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WHERE ARE WE GOING AT SLAC?

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This talk is part of a new role for me. I have been an experimental physicist for my entire career, but now I am turned into another kind of beast — a Director. I have lectured to many a class, given talks at many scientific meetings, testified at Congressional hearings, and so I should be used to public speaking. But this is the first 'Directorial' talk I have given, and I confess that it has given me more trouble than my larger scientific lectures.

My theme is "Where Are We Going at SLAC?" To put this in context, I need to talk a bit about where we have come from in high-energy physics in general, and in high-energy physics at Stanford in particular.

Today is the tenth anniversary of what is known in high-energy physics as 'The November Revolution.' That Revolution was triggered by the simultaneous discovery by Professor Ting and his group at Brookhaven and the Mark I collaboration working on the *SPEAR* storage ring at *SLAC* of what appeared to be a new kind of elementary particle which did not fit into our previous conceptual framework.

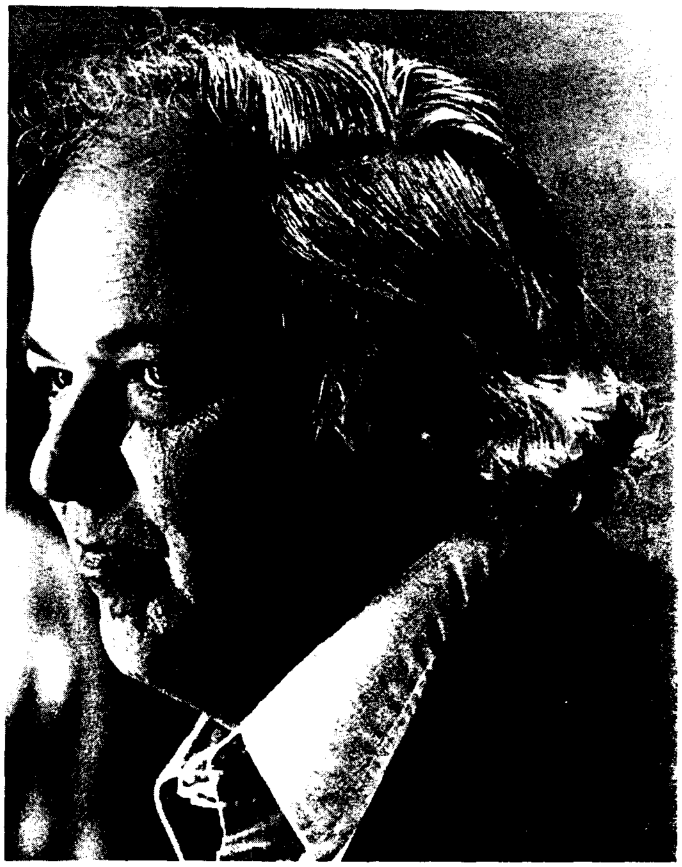
In the decade since that discovery we have made remarkable progress in both experiment and theory. On the experimental side, work at laboratories all over the world has given us new information on the weak interactions; turned up the fourth and fifth quarks, and if recent experimental results from *CERN* are confirmed, the sixth quark as well; discovered a new lepton, a heavy brother of the electron; given much new information on the strong interactions; found the carriers of both the strong and weak interaction; and much more.

On the theoretical side, our view of the structure of matter and the forces of nature has changed dramatically as our theoretical colleagues have absorbed the results of these experiments into new theoretical models. These models say that matter at the most fundamental level is not composed of the atoms of the periodic table, but rather that these familiar building blocks of such things as people, trees, planets, and stars are composite entities built up from fundamental things called quarks and leptons.

Ten years ago, most physicists believed that there were four fundamental forces in nature: gravity, the electromagnetic force, the strong force that holds the nucleus together, and the weak force that is responsible for radioactive decay.

In this decade a new theoretical framework, the Gauge Theory, seems to have succeeded in giving us a unified picture of electromagnetism and the weak interaction. This seems an odd combination for unification: electricity and the force responsible for radioactive decay. We have given this a new name, the electroweak force, and attempts are being made to make a unified picture of the electroweak and strong interactions. Some progress is being made in this area. We're even struggling with the most difficult of all problems — bringing gravity into this picture — with very little success so far.

