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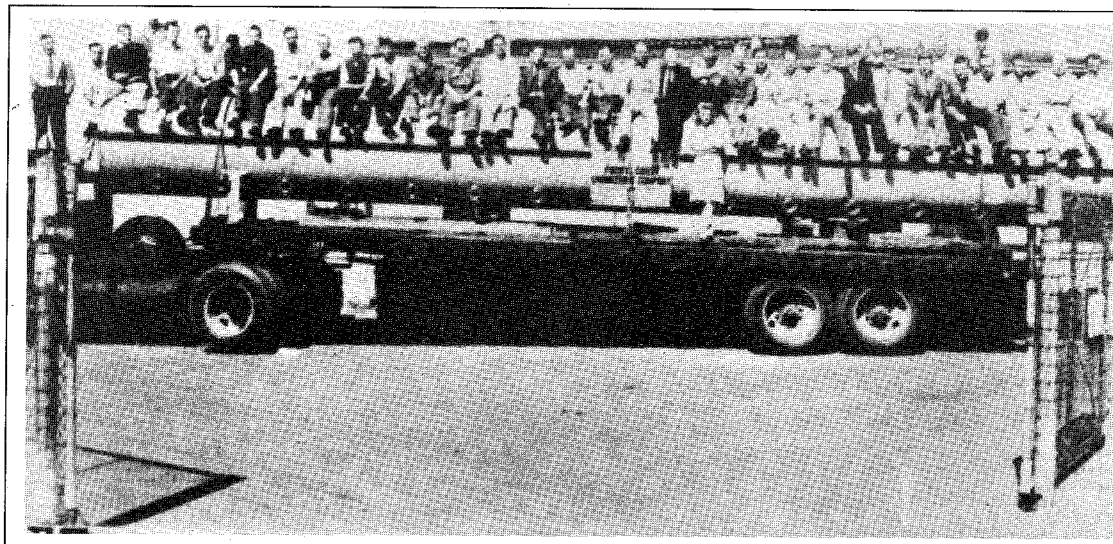

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On the cover:
Proton linear accelerator designed by Berkeley's Luis Alvarez and his associates (above); Bill Hansen's Mark I linear accelerator (below). Hansen is at far right. Photographs courtesy of SLAC.

BIG physics and SMALL physics AT STANFORD

BY W.K.H. PANOFSKY

I came to Stanford University in 1951 from that hotbed of big science, the University of California's Radiation Laboratory at Berkeley. Beginning with the pioneering work of E. O. Lawrence, the Rad Lab built a series of major facilities for purposes ranging from nuclear physics to what is now called high energy physics, and branched out into many different sciences. In the 1930s, when Lawrence embarked on the big science path, physics at Stanford University was centered in its small but well known Physics Department. Today, physics at Stanford University is highly diverse, spreading over many