experiment which is a continuation of high p perpendicular, proton plus proton goes to pion plus anything. This is an extrapolation of

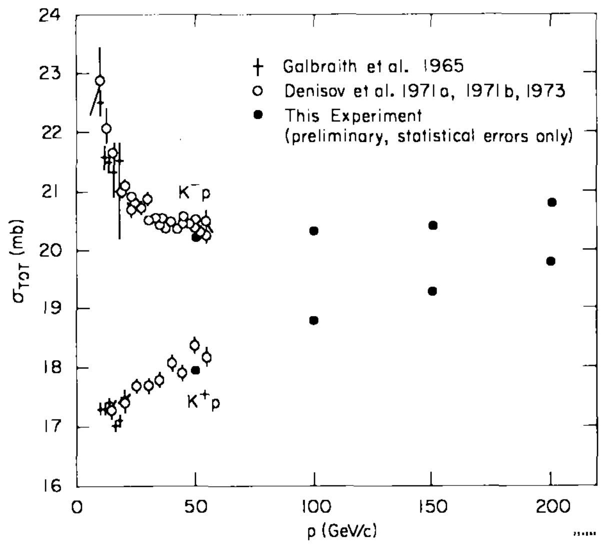

FIG. 12--The $K^{\pm}p$ total cross sections.

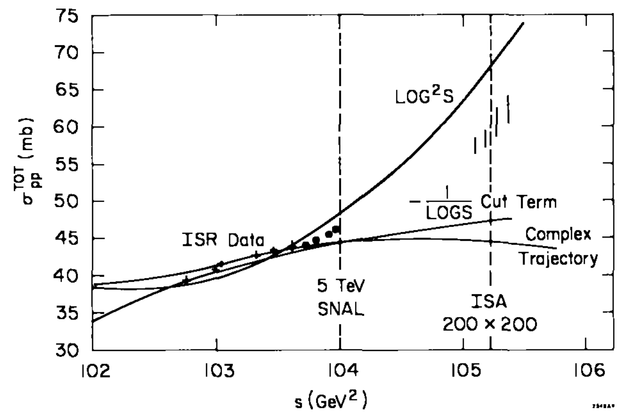

FIG. 13--Model-dependent extrapolation of the proton-proton total cross section.

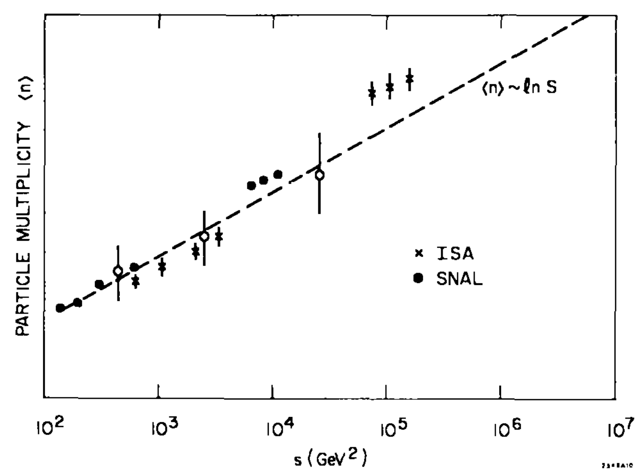

FIG. 14--ℓn s extrapolation of the particle multiplicity.

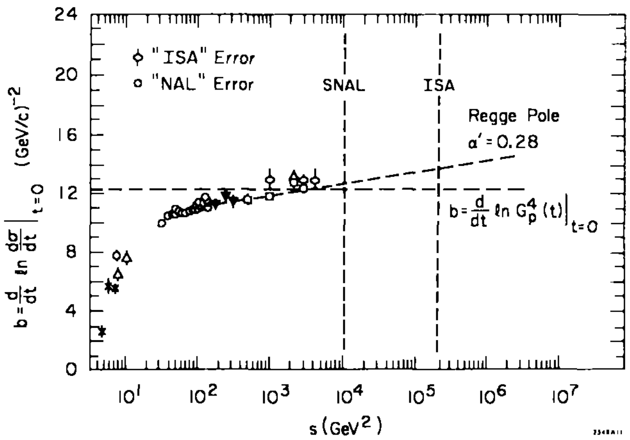

FIG. 15--Slope parameter in proton-proton elastic scattering.

