Fermi National Accelerator Laboratory

FERMILAB-Conf-91/27

Where Do We Go From Here? *

Leon M. Lederman
Fermi National Accelerator Laboratory
P.O. Box 500
Batavia, Illinois 60510
and
University of Chicago
5640 Ellis Avenue
Chicago, Illinois 60637

December 1990

* Presented at the Yoshio Nishina Centennial, Evolutional Trends in Physical Science, Tokyo, Japan, December 5-7, 1990.

Operated by Universities Research Association Inc. under contract with the United States Department of Energy

Fermilab Conf. 91/27

Yoshio Nishina Centennial
Evolutional Trends in Physical Science
Tokyo, Japan - 5-7 Dec 1990

WHERE DO WE GO FROM HERE?

Leon M. Lederman

University of Chicago, 5640 Ellis Avenue, Chicago, IL 60637
Fermilab, P.O. Box 500, Batavia, IL 60510

1. Where are We?

1.1 Introduction

Of course we are in Tokyo, celebrating the 100th anniversary of Yoshio Nishina, a scholar whose activities encompassed so many different fields.

Among the many accomplishments of Yoshio Nishina we must remember that accelerator science was one of his major interests and he directed one of the foremost accelerator labs in the world up until the war.

In this talk I will summarize where we are, emphasizing those aspects of both theory and experimental science which are likely, in my opinion, to be springboards into the future. Unlike Nishina, I will stick to high energy particle physics although the guidance and strong influence of cosmology must of course be included. If you notice that I spend more time on experimental facilities than on the prospects for superstring theory, it is only that I truly believe the road to the future as we now dimly see it, is more likely to require new machines and new detectors than improved mastery of Calabi-Yau manifolds. Of course, we are inherently guided by theory and where we are going will very likely have the same felicitous blend of theory and experiment as we enjoy now. One thing about the future compared to the present is that it is undoubtedly longer.

1.2. Theory

We begin our springboard survey with a reminder that we live in the shadow of an incomplete Standard Model. This teaches us that the matter in the world is made up of six quarks and six leptons. In each family there is a missing member. In both cases, the absent particle has a very special role and both particles, the as-yet-undiscovered top quark and the not-yet detected tau neutrino will in fact play prominent roles in our future. For now the puzzle has to do with why the top mass is so heavy, (it is at least 90 GeV according to Fermilab results) and whether the tau neutrino has any mass at all and if so, is it enough to make the expansion parameter of the universe $W = 1$?

These mass puzzles extend over all the matter particles in the entire standard model and, are a 1990 version of Richard Feynman's 1950 question: "Why does the muon weigh?"

To complete my description of the Standard Model, the matter particles are beholden to the electroweak and the strong force. These are represented by 12 gauge bosons. Here too something is missing and again it is related to masses.

The unitarity crisis required the introduction of a new interaction carried by a neutral scalar particle, the Higgs. This field has the added feature of being capable of mass generation, giving the Z^0 a large mass and thereby breaking the symmetry in the electroweak interaction. It seems likely, if I understand what my theory colleagues are saying, that all fermion masses are generated as potential energy in the Higgs field. Well, if true, Higgs is crucial to any advance and we must try to find Higgs particles. The Higgs mass is an open parameter of the SM and here again we have an important (and very expensive!) springboard to where we are going. Fortunately there is a clue in that the theory becomes inconsistent (Higgs-Higgs scattering etc.) unless the mass of the Higgs is less than 1 TeV or so. This limit motivated the design of the SSC, the 40 TeV proton-proton collider now under construction in Texas.

I will select just one more of the questions left open by the SM and that has to do with CP violation, the ability of neutral K-mesons (the K-Long) to decay to 2 pions. This reaction has vast cosmological implications, nothing less than the "origin of matter." There has been a tremendous experimental effort to measure CP violating parameters and these will surely continue but the more recent possibility of studying CP violation in the B-meson ($b\bar{d}, b\bar{s}$) system has spurred proposals for the construction of machines specifically designed to do these things. These are usually called Beauty factories.

