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Perspectives on Future High Energy Physics*

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In preparing this talk on perspectives on the future high energy physics, I was again reminded that one should be wary of attempting to forecast the future, if at all, one should do it in general terms. If one examines previous attempts, one finds very few accurate predictions. Indeed, some are notoriously bad. Lord Kelvin was reported to have stated in 1895 that heavier than air machines are impossible, in 1897 that radio has no future and in 1950 that x-rays are a hoax. Of equal inaccuracy is the quote attributable to Michaelson in 1898.

"While it is safe to affirm that the future of physical science has no marvels in store even more astonishing than those of the past, it seems probable that most of the grand underlying principles have been firmly established and that further advances are to be sought chiefly in the rigorous applications of these principles to all phenomena which come under our notice. An eminent physicist has remarked that the future tasks of physical science are to be looked for in the 6th place of decimals."

To take just one example from the recent past, L.C.L. Yuan edited in 1964 a compendium of essays on the Nature of Matter by a group of pre-eminent scientists. Although erudite and thoughtful in expounding in what had been accomplished as well as outlining many of the short term extrapolation of scientific endeavors--they all missed the extraordinary evolution of the standard model with its family of quarks, leptons, gluons and bosons. In our own day, the recent book by John Hazen, the End of Science, is another example of poor prognostication. In this case, he forecasts the end of everything, physics, cosmology, etc., but doesn't make the case. Indeed the tone is reminiscent of the Michaelson quote. In sum, be wary of predictions.

If that is so, how can one proceed? The answer is with caution and humility and in two general ways. The first is to utilize the state of knowledge in the field and thereby provide theoretical and experimental guidance on future directions. Such a brief synopsis is outlined in Table I enumerating the ingredients of the standard model. In essence, all observables are described by the model, however, there are ~ 17 constants that are not presently calculable.

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Table I. Standard Model

SU(3) x SU(2) x U(1)
Constituents -- 3 sets of doublets

Quarks

u	c	t	
d	s	b	Gluons

Leptons

ν_e	ν_τ	ν_τ	
e	μ	τ	γ, W, Z

Coupling α_i I = 1-3

K-M matrix

Electro-weak Symmetry Breaking

Higgs - Introduce 1 or 2 complex scalar bosons.

At least one Higgs boson at ϵ -m scale mass

Not fundamental

Low Energy Approximation

As such it is not fundamental theory but possibly a low energy approximation to a more global theory. What are the expectations? Some people have hypothesized a desert, from 10^2 - 10^4 GeV. If so, all possible next generation machines will produce nothing and this field will come to a hiatus, at least until some future data when someone will come up with a clever, clever idea for a super energy machine. A more likely scenario involves electro weak symmetry breaking by some mechanism, possibly one or more Higgs bosons. Predictions of masses for such particles range from 120 GeV up to 1,000 GeV with varying degrees of confidence. Even more exciting is the possibility of super-symmetry with multiple Higgs bosons and which is a natural consequence of string theory which unites all the forces with gravity. If this turns out to be a correct description of nature, then for every boson there is a fermion and vice versa. There should be many states to be found with masses less than 1,000 GeV. We can reasonably conclude that although we don't know which description is correct (although most theoreticians bet on super-symmetry), something should happen at a mass scale of 1,000 GeV. Therefore, exploration of the energy mass range of several TeV could be extremely desirable.

The second approach is technical, namely, how well can one do in going to higher energies with present techniques or new accelerator principles. This was the argument for the BNL (AGS), a new principle allowing for higher energies at lower cost, as well as for the SLAC complex, a large extrapolation of the known linear accelerator technique. In both cases the physics justification were far off the mark, nowhere is there even a hint of the major discoveries at the AGS (2v, J, CP) and

SLAC (Ψ , partons, τ). Excerpts from documents proposing the contribution of each of these projects are noted as follows.

AGS Justification

"...the Cosmotron has, during its relatively short operational use, yielded much new data on meson yields, and on the energy dependence of π meson and fast neutron cross sections and has even led to the observation of certain hitherto unobserved heavy meson phenomena. That extension of the available energy would yield many fruitful results seems unquestionable; indeed, it is already possible to visualize many useful experiments requiring considerably higher energies. Although many of these will be made possible by the 6 BeV soon to be available at the University of California Bevatron, still further extension seems highly desirable, for specific and predictable reasons as well as on the general grounds that past extensions of energy have always proved highly profitable."