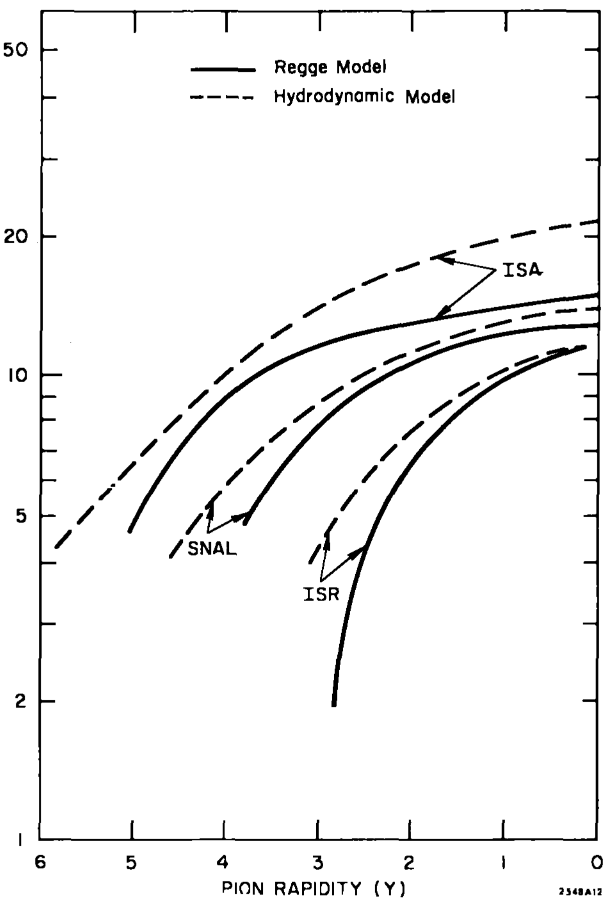

FIG. 16--Two model-dependent extrapolations of the pion rapidity distribution at ISR, Super NAL, and ISA.

ISR data, to SNAL and SISR and you see that 20 GeV/c is observable if one can go to cross sections like 10^{-39}, and I think that is certainly possible for SNAL. On the other hand, for an ISR I think it is very hard to go say, below 10^{-37}, but this also gets to ~20 GeV/c.

The two machines in this particular piece of physics, where the s dependence is not very dramatic, are roughly equivalent. SNAL however permits additional information to come from incident pions, kaons, and $\bar{p}$'s. On the other hand, one of the things which is most intriguing to me in high

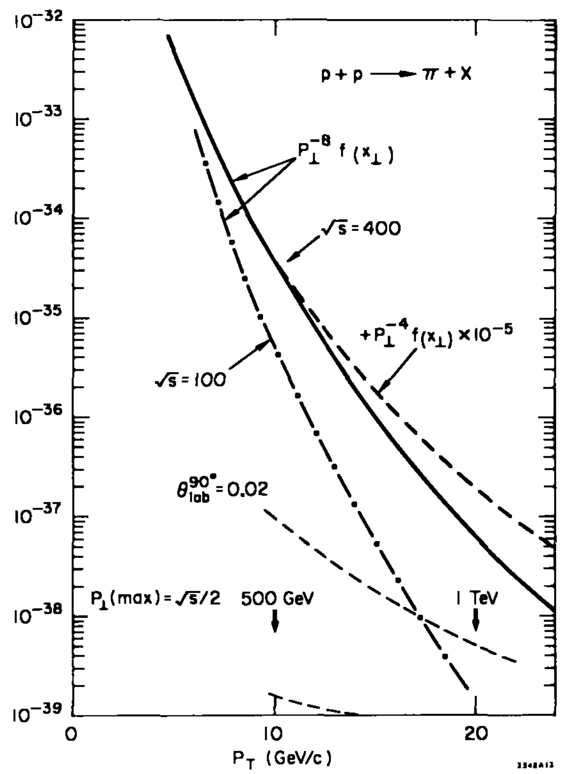

FIG. 17--Cross sections at $\theta_{cm} = 90^0$ for inclusive production of π mesons, as a function of the transverse momentum of the π meson.