Finally, we must realize that the story of particle physics is a mixture of futures; futures motivated by theoretical crises and predictions and futures motivated by experimental and technological opportunities. For example, the $\pi, \bar{p}, \nu_\mu, K^\circ_L, W, Z,$ predictions lead to searches, new accelerators and new techniques, the $\mu, K^\pm, \Lambda^\circ, CP, J/\psi, \Upsilon, \tau$ were surprises, gifts of new techniques and of machines. If history is a guide, we will use our increasing powers of observation and measurement to test today's theory but also to search for new phenomena.

1.3 Facilities

The inventory of front-line machines is a decreasing function of time. In 1990, the Fermilab Tevatron provides nearly 2 TeV for $p\bar{p}$ collisions at a luminosity which permits observation of 10^5 collisions per second. It is likely that this will be increased by a factor of 50 or so by the mid-1990's. Such a luminosity (10^{32} cm^{-2}sec^{-1}) could permit the observation of processes that have cross sections as small as 10^{-37}cm^2. The Fermilab collider is the only existing machine that can produce the top quark. The Fermilab fixed target program at 800-900 GeV also provides the highest energy collisions of a wide variety of primary, secondary and tertiary particles. If we are to see the tau neutrino, it will almost certainly be in the fixed target program at Fermilab.

At CERN there is a $p\bar{p}$ collider which pioneered the technique of creating intense antiproton sources and then head-on collisions of protons and antiprotons. This machine produced spectacular data: discovery of W and Z as well as "jets." However with an energy of 630 GeV, it is scheduled to close in 1991.

The CERN LEP machine, currently running at 50 GeV e^+ colliding with 50 GeV e^-, is a Z^0 factory.

The scheme of this 27 km circumference machine with its four large and sophisticated detectors is to study the decay modes of the Z as a probe of new physics and as a means of establishing important SM parameters with great precision. They are approaching 10^6 Z^o's and in a few years, perhaps as many as 10^7 Z^o's so that very rare decay processes can be seen. Also in the next few years this machine will go to a total energy of about 180 GeV in order to study WW, ZZ, WZ, and γW pair production processes.

The SLC machine at SLAC manages the same collisions as LEP but in an accelerator of innovative design using the SLAC linac to accelerate e^+ and e^- and bring them together in two semicircular tracks. The machine is an approach to a linear collider, a much studied configuration for producing much higher energy e^+e^- head-on collisions. Unfortunately, its luminosity is only a few percent of LEP.

The smaller e^+e^- machines at Cornell (CESR), KEK, DESY, SLAC are providing detailed data on SM properties with the CESR and DESY machines until now providing the bulk of the data on B^o mesons. Lower energy fixed target machines at BNL and CERN have very selective programs e.g. Brookhaven's study of very rare K-decays and CERN's precision measurements of CP violation and of neutrino scattering. The Beijing charm factory has recently entered the field and will continue the work carried out at SLAC's SPEAR. HERA, a unique e p collider (30 GeV e's x 800 GeV p's) is scheduled to turn on in 1991 and will provide both search and measurement data.

Finally, I would tell you a bit about the apparatus. We are today in a situation where groups of 200-500 physicists can, in 6-8 years, assemble collider detectors of impressive complexity, making use of data acquisition systems and computational power that rival the accelerators in cost and technical sophistication. Consider the CDF dectector at Fermilab. It looks at 10^5 (soon to be over 10^6) events per second, each with up to 100 tracks and about 10^4 bytes per track. This is 10^{11} bytes per second. An on-line system examines these events and by a process of sequential filtering, finally writes about 5 events per second to tape. This is the springboard to the supercollider or CERN's version, the LHC, where the problem grows to 10^{15} bytes per second!

We have no time to describe the quality of the data, the trajectory measurements, the precision track-origin locators (to $\pm 10\mu$), calorimetric energy measurements, etc.

2. Where Are We Going, (Part A)?

Let's review the selected SM weaknesses in order to trace these threads into the future. We discuss these in the context of presently available accelerators.

