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Remarks

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Thank you for inviting me to speak today. I was pleased to hear the good results just now from the CDF and D0 detector collaborations. The D0 project started when I was president at Stony Brook, and I am proud of the role the university played in its success. These are world-class instruments at a world-class facility. I look forward to reports from the runs at higher luminosity, and especially those that further constrain the Higgs mass.

The first message I want to convey today is one of congratulations on working at these fine facilities at the highest energy particle accelerator now operating anywhere in the world. These facilities, including the Tevatron and its complex of accelerators and detectors, are central to the nation's leadership in fundamental physics. You are witnessing phenomena in the highest energy physics experiments ever conducted by human beings.

High energy physics today stands at the threshold of profound new discoveries. Forty years ago, when I was a beginning graduate student, the field seemed very confusing. New particles and resonances were being discovered with each new accelerator energy upgrade. The first successful ideas about hidden or broken symmetry were beginning to bear fruit, but of course the SU(3) of those days was not color SU(3). That emerged later to organize the hadrons and produce the strong interaction through the marvelous mechanism of local non-Abelian gauge invariance. We had no idea at the time that gauge field theory would work, or indeed if field theory was even the right framework for understanding matter. A decade later the dust began to settle, the outlines of the Standard Model appeared, and for the next quarter of a century, experiments hastened to catch up with theory rather than the other way around. In the early 1960's, no one could have predicted that by the end of the century we would be looking for minute deviations from a Standard Model that rested on deep field theoretic foundations.

And yet there are some not so minute issues to be resolved. Apart from the puzzle of the Higgs mechanism – essential to the theory but itself an enigma – there are the recent discoveries of neutrino mass, and a yawning gap between the cosmology inferred from deep space observations and the fundamental set of fields we understand today. Unambiguous traces of dark matter, lots of it, and the apparent need for an enormous quantity of "dark energy," plus the existence of ordinary matter in amounts exceeding what we can explain by the Standard Model, all add up to an embarrassing ignorance of the substance of the universe. Apparently we humans are living in the fine structure of the ground state of a stupendous system of condensed matter dominated by fields that we have not yet identified. The success of the Standard Model, however, gives us confidence that field theory is a good framework for the next stage of discovery.

The next features we observe in the high energy spectrum will provide more new insight into the fundamental constituents of matter than all the discoveries of the past twenty five years. They will give us badly needed signposts to the correct path along which the Standard Model should be extended. New tools are emerging, in theory, in computation, and in instrumentation, that will allow us to extract maximum information from the hard-won data you and your colleagues are collecting at Fermilab today, and will collect in the future from CERN's Large Hadron Collider when it begins to operate later in this decade.

My second message this afternoon is that these exceptional opportunities for high energy physics are occurring simultaneously with other profound changes in science. The same advances in computing and instrumentation that have been important for your field are having a profound effect on all other fields of science. The science of phenomena dominated by the electromagnetic force, in particular, is undergoing a revolution. For the first time, it is possible to image, analyze, simulate, and manipulate ordinary matter at the atomic level. This is the domain of nanotechnology and biotechnology, "photonics" and "spintronics." It encompasses exciting efforts to extend quantum coherence to macroscopic scale, to exploit the quantum properties of entire atoms, and to prepare bulk materials with physical characteristics that would never occur through the ordinary processes of nature. This is also the domain in which structures of astounding complexity emerge, and its exploration requires the ability to store, analyze, and visualize very large amounts of data. This new domain of complexity has its own new frontiers, new paradigms, and new social structures within the scientific community.

Two important aspects of these emerging opportunities elsewhere in science are important for the future of particle physics. First, some of the new capabilities require investments in apparatus on a scale that formerly occurred only for high energy and space physics. Thus there are new competitors on the scene for large scale, expensive facilities. These include intense photon sources based on electron accelerators, intense neutron sources such as the Spallation Neutron Source at Oak Ridge, scanning electron microscopy, high field NMR devices, and specialized super-computing facilities. Second, the science opportunities created by these facilities are also fundamental, exciting, and demonstrably of greater relevance to human-scale issues than particle physics or astronomy. The phenomena they deal with are closely linked to the technologies important for national issues such as health care and economic competitiveness. They are important for homeland and national security. In short, they deserve, and are likely to receive, high priority for funding even in an era of tight budgets.