$P_\perp$ phenomena is the possibility of observing interference effects between strong and electromagnetic, or between strong and weak interactions. One does not really know where those interference effects take place — they may take place near $P_\perp$ ~ 15 GeV/c, or maybe a little bit beyond. And here you might, then, look at strong interactions for violation of discrete symmetries in order to detect such interference. You might look for, say, parity violation, and so on. For that reason, you are certainly led to a greater interest in the high energy. And that will be primarily true, in general, for weak interactions.

Figure 18 shows another way to look at it. This is a sort of kinematic region from the point of view of testing any scaling laws for deep inelastic stuff, and clearly SISR takes you out a long way in s; on the other hand, SNAL covers a big chunk too.

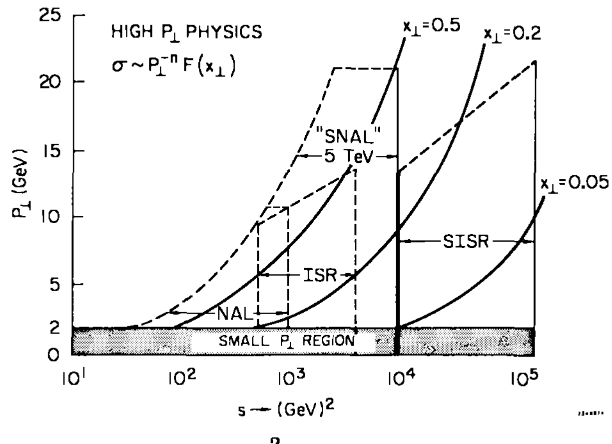

FIG. 18--Regions in s(GeV^2) and $P_\perp$ (GeV) available to NAL, ISR, Super NAL, and Super ISR.

Figure 19, shows a case of looking at electromagnetic interactions with proton machines, again in a large storage ring, one can (if one believes in these extrapolations which are admittedly model dependent, maybe totally wrong, but which are at least thinkable) explore masses, look for things

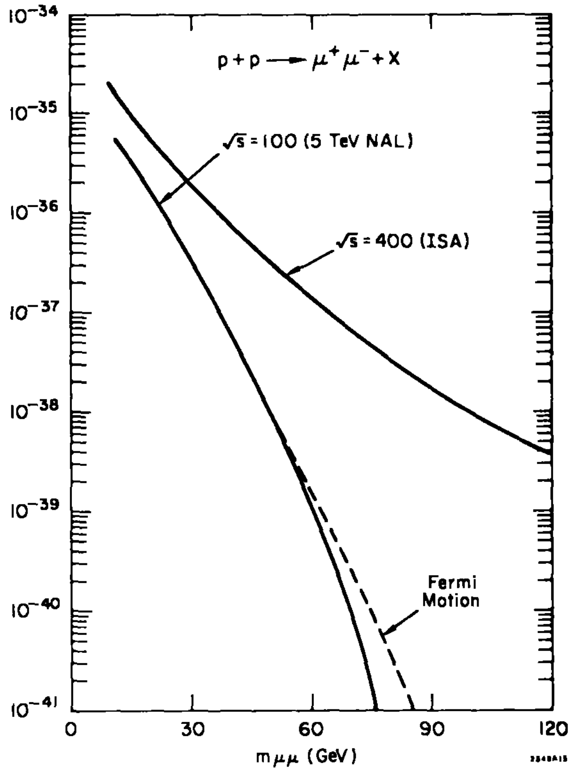

FIG. 19--Electromagnetic interactions with a proton machine; μ-pair production at a 5 TeV NAL and ISA.