Stanford Linear Electron Accelerator

"Any high-energy accelerator is useful in the following general categories: (1) as a controlled source of artificially-produced particles which are otherwise only produced in very low intensity from cosmic rays; (2) as a source of particles to explore nuclear structure and to study the interactions of these particles in nuclear matter; (3) as a tool to study the basic production process in the creation of particles. Added to these, in the case of electron accelerators, is: (4) as a tool to study the high-energy limits of quantum electrodynamics."

For assistance in devising a strategy for technical accelerator possibilities, we can look at the historical record, Fig. 1, the Blewett-Livingston plot where accelerator energy is plotted as function of calendar time for various types of accelerators. One notes the well known pattern of exponential increase over decades of time driven by new ideas. The demise of the SSC which was the logical next machine and its partial replacement by the LHC in 2004 is certainly a setback. Our present high energy complement of machines is the Tevatron at 1.8 TeV, the SLC at 100 GeV and LEP at 200 GeV, and HERA the only CP collider. These are supplemented by the AGS and CESR at the lower energies and with high intensities. There are certainly ideas and projects for going to the multi-TeV range technically. Further along are designs for e^+e^- linear colliders in the 0.5 TeV to 1 TeV energy range. A novel idea for $\mu^+\mu^-$ colliders has recently emerged and a vigorous R&D program is clearly warranted. In addition, there are prospects for a 60-100 TeV pp collider. All should be pursued. What we should not do is duplicate machines. Recall the saga of PEP, PETRA, and TRISTAN all chasing the top quark which originally was expected at 15 GeV and emerged at 170 GeV.

Whatever emerges is going to have constraints, political and economic. Lessons learned from the demise of the SSC would serve us well. There are numerous reasons for the lack of success in this project among which were the lack of 100%

Figure 1. Blewett-Livingston Plot

support of the U.S. high energy community, similar reluctance on the part of the U.S. physics community, notably those in condensed matter, the confusing signals of the international community vis a vis the LHC, the competition of NASA projects especially in Houston, Texas, and finally, the luke-warm endorsement by the AAUP President contingent on no reductions in any other areas of science for support of the SSC. As a consequence, the project was lost, the logical game plan destroyed and the money, \$600M per year, disappeared.

What is to be done. First one must stabilize U.S. science funding for all of science and in particular for high energy physics. There has been some recent success, in that the previous downward budget projections for DOE have been revised to flat. The high energy budget in FY'97 has two construction projects underway, the main injector at Fermilab and the B factory at SLAC totaling $\approx$

\$100M which goes to zero in FY'99. We must retain this money in the high energy program. In order to improve the budget situation for science, it is imperative that all of science speak in one voice, chemists, biologists, physicists, medical people, etc. And in doing this, we must do a better job of communicating to the public and the Congress the importance and value of what we do.

The future strategy is straightforward. We must incrementally upgrade and run our present accelerator facilities producing exciting and forefront research. We are pressing on several frontiers with protons, electrons, in energy and intensity constantly testing the standard model. At the same time, we should sharpen our theoretical tools both extrapolating from lower energies (100 GeV) to high (multi TeV) and vice versa. The U.S. should be involved in the LHC, both in the accelerator and experimental areas as is presently proposed and planned. We should have an extensive R&D program on accelerators for a multi-TeV capability, emphasizing e^+e^- and $\mu^+\mu^-$ colliders. Finally, we should maintain and strengthen our international cooperative activities for it appears that scale and costs dictate that the next multi-TeV machine will be truly international.

I close by quoting an observation by Julian Schwinger on the future of fundamental physics.

"And one should not overlook how fruitful a decision to curtail the continued development of an essential element to the society can be. By the 15th century the Chinese had developed a mastery of ocean voyaging for beyond anything existing in Europe. Then in an abrupt change of intellectual climate, the insular party at court took control. The great ships were burnt and the crews disbanded. It was in these years that small Portugese ships rounded the Cape of Good Hope."

This should provide a lesson for all of us.

Appendix

For completeness I have added an appendix briefly commenting on the high energy experimental program at the AGS at BNL. This utilizes the high intense proton beam, 6×10^{13} protons per pulse allowing for the exploration of very rare processes in K decays and the anomolous magnetic moment of the muon. These are listed in Table II where Group A involves ongoing experiments and Group B proposed experiments. One notes present sensitivities of 10^{-10} to 10^{12} . Such measurements are relevant to parameters of K-M matrix and particle masses of 100 TeV coupling to μe .