2.1 Top Quark

We already know that $M_t > 90$ GeV. My own puzzlement is illustrated by a new table of the Standard Model which I call the Lego SM plot (see Fig. 1). The diagram is designed to emphasize the puzzle of the massiveness of the top quark. The sensitivity of the search for the top quark depends on the energy of the colliding quarks (partons) and on the integrated number of collisions. The above limit was based upon about 10^{11} collisions or an integrated luminosity of 4.2 pb^{-1}. In the 1991 run of the TEVATRON collider, the CDF detectors will be joined by a new detector, DZERO. It is expected that each detector in the 1991 run will have an integrated luminosity of 20-30 pb^{-1} which enables the mass range of up to about 130 GeV to be searched. By 1996, given the upgrades Fermilab has proposed, the top will be found if its mass is < 250 GeV.

Theoretical consistency of data on B mesons, on the W mass (within the SM) leads to the conclusion that M_t< 250 GeV. This is because the top quark enters in radiative corrections to SM parameters. If the TEVATRON does not find the top quark, the SM is incorrect. (The Higgs thing also enters into this argument). The issue in the quest for the top quark is then to know the mass and to determine whether the huge mass is merely an accident or is it some signal (see Fig. 1) that top is special and its properties will tell us about the very nature of mass.

2.2 Beauty Meson Factories

We mentioned that all the data on B's comes from the e^+e^- machines. Although the hadronic production of b-quarks has a much greater cross-section, until very recently backgrounds have prevented competition. However, excellent mass resolution has enabled CDF to reconstruct B^o events and study the specific mode:

$B \rightarrow J/\psi + K$

It is expected that the next CDF run which will have a silicon vertex detector should collect about 100 times the number of B events. However, there is now a world-wide effort to design an e^+e^- beauty factory with work going on at KEK, SLAC, CERN, SIN, and NOVOSIBIRSK. The motivation is CP violation which promises to be very informative if seen in the $B^o \bar{B}^o$ pairs. B-factories are designed so that they can measure CP violation in a year's run.

Hadron machines hope to get in the game. The ratio of B production to total cross-section is only 10^{-6} (fixed target) or 10^{-3} (collider). The CDF B signal now has as many reconstructed B's as do the e^+e^- colliders. The evolving technology and ingenuity may well make this an interesting race, i.e. between existing hadron machines i.e. FNAL's collider <u>and</u> fixed target vs the e^+e^- machines, existing and proposed.

2.3 Neutrinos

Since Pauli's inspired speculation, neutrinos have continued to puzzle and lead physics to new ideas. Try to explain to a science writer that there is a particle that has no charge no radius and no mass but that it enables the sun to shine, to cool stars, and to distribute the heavy elements cooked in dying stars, throughout the universe! No mass? The limit on ν_e is about 10 ev, on ν_μ it is 200 KeV, the tau neutrino can be as heavy as 35 MeV.

The neutrino structure and especially the possibility of finite mass is one of the outstanding problems today and clearly a springboard to major research over the next decade. The current research had three motivations: (i) The famous solar neutrino problem (what depletes the flux of ν_e's?) (2) The dark matter problem, i.e. we need weakly interacting neutral particles with some mass (not too much!) and neutrinos are good candidates because they do exist; and (3) the width of the Z insists that a fourth generation neutrino, if it exists, must have a mass greater than 40 GeV.

A vigorous use of neutrinos as tools for studies of quark structures and weak interactions led to detectors of 1000 tons. The proposals now emerging involve higher intensity neutrino beams e.g. the Fermilab Main Injector machine which would increase the collider luminosity would also yield 10^{13} protons per second at

120 GeV and a superintense neutrino beam. They also involve more sensitive searches for neutrino oscillations and for the detection of the tau neutrino. Since we know least about ν_τ, it has been considered the most likely candidate for astrophysical dark matter. The question of whether the τ-neutrino has mass is crucial here. If it does have a mass, the mechanism that generates it is "....a window on the world beyond the SM." Some proposers insist that neutrino beams be aimed at detectors hundreds of kilometers away (long baseline oscillations).