up to enormously large masses, because again if we can reach 10^{-37} we seem to have sensitivity out to about 60 GeV of mass. Again, though, if you look down here at 10^{-39}, you see that you have about the same limit with a conventional accelerator. Why would you want to do this sort of thing? Well, there are several reasons. One is, you would like to look for bumps, you would like to look for the Z_0's, and a Z_0 on a background of this kind would presumably show up as some resonance in the effective mass of $\mu\mu$ or ee or whatever lepton pair you look for. Also, if the continuum does have some size which can be measured, one wants to answer questions of scaling, and you might also want to calibrate the search for intermediate bosons, which presumably have the same hadronic part but an interesting and new leptonic part. Figure 20 shows one of a long series of things that can be made with the virtual photon flux discovered in Fig. 19. These are heavy leptons. These curves are steep and so SISR, for the first time, really takes over (up to $m_L \sim 80$ GeV). Other "things" calibrated by Fig. 19 are quarks, monopoles and, in fact, anything that can be pair produced at say, PEP. We DO NOT discuss the relative experimental problems without bullet proof vests.

We now come to what could be the most interesting point: weak interactions. The main point about pp collisions is, of course, that you have such high energy. Somehow we have to be freed of the tyranny of doing neutrino physics. At the moment we are stuck with it, but it may be that one can never do weak interactions with protons because the effective signal is too small. One does not know yet. In perhaps two or three years, one will know whether this is a feasible reaction. There are two ends here: one is actual production of the intermediate boson, and the other is weak interactions without an intermediate boson.

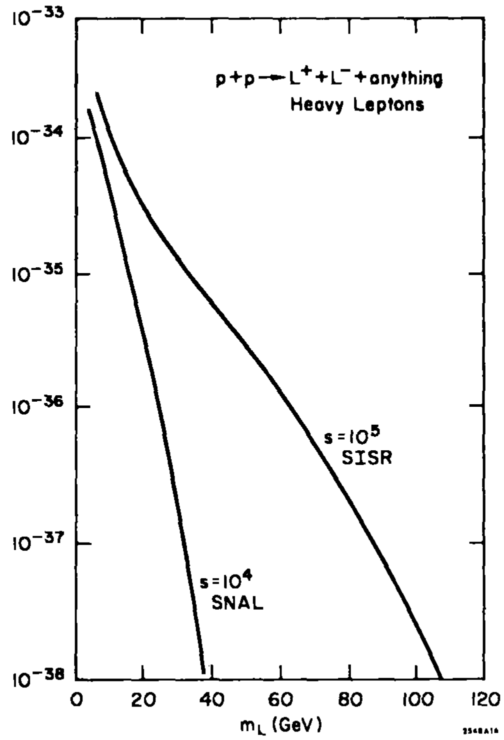

FIG. 20--Heavy lepton production as a function of lepton mass at Super NAL and Super ISR.

Figure 21 shows that, at $\sqrt{s}$ = 400 you have a signal which might be measurable out to beyond $Q^2 = (100\ \text{GeV})^2$. Remember that in number of events, one multiplies by 10