The opportunities in high energy physics have increased, not diminished, in importance during the past decade. But at the same time the opportunities in these other very attractive fields of science are also increasing, and very rapidly. What this suggests to me is that federal budgets for high energy physics are not likely to grow substantially faster than in the past. The United States is investing approximately $800 million per year in high energy physics research, and slowly increasing. I do not think the rate of increase is enough to satisfy the current appetite for big projects in this field, including new accelerators, neutrino detectors, and space-borne observations. Some of these projects would seem to have very high scientific payoffs. Which ones? We have to answer that question quickly and carefully because there is danger of saturating the available budget with lower priority activities.

The conclusion I draw from these observations is that there is a need for a new emphasis on, and perhaps even a redefinition of, strategic planning in high energy physics. As a first principle of planning, machines and instrumentation must be subordinated to a broader view of the field. Priorities for projects need to be justified by the expected science payoff in breadth and/or depth of discovery potential. Justifying accelerator construction on the basis of technology spinoffs has become a weak argument, in view of the much greater relevance to technology of the new bio-, nano- or complexity-oriented fields. By far the strongest argument for pursuing high energy physics is the human imperative to discover the nature of the physical universe. Even the discovery of the Higgs is not an adequate justification. It is the value that observing the Higgs adds for the elucidation of the whole picture that is important. If we did not have something like the Standard Model to provide meaning, or at least the conviction that something like the Standard Model would be discovered, then the continued search for ever higher energy excitations would be pointless. Now we have something even more exciting than the Standard Model. We have a set of cosmological mysteries including inflation, dark matter, dark energy, and matter-antimatter asymmetry, all of which must be related in some way to a bigger picture of which field theory and the Standard Model are a part. Choices of what activity to fund need to be related to their impact on filling in this picture. Theory, and a wide variety of experimental approaches all need to be evaluated together in this context.

A second principle of strategic planning must be to acknowledge the impact of one area upon another. Expensive projects in one field definitely affect the chances of support for other fields, or for other less expensive activities in the same field. A rational science policy considers the complementary capabilities of agencies, and seeks to avoid duplication. This requires comparing the big science programs in NASA, NSF, and DOE. Two years ago, the Office of Management and Budget required NASA and NSF to coordinate their planning for big telescopes, and not to treat space-based and land-based telescopes as two entirely separate species. The result was the National Astronomy and Astrophysics Advisory Committee (NAAAC) now embedded in the language of the 2002 NSF reauthorization bill. In my opinion, the Department of Energy should be included in this committee, and its purview should include all the means of astronomical observation, including photons, neutrinos and gravitons. It makes no sense for DOE to be building space-borne instrumentation designed to probe the mystery of dark energy, for example, without strong coordination with NASA. Nor does it make sense for NASA to be flying space-based experiments relevant to particle physics without strong coordination with DOE. NSF and DOE currently draw on HEPAP expertise for program guidance. NASA should too. Without strong evidence of coordinated planning and execution, the forces of entropy, ever present in our complex federal funding process, will erode the quality of our enterprise, and move the horizon of discovery in the wrong direction.

A third important component of a new approach to strategic planning is the international dimension. Fundamental physics has always been an international activity, and it will continue to be so in the future. In the immediate postwar period, particle accelerators of very similar capabilities were built at Lawrence Berkeley Lab, Brookhaven, and CERN, and there was a strong and healthy sense of competition, despite the multinational composition of the scientist teams who built and used them. Today each developed nation understands the need to invest in the sciences that undergird their technologically intensive economies. Their choices of how to make that investment are influenced by the same forces I described earlier. One consequence of this that I foresee is that there will be less duplication of large facilities devoted to high energy physics, and more equal sharing of the burden of building and operating these facilities among nations. I am saying this based on the magnitude of the costs of projects of which I am aware. I think it will be difficult to find a host nation in the future for a NLC-scale device if the host is expected to pay the bulk of the expense.

Along with more nearly equal sharing of expense, I foresee new models for operation that bring the international partners into closer contact (perhaps virtually) with the facilities. High energy physicists have pioneered the use of technology for big-project international collaboration, and the technology available for high speed communications, data sharing, and instrument control continues to develop rapidly. I expect this community to continue to provide leadership in this area.

I would like to see closer coordination in the planning of large-scale experiments in fundamental science among nations. We are all going to have to invest competitively in the science infrastructure for our technology-based economies. We should invest non-competitively in the science infrastructure for large scale basic science. That includes particularly the astronomical sciences and fundamental particle physics.

National science policy responds ultimately to the needs of the science community. We are going to depend upon you and your colleagues for ideas about how best to plan the future exploration of nature, and how to use scarce resources wisely in the endeavor. I appreciate the opportunity to talk with you today about these issues. Thank you.