Finally we should mention neutrino astronomy and solar neutrinos. We know there is an ambient flux of neutrinos from outside our solar system and even outside our galaxy. The detection of some 11 events from SN1987A in Toyama and Cleveland marked the first time non-electromagnetic signals have been received from outside the galaxy. Since γ -rays of PeV (10^{15}eV) have been detected, since these are generated by hadrons, these must almost certainly also generate neutrinos via hadronic weak interactions. Detecting TeV neutrinos would be a cosmological bonanza. The subtleties of solar neutrinos may indicate oscillations generated by fractions of an eV mass differences between neutrino species. These in turn could have a vast influence on the large scale structure of the universe.

In summary, we touch the problem of mass again with neutrinos since it is not easy or natural in the SM to generate mass for neutrinos. Thus oscillations or any direct way of observing ν–mass must require theoretical extensions beyond the Standard Model.

2.4 Higgs

We noted that the Higgs particle mass is an open parameter which can be as high as ~ 1 TeV. There are some theoretical estimates based upon an idea of Nambu by several authors [1] which is inspired by the massiveness of the top. These theorists attempt to make the "Higgs" a bound state of top and antitop. These models give specific predictions for the masses of the top and the Higgs, in the domain of 100-200 GeV. Whether or not the $p\bar{p}$ machine can find a 200 GeV Higgs is an open question and depends critically on the luminosity of the improved TEVATRON.

3. Where Are We Going; (Part B)

This history has gotten off to a lively start. SSC was "conceived" in the late 1970 ICFA studies but it was brought to a sharp focus as a national plan in 1982. By July, 1983, it was embraced by the DOE and there began a serious design study under M. Tigner at the LBL headquarters of the SSC Design Group. The energy is 20 TeV in each beam yielding a splendidly violent 40 TeV in the CM with a collision rate of 10^8/sec.

Magnet R&D aimed at SSC was diversified to three laboratories (LBL, FNAL, BNL) and did not break speed records.

In 1987, SSC became U.S. policy, the site was selected and the SSC Laboratory founded in Texas under Roy Schwitters. As of current writing, the cost estimate for the SSC "hovers~ between $7.8 billion and $8.3 billion."

So what is the scientific drive for SSC?

We start with the list that any Congressman is completely familiar with:

1. Higgs! Electroweak symmetry breaking and SM predict that the reaction: $H^o \rightarrow Z^oZ^o \rightarrow$ 4 leptons will be seen at SSC if the mass is less than 800 GeV.
2. Z's, W's, copious production in pairs
3. Top physics
4. SUSY searches
5. Compositeness, is the quark (electron) a point?
6. Strong W_LW_L scattering
7. B physics
8. New physics which "explains" CP, 3 generations, quark lepton symmetry etc.

There is an incredible literature on the physics at SSC, and/or its European version, LHC. At this time, "expressions of interest" running to hundreds of pages have been received. This confirms the notion that interest is worldwide. About 5 or 6 propose to build generic detectors which are modelled on the DO and CDF or UA 1, 2 style of "4π" do-everything detectors. One detector proposed by 837.5 authors is a 6π detector. Other expressions of interest vary from the ubiquitous logs physics, to fixed-target beauty research. So far, only one, perhaps two, seem to be based upon totally new technologies. The next few years will see a refinement of these expressions-of-interest.

One of the more challenging aspects of SSC experimentation has to do with the collision rate. The design luminosity would yield 10^8 interactions/sec, each interaction generates ~100 particles requiring ~10^7 bytes to describe. This data rate requires all kinds of new techniques, radiation-hard detectors and up-close electronics, a refined mechanism for selecting the interesting events, etc. Whereas very few experts would claim that this problem is now completely solved, there is nevertheless considerable pressure to go to 10 times this rate or even more! From the theoretical physics point of view it is clear that this would help the Higgs problem, but from the experimental point of view, it is not at all clear that 1990-1992 technology can deal with these kinds of data rates.