Such an enormous amount of progress was made during the decade of the 1980s that a very distinguished theoretical physicist, who should have known better, said that it was all over, that we understood it all, at least in the mass region from 100 GeV to 10^{15} GeV. Over this huge energy region spanning a factor of 10 million million in energy, the only thing that the experimenters had to do was to confirm the predictions of what was obviously the correct theoretical model of the world. The last time anyone had the temerity to make a statement like that was about 100 years ago — not long before the discoveries of radioactivity, relativity and quantum mechanics completely transformed the world of physics. The recent assurance that "all is understood" has also proved to be



wrong; the model on which the assertion was based made one specific experimental prediction that could be tested at present energies, and that experiment did not come out as it was supposed to.

We are now back to normal in high-energy physics, where normal means an interplay of experiment, theory and technology that advances our understanding of nature. These three horses pull the chariot of science forward: sometimes one pulls harder than another, but all three are necessary. In deference to our visitors from Washington, I should say that there is a fourth element involved in the advance of science; it is money, and it might be likened to the harness that hitches those three horses to that elegant chariot.

What might be termed the 'standard model' for the advance of science involves an interplay of experiment, theory, and technology. The experimenters, guided by what we know, test the present theories and uncover new facts that sometimes fit and sometimes don't fit into our existing world model. The theorists take the output of the experiments, particularly those things that don't quite fit, and use them to extend the theoretical model to get at a deeper understanding of how our physical universe works. These new models require new experiments, and the accomplishment of those new experiments requires new tools, particularly new accelerators, to give us the ability to probe more deeply into matter. This sounds very evolutionary, and sometimes it is, but sometimes progress in science comes about from revolutionary advances in theory or experiment.

Stanford University and *SLAC* have played a major role in both the evolutionary and revolutionary advance in high-energy physics as long as I have been here, and I have been here for 28 years. It is unusual for a scientist to stay in one place so long, but when I first came to Stanford in 1956 with my new PhD, I came because I believed that the electron beams available from the accelerators at Stanford were the best tools with which to gain a better understanding of the structure of matter. I joined a group of scientists who believed as I did, but who back then at least were considered odd by most physicists who thought that experiments at proton accelerators were "the only way to go" in high-energy physics. Things have changed since then; some may still think we are odd, but that has to do with our personal characteristics, not our choice of scientific research.

Critical contributions to all three of the areas I mentioned before, experiment, theory, and accelerators, have been made by scientists working here. The theorists have contributed their share of both brilliant ideas that illuminate the scientific landscape and "make it all clear" and of red herrings that lead the poor experimenters into spending years of their lives chasing dead ends. The theoretical work is important, but the theoretical contributions made at Stanford are not the reason that all of us are here. This laboratory and its predecessor laboratory are supported because of what we do to develop new accelerators and because of what we do in the way of experiments to use those accelerators. So with apologies to my theoretical friends, I will leave them out in continuing on with this story of where we are going.

Where we are going is determined partly by where we come from. When I first arrived at Stanford, the Mark III linac had just recently begun operation. It, itself, was a bold step in energy, moving linac technology from the tens of MeV that were the maximum energies of existing machines to over 800 MeV. Mark III was a remarkably large machine — all of 300 ft. long. It was a technological *tour de force* in its time, and it was used for many important physics experiments. To mention but one, Professor Robert Hofstadter used the beams from that machine to measure the shape of the proton, showing that it was not a point particle and determining its size.

The advances in accelerator technology pioneered at the High Energy Physics Laboratory *(HEPL)* on campus were, in the long run, probably just as important as the experiments done with the machine. The first colliding beam storage ring was built there both to pioneer a new technology and to carry out experiments at a new energy inaccessible without the new technology. All major accelerators under construction today are colliders of one type or another, and they all owe much to those early Stanford efforts on colliding beams that showed the way to get much higher energy for a given cost.

While all this was going on, a group of scientists led by Panofsky were thinking about the next step in linear accelerators. In 1956 the first meeting was held to begin the conceptual design of a new giant machine, then called 'the Monster' because it was so very large. The first beam from that accelerator, the 10,000-foot-long *SLAC* linac, was delivered in 1966. Most physicists still thought that electron physics was an odd backwater, but the story goes that in Washington the feeling was that if Panofsky thought that it was a good thing to do, we ought to do it. That's probably an overstatement, but not by much. We did have strong support from the theoretical community who were not quite so narrow-minded as most of the experimenters.

