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LEDERMAN'S SHOULDER, WEINBERG'S NOSE, AND OTHER LESSONS FROM THE PAST*

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Planning for discovery is both absolutely necessary and fundamentally silly. We can't know what will be. However, we can look back. The unexpected has come sometimes at the highest energy frontier, such as scaling at SLAC, and sometimes in a careful look over old ground, such as CP violation or the heavy vector meson of Christenson et al. Whatever the current theoretical beliefs, our future plans should not stifle the possibility of discovery. This is where, I think, D0 has a role to play.

Three issues came to mind regarding D0 experiments: 1) far more money and luminosity are available at B0; 2) the CERN collider is already running; and 3) most theorists regard 2 TeV as a factor of 10^{13} too low to do anything really interesting. But before being discouraged one should consider: 1) The B0 detector is a mammoth, all-purpose compromise, designed to see what is expected. It will either extend the Monte Carlo calculations one more decade (especially after they have been tuned to the CERN data) or discover the obvious. 2) Regarding the specialized detectors that should go into D0, data coming from CERN can only help in optimizing designs and suggesting new directions. The Crystal Ball is an outstanding example of the power of a highly specialized detector, and it only benefited from information provided by nominally competitive experiments. And 3) we have never in the past proven to be nearly as smart as we thought, so why should now be different?

These are certainly exciting times in theoretical physics. Buoyed by the successes of the "standard" model, we have tried to apply the same ideas to an ever wider range of phenomena. But as we attempt to answer certain questions, yet others come embarrassingly into sharper focus.

Gravity and early cosmology have become active areas of particle physics research. Yet the quantum mechanics of gravity itself remains a mystery. The explosive new scenarios for the Big Bang address several old puzzles regarding what was thought to be an excessive homogeneity of the observed universe. However, they rest heavily on the effects of what Einstein called the cosmological constant. All theoretical estimates of this constant are larger than the observed upper limit by a factor of 10^{120}. The only plausible resolution of this dilemma that I have heard so

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*Synopsis of a talk presented at the D0 Workshop, Fermilab, November 19, 1982 far relies mostly on the fact that we really don't know what we're talking about.

Supersymmetries have stirred much excitement because they are the only purely group-theoretic way to relate particles of different spin. Undaunted by the fact that none of the particles we know are so related, theorists posit a supergap: the splitting in mass between particles we know and their superpartners. Popular supergaps are on the scale of GeV, TeV, or 10^{19} GeV. Not coincidentally, these are the natural scales of strong, weak, and gravitational interactions, respectively. The paucity of alternative suggestions reflects our ignorance of intermediate scales rather than their impossibility.

The suggestion of unification of strong, weak, and electromagnetic forces not only stimulated the current round of proton decay and neutrino mass experiments; it also implied one spectacularly successful prediction: it gave the Weinberg angle right on the nose. I regard this as a stunning triumph.

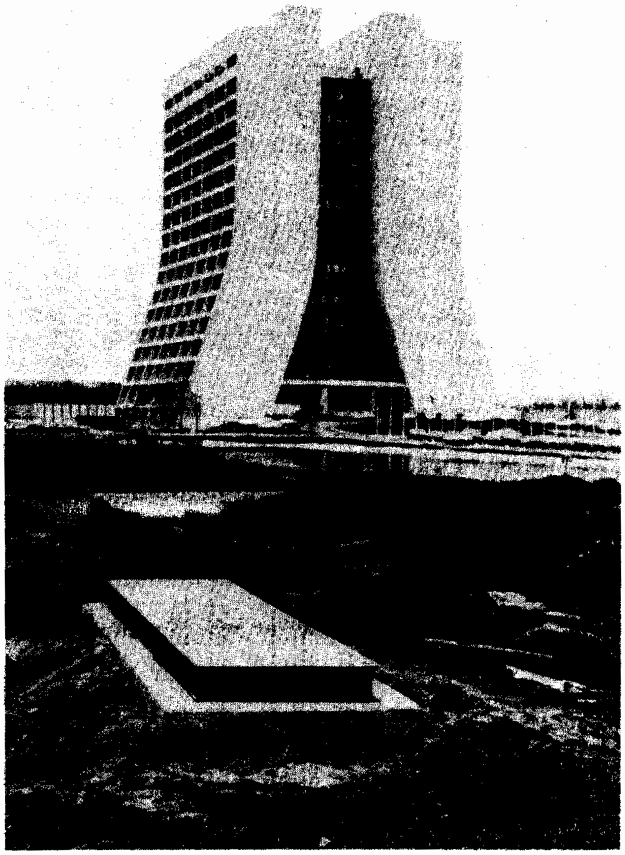
We have known for decades that something interesting must happen around a TeV because of the unitarity bound on weak interactions. While much has been learned in the past decade about the structure of the weak currents, we are still ignorant of the details of how their forces are transmitted over the relevant range of 10^{-16} cm. Alternatives to the Weinberg-Salam model are based on the observation that all other short-range forces we know are short ranged precisely because of the composite nature of the participants. So physics at 2 TeV may well reveal composite quarks and composite would-be W bosons. An indication that there is something to the weak interactions that we just don't understand is the existence of the approximate symmetry that makes $m_W/m_Z \cos\theta_W$ nearly 1. Exact symmetries are simply symmetries, but approximate symmetries have always been a clue to something interesting happening on a deeper level.

In the realm of pure hadron physics, if we had a basic understanding of at least some aspect of QCD, then experiments of confusing complexity would be pointless. But, in all honesty, our confidence in QCD is not based on any successful application of the theory to understand how something works. Rather, all we know directly from theory alone is that the QCD interaction strength vanishes at short distances. QCD is unique in this aspect and is, therefore, the only plausible explanation for some of the successes of the parton model. Virtually all other applications of QCD rest heavily on phenomenological inputs that we gleaned from experiment, with theorists having only the faintest glimmer of how these observed properties might follow from the basic equations. The reason it has proved so hard to "test" QCD is that our predictions are so shoddy. But even if one were convinced of the correctness of QCD, more experiments are needed to help us figure out how it works--to search for new phenomena and to understand how other gauge theories might work.

The list of what we don't understand is endless. We still debate the correctness of the Drell-Yan picture of hard hadron collisions and likewise the nature of high-energy elastic scattering. We do not understand how individual hadrons (save perhaps heavionia) are composed of quarks. Is there, for instance, any validity to a naive, non-relativistic constituent picture? Why do total cross sections rise as they do? How are individual hadrons produced and in what relative proportions?

Hadron jets at 2 TeV will be spectacular. In analyzing them, the uncertainties implicit in our "hadronization" models will definitely be somewhat less important than they are in current e^+e^- physics. But other uncertainties in the theory will come to the fore instead. Even for arbitrarily high energies, we do not know the structure of jets inside cones of some fixed (small) angle.

These are all subjects of active theoretical study, but any new information from the experimental side would be more than welcome. I firmly believe that anything that can be measured well is worth doing.



Completed construction for 1 TeV in the Switchyard area.
(Photograph by Fermilab Photo Unit)