4. Where Are We Going; (Part C: Beyond SSC)

It is the 130th anniversary of Yoshio Nishina. The year is 2020.
So by now we can also invent SSC results, e.g.

H_1^0 = 422 GeV found at SSC in 2004

$H_2^0 \equiv$ 699 GeV but only 3σ

Indications exist that there is a Higgs sector with a rich Higgsian spectroscopy.
To study this, we obviously need higher energy.

SSC may instead discover a new class of strong interactions which may, in the words of Steven Weinberg, revive the physics of our youth; dispersion relations, Regge poles, sum rules, all at a much higher energy. Again we'll need a machine appropriate to the energy. To decide the state of hadron colliders, we are fortunate to have the well-tested Livingston Chart (Fig. 2). This predicts that by 2030, we will have 1000 TeV in the CM. In order not to violate this schedule, we must start in 2020. The dilemma facing us in 1990 is that we can't know now what kind of facility will be appropriate. Of course by 2020, we'll know!

4.1 Electrons vs Hadrons?

There is a segment of devotees of e^+e^- collisions that seem to hold to a belief that the next machine after SSC "belongs" to electrons and this is as sensible as if the experts on Geiger counters would insist that they be employed on the next detector. The point is that we are all driven by physics. Electron machines were powerful in the 1970's and LEP's contribution to Z^0 physics, especially the width, is clear. The virtue of electrons, their clean initial state, may however count for less and less as the violence increases. Very narrow resonances like the Z^0, strongly coupled to electromagnetism, is one of the few states that strongly favor e^+e^- machines and these may be a vanishing breed at post-SSC energies. If hadron colliders can solve the rate problems and the messiness of the spectator partons, its relative economy in dollars per GeV and its large variety of initial states may win over e^+e^- colliders in the next round. A strong indication of this does not have to wait for SSC results in the 2000's but will be guided by 10^{32} luminosity in the upgraded Tevatron in the mid-1990's. If constituent collisions continue to be as clearly discernible at these rates, it will be a strong indicator that a 500 TeV x 500 TeV pp machine can be the 2020 machine, rather than the equivalent 50 TeV x 50 TeV e^+e^-. We must keep our minds open and weigh the physics potential of these two approaches. Both have formidable challenges, the former is largely in cost reduction. In the e^+e^- case, the technical challenges are so daunting that it is likely that the only sensible approach is an iterative, learning process, through a, e.g., 200 GeV x 200 GeV collider, then a 1 TeV x 1 TeV, etc. Each process is in the billion dollar category and probably requires of the order of ten or more years. Thus some imaginative efforts at magnet R&D to reduce costs of the post-SSC accelerator should start in the period of 1995-2005. Progress in high temperature superconductors is clearly relevant.