The *SLAC* linac was a huge extrapolation in technology, taking linacs from the then 300-foot machine at *HEPL* to 10,000 feet at *SLAC*. Together with the innovations in linac technology came innovations in experimental apparatus, and *SLAC* began its experimental program with such major devices as the large spectrometers of End Station A, the hydrogen bubble chamber, spark chamber systems, etc. The first major experiment proved the worth of the entire effort. Taylor, Friedman, and Kendall, in an experiment on inelastic scattering, proved that the proton had a substructure—it was not elementary, but seemed to be made up of still smaller entities very tightly bound into the particle that we call the proton. As Drell put it at the time, "there seemed to be seeds in the grapes." It was one of those experiments that changed our view of the subatomic world.

There were many important linac experiments in the 18 years that have passed since *SLAC*'s turnon. I won't describe them; instead I'll turn to work that went on in parallel to the development of a new linac, work on colliding beams. In 1961 Dave Ritson and I started the design of what would be the *SPEAR* storage ring. Construction started in 1970, and the turnon was in 1972. Here, too, together with the innovation in accelerators was innovation in experimental apparatus. The Mark I magnetic detector was a powerful tool in its own right, and has been the forerunner of much more sophisticated devices of the same general type located at accelerators all over the world.

I don't have to tell you about physics output of the *SPEAR* storage ring — that is the origin of the November Revolution. The storage ring is still going strong, and we are just now making major improvements to it to increase the colliding beam intensity. While it is hard to see more than five years in the future in high-energy physics in determining what work will be interesting to do, it certainly looks like the high-energy physics program at *SPEAR* will continue for at least that long, giving it at least 17 years of productive lifetime.

In 1970, before the turnon of *SPEAR*, the first paper on higher energy electron-positron colliding beam systems was written by John Rees. Out of that paper came the machine that is now *PEP*, which began its experimental program in 1980. It, too, had a new set of experiments of even greater sophistication than what had gone before, and it is now working to explore the structure of the elementary particles and the forces of nature at still higher energy.

In 1978 work began here on a new kind of colliding beam device — what is now called the *SLC*. The need for a new technology in colliding beam devices became apparent to some of us when we took a hard look at the greatest of all the storage ring colliders, the *LEP* project at *CERN*. This machine is 27 kilometers around and will cost more than half a billion dollars. The scaling laws for electron storage rings are well known; size and cost go as the square of the energy. Given this scaling law, to go up a factor of 10 in energy would require a machine of some 2700 kilometers in circumference costing about 50 billion dollars. A new technique was needed to continue at a price that our real masters, the taxpayers, would consider to be reasonable, and the *SLC* seems to be that technology.

I have gone through this long review to make a point. No laboratory is a static entity. No tool for physics research has an infinite lifetime. The big *SLAC* linac itself — the reason for building this laboratory — is no longer being used for frontier high-energy physics experiments, but serves as an injector for our present generation of storage rings; however, the laboratory is still doing frontier research and is still supported because of that frontier research. We continue because of the innovations in accelerators and technology made by our own staff and adapted from work outside this laboratory. Work on those innovations proceeds while the 'old' facilities are being exploited.

This brings me to the present and face-to-face with the question, What now? I can gaze into the future and make an easy prediction about the next 10 to 15 years.

With the completion of *SLC* in 1986, our linac will be back as a forefront facility. It is the heart of the linear collider, but it will have undergone considerable improvement in the *SLC* project. Beginning with 1986, a new era of experiments will start at *SLAC* that we all expect to contribute important new information to our evolving view of the structure of the physical universe. If we look at the life cycle of any particular facility, we can expect something like 10 or 15 years of productive experiments from the *SLC*. Indeed, the first *SLC* improvement project is already under design (polarized beams) even though the machine is not yet complete. We have no worries about what we will be doing until the second half of the 1990s.

What will we do for an encore?

The answer to that question really depends on the imagination and the initiative of the *SLAC* staff. I can tell you what we're thinking about now, but if one of you has a sufficiently bright idea, we may be thinking about, and working on, something quite different a few years from now.