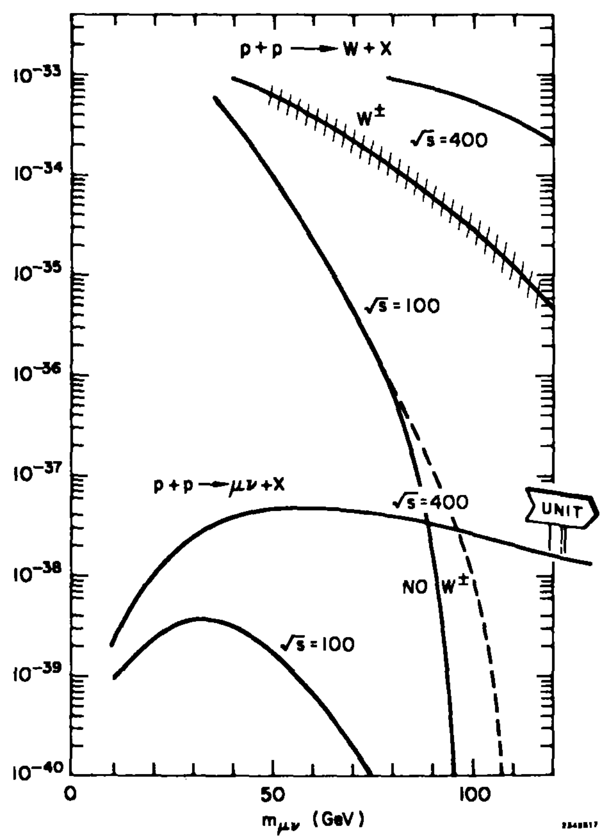

FIG. 21--Production of W boson or μ ν in proton-proton scattering.

if one takes a reasonable bite in Δm. So there are of the order of a hundred events per day, and one can track this thing out and answer one of the crucial questions of weak interactions as to what happens at very, very high values of the effective lepton mass. Strictly, there is no unitarity limit in this reaction. On the other hand, one knows that for a four fermion interaction at a center of mass energy of some hundreds of GeV, unitarity breaks down. Whatever physics intercedes should show up in a reaction of this kind at high masses. That is the force of this kind of research. All of this is to say that ISR's may be useful for weak interactions. They may even be useful for such esoteric things as neutral currents, e.g., interference between virtual Z^0 and γ's to produce lepton pairs. We know NAL type machines can also study these questions, but not much beyond ~50 GeV. Experimentally, there is reasonable hope that backgrounds of ~100 GeV/c transverse momentum objects from non-weak sources will be manageable. Missing transverse momentum signalling a neutrino will also be helpful.

We now go to the report card about secondary beams. There is simply an enormous amount of stuff that still wants to be done at the next level of energies. For example (see Fig. 22), elastic scattering of secondary particles, total cross sections, hyperon interactions, high $P_\perp$ events for not

1. π, K, $\bar{p}$ elastic
2. π, K, $\bar{p}$ total
3. hyperon-nucleon scattering
4. high $P_\perp$ for $\pi p \to$ hadron + anything
5. resonances $Kp \to$ hadron + anything — $\sqrt{s} \sim 80$ GeV
6. K^0 physics $pp \to$ hadron + anything
7. muon beams $\to 10^7$/sec up to 3 TeV
   μp scattering
   Brems. (QED)
   μ^* Prod.
   Universality etc.
8. electron beams $\to$ prob. similar
9. γ-beams, even polarized!
10. neutrino beams $E_\nu \to 2$ TeV
    very high σ will permit
    more civilized detectors
    But: shielding! νp
    νe!

FIG. 22--Secondary beam possibilities at Super NAL.

only protons but also the other particles because the quantum number effects may be revealing. The resonances are things which have to be pursued and which with storage rings will be very difficult. There is K^0 physics, and maybe some elucidation of the CP problem, maybe that we are stuck with K mesons and have to do more experiments of that kind. Then going to electromagnetic interactions there are photon beams of interesting intensity and these may even be polarized by crystal techniques. There are muon beams of the order of 10^7/sec, up to perhaps 3 TeV. Now that should be a very beautiful thing for scattering, for tests of QED, at center of mass energies of the order of 100 GeV, for perhaps searches for heavy leptons, and tests of μe universality. And for some sort of clue to the Bjorken formula we saw before. Presumably at these accelerators you could also make electron beams, like muon beams, with similar intensities. Then, of course, in weak interactions there are neutrino beams, and to have a 2 or 3 TeV neutrino beam with the consequently much higher cross sections might permit more civilized detectors, detectors which may not be so big. There is, of course, the problem of shielding, which I have not solved with these high energy machines, but somebody will solve them.