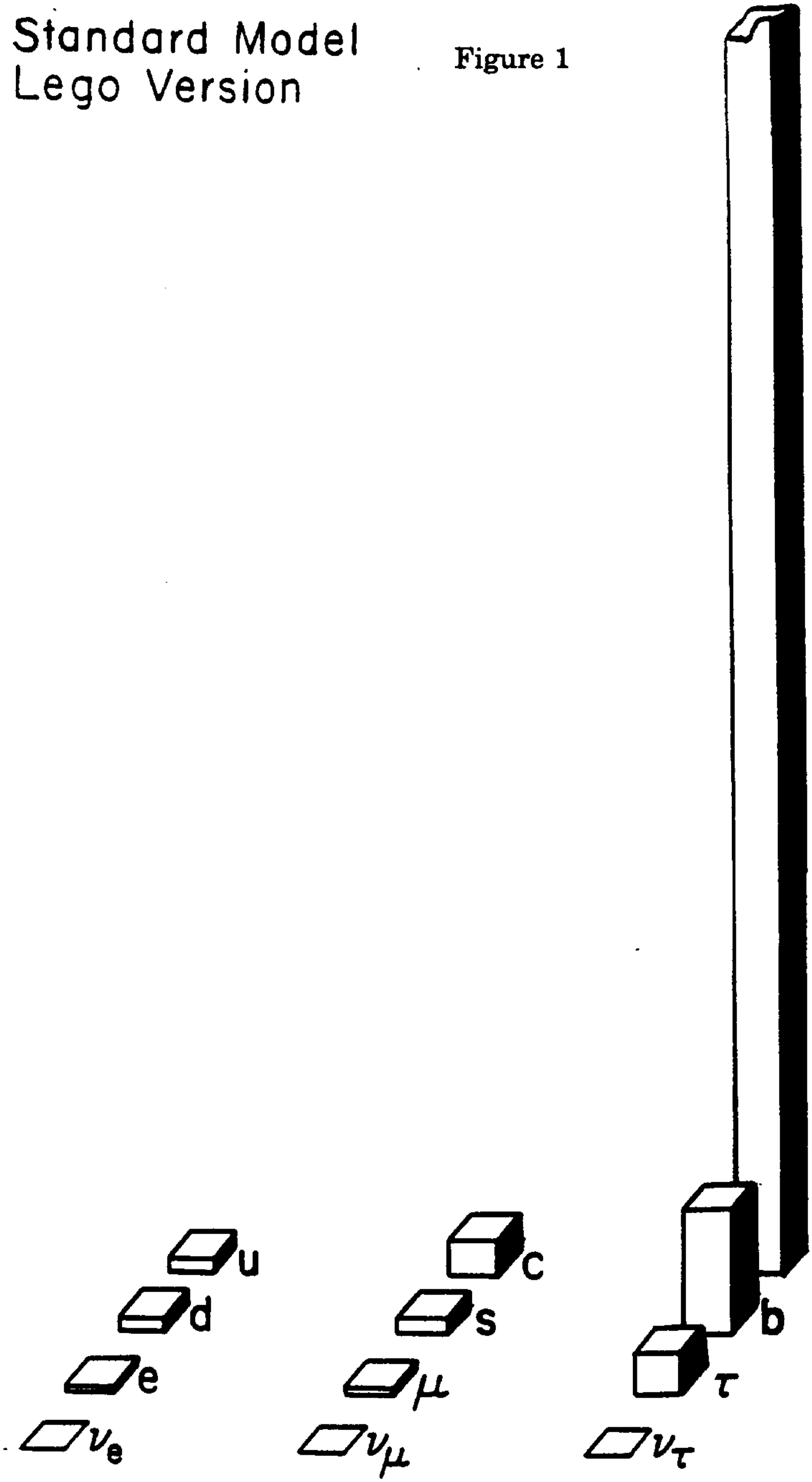
A design of a 500 TeV x 500 TeV machine was carried out in 1985 by J.D. Bjorken. The only daunting problem was the cost.

Some speculative theoretical ideas [2] in fact would strongly favor hadron accelerators in the hundreds of TeV range. These ideas are related to the notion that electroweak interactions become strong (non-perturbative) at high energies. Violations of B (baryon number) and L (lepton number) could be induced by new gauge fields (instantons). Observations of large probabilities of huge multiplicities in quark-quark collisions are possible outcomes of these ideas. What is involved is nothing less than the topological structure of the electroweak vacuum. So there! Both theoretical and experimental progress is needed before using these ideas as a decisive issue in this mythological next accelerator. However it does support the thesis that it is not at all certain that this will be an electron linear collider. It should be noted that in Europe, the "Eloisatron" concept of a multi hundred TeV hadron collider has been discussed by some of the more imaginative physicists for some years.