At present we are working toward the realization of the second part of the *SLC*'s purpose. When *SLAC* first proposed this facility, we said there were two reasons to build it. The first was to carry out experiments in high-energy physics at a new and extremely interesting energy range. We will certainly do that, and I have already mentioned the experimental program and its expected duration.

The second was to develop a new technique for colliding beams, which would allow affordable machines of considerably higher energy to be built. The *SLAC* linac is not optimized for the acceleration of the kind of beams required for a very-high-energy linear collider. The linac was aimed at delivering long bursts of particles with as uniform an energy spectrum as possible. Linear colliders deliver very few bursts within a single pulse, and in that mode their optimization is different. The *SLC R&D* program is already teaching us a great deal about beam dynamics in linear colliders, and we will learn much more when the project is in routine operation.

But linear collider optimization seems also more complicated than optimizing storage rings. It may be that when we are as familiar with them as we are with storage rings it will all seem simple, but we are just learning about them now. There is a group now working at *SLAC* called the Big Collider Study Group — composed of theoretical physicists, experimentalists, and accelerator physicists. This group is studying linear collider optimization with a view toward understanding what technical developments are important in making very large machines both practical and affordable.

Two things have come out of their studies already. The first is that much more efficient radio-frequency power sources than our present klystrons strongly impact the design of the large linear collider and can sharply reduce the cost for a given energy. Necessity is indeed the mother of invention, and, in response to this need, an rf power source, which uses a laser to produce the electron beam in the tube, has been analyzed on paper and looks to be considerably more efficient than our present klystrons. A combined group of people from the Technical Division and the Research Division has done a thorough analysis, including computer simulations, and has started on the construction of a proof-of-principle device which we hope to have ready in about two years. This power source is supposed to have a peak power output of about 100 MW — about twice the power output of the new klystron we are developing for the *SLC* — have a pulse length of one microsecond, and be more than 70% efficient. When we achieve it, this will be the most efficient high power pulsed high-frequency rf source in existence. If it performs as expected we will know that we can reasonably expect to go on to power sources of greater than 90% efficiency.

The second thing to come out of the very early stages of the work of the Big Collider Study Group is that much higher accelerating gradients than we use in the *SLAC* linac look to be desirable to make cost-effective machines. In response to that need, we have begun a program to find out just how large an accelerating gradient can be obtained in a structure similar to the *SLAC* accelerating structure. It has already been shown that we can get more than 100 MeV per meter accelerator gradient, or more than 6 times what is now used in the *SLAC* linac. Since this has been demonstrated in a section about 1 foot long, I have issued a challenge to the Technical Division: deliver a 1-GeV accelerator less than 30 feet long for less than one million dollars. I expect they will do it.

There are many other things coming out of the early work of the Big Collider Study Group, which are pointing toward research programs which we should be carrying out.

In addition to these exciting extensions to the technology that we have been using here for years in building big accelerators, people at *SLAC* and elsewhere in the world community are working on exotic acceleration schemes. These include plasma accelerators and laser accelerators. They probably won't be practical any time soon, but when they are, they promise a technology that will lead us to still higher energies.

Why are we doing all of this? To the people involved in the scientific enterprise the goal is knowledge. We are trying to learn how the physical universe works, what its structure is, how it was born, how it will evolve, and how its elementary constituents interact with each other.

Society supports us partly for this reason, but for other reasons as well. It is true that knowledge is power, and as we learn more about our physical world, we are better able to manipulate it to accomplish the goals of mankind. I have no notion of whether the work in particle physics that we are carrying out now will lead to new methods of controlling nature; I can only say that it always has in the past, but no one can say whether it will in the future.

As to the future of *SLAC*, I have already told you some of the things that I see in the next 10-15 years, but my real message is perhaps best summarized in the old axiom that the Lord helps those who help themselves. Over the entire time I have been at Stanford, we have continued to develop high-energy physics here by helping ourselves through the invention of new theories, new accelerators and new techniques of experiments. How far we will go beyond the 10-15 years that I can see clearly depends on all of you and your contributions to the next generation. I will do my best to guide the program and obtain the resources so that what you dream up can come into existence.

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