The proposed experiments increase this sensitivity especially in the μ to e scattering, which is relevant to SU SY particles. The neutral K^0 decay is a direct measurement of CP and the others either look for effects beyond the standard model or measure a K-M matrix parameters. This work complements that from other labs especially the B factories.

BNL has a long-range plan in high energy and nuclear physics and this is

graphically displayed in Table III. The vertical axis is proportional to funding levels and the horizontal axis is calendar time. As is evident, there are many activities starting with the ongoing AGS program whose content was previously outlined. By

Table II

Group A

$$K^+ \rightarrow \pi^+ \nu \nu \quad BR \sim 1 \times 10^{-10}$$

$$|V_{td}| \sim 15-25\%$$

$$(K^+ \rightarrow \pi^+ \gamma \gamma, K^+ \rightarrow \pi^+ \mu^+ \mu^-, K^+ \rightarrow \mu^+ \nu \gamma)$$

$$K^+ \rightarrow \pi^+ \mu e \quad 3 \times 10^{-12} \text{ goal.}$$

$$\text{Also, } K^+ \rightarrow \pi^+ \mu^+ \mu^-, \pi^+ e \nu$$

$$K^+ \rightarrow \mu e \quad 8 \times 10^{-13} \text{ goal. (Current } 2 \times 10^{-11})$$

$$g-2 \quad \Delta a_\mu \approx 20 \times 10^{-11}.$$

Group B

$$K^0 \rightarrow \pi^0 \nu \nu \quad 10^{-12} \text{ sensitivity.}$$

$$\text{Direct test of CP } (\eta) \quad (V_{td}^* V_{ts})$$

$$K^+ \rightarrow \pi^0 \mu^+ \nu \quad \text{Transverse Pol. of } \mu. \quad P_\mu^t \approx 1.3 \times 10^{-4}.$$

$$K^+ \rightarrow \pi^0 e^+ \nu \quad |V_{us}|, \lambda \pm 0.35 \% \text{ Cabibbo.}$$

$$\mu^- Ae \rightarrow e^- Ae \quad 10^{-16} \text{ sensitivity. LOI}$$

Above sensitivity to SUSY.

FY'00, RHIC is operational with a full heavy ion program and four detectors. The potential for discoveries with this new facility are great, centered around the creation of the quark gluon plasma, the restoration of chiral symmetry and the observation of the excited vacuum. The possibility of augmenting this program with polarized pp interactions is now assured with funding from Japan via RIKEN and should take place in the early years of operation. As also noted, there is a commensurate reduction in the AGS program in FY'00. Since RHIC is paying for full operation

for the complex, the high energy physics program pays incremental costs for its implementation. As such, an AGS 2000 program has been formulated, a set of unique, crucial, and competitive experiments. The extent of this program will be reviewed by DOE in the next year.

Table III. BNL Long-Range Plan

For completeness, we note experimental participation in both the D0 detector at the TEVATRON and the ATLAS detector at the LHC at CERN. The D0 participation has been longstanding and will continue with BNL being major contributors to the calorimetry and computing. With respect to ATLAS, BNL is again a major player and in this instance will be the center for the U.S. segment of the collaboration.

The rather extensive accelerator R&D activities are also noted. These involve a Center for Accelerator Physics with the Accelerator Test Facility which has been in existence for ≈ 10 years. This provides a facility for advanced accelerator concept experimentation with three beam lines and user groups from academia, university and other national labs. The BNL participation in the LHC accelerator is also noted with the main responsibility being the dipole magnets for the insertion areas as well as the short sample measurement of all the superconducting cable. The last entry is the muon collider R&D. This effort led by Bob Palmer addresses the issue of reaching multi TeV energies in the constituent reference frame. For a variety of reasons, the use of muons as the colliding particle is very attractive. The lack of

beam strahlung on the part of the muon at high energies (> 1 TeV) in contrast to electrons is also an advantage. Much R&D effort has to be done to ascertain that such a collider is feasible and cost effective. BNL is actively pursuing this approach with Fermilab and LBL. This may supply the answer to Rabi's query on who ordered the muon--to make a muon collider.

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