5. References

1. Y. Nambu, EFI 88-39, 1988
   Bardeen, Hill and Lindner, Phys Rev. D41, 1647 (1990
2. G. t'Hooft, Phys Rev. D14 (1976) 3432
   A. Ringwald, Nucl Phys B 330 (1990) 1
   S. S. Gerstein, Yu. F. Pirogov "Workshop on Physics at Future Accelerators" CERN 87-07 (1987)

Figure 1



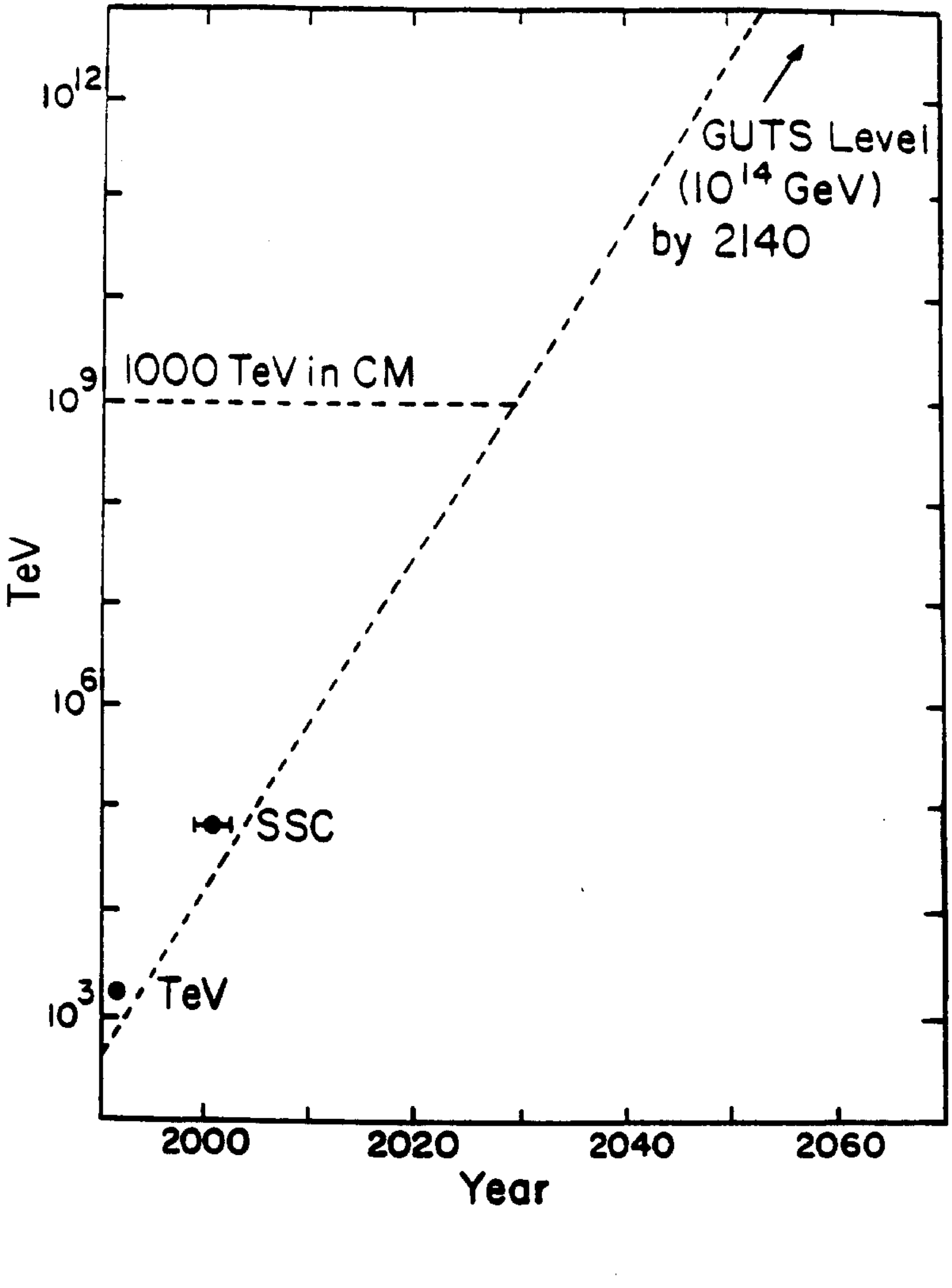


Figure 2. **Upper reaches of the Livingston Plot whose absolute validity is established in the off-side years 1930-1990.**