Let me discuss electron-proton scattering. I did say that ep can be done two ways — storage mechanism or via conventional accelerator. One usually looks at a kinematic domain, q^2 vs ν where we compare a 15 GeV e against 200 GeV p storage ring solution to the problem, with muon beams of 3 TeV. Using 10^7 muons and a reasonable target I get a muon luminosity of 5×10^{33}, roughly get a factor of 50 improvement over storage ring in luminosity. Off hand, in Fig. 23, it looks as if a very large kinematic range is

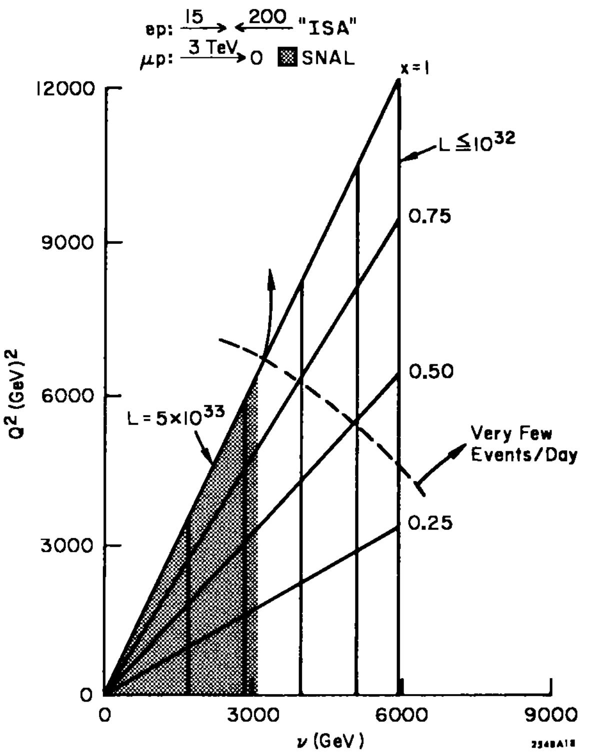

FIG. 23--Availalbe ν (GeV) - Q^2 (GeV^2) regions in lepton-proton scattering: comparison of ep storage ring (ISA) and μp conventional accelerator (Super NAL).

exposed by ep storage rings. On the other hand, if one looks at the numbers, one finds that in a large part of this plot the number of events per day is very small. So if one says, "Well, one can do good physics with a very small number of events per day", then of course the whole thing is relevant. If one says, "You really cannot learn anything very incisive from a very small number of events per day", then the two experiments look more competitive, with greater flexibility, e.g. in secondary particle detection, going to the muon approach.

I briefly considered alternatives to the e^+e^- storage question and could only come up with the "Fermi accelerator" Fig. 24 to compete with 15 GeV × 15 GeV e^+e^-. Conventional magnets are used. Rumor has it that Wilson wants the real estate for 100 kG superconducting magnets. This way he would feel challenged. For storage rings of course.

My conclusions are that a ~10 year program must include both proton storage rings and a fixed target proton accelerator. The graphs here indicate that a good match would have the Super ISR with ~6 -10 times the $\sqrt{s}$ as

FIG. 24

a "conventional" Super-NAL, that is, the example I chose finds the two machines too closely matched. Costs become a crucial matter. We need e^+e^-, and probably in the form of storage rings. As NAL and ISR develop, in the next few years we will see how sound the comparisons made here turn out. I hope that in the next few years, also, that we will find enough things to shake what I thought was the great complacency of Gell-Mann in saying that he sees in the air the ultimate theory. I hope we can spoil that. We will surely try.

DISCUSSION

Murray Gell-Mann, (California Institute of Technology): Can you give some idea of the cost?

Lederman: You want to know how much a 5 TeV accelerator will cost. I have no idea. I don't think a reasonable estimate is possible. I can give you a number that no one can prove that's impossible, a number like 200 million dollars can not be proven an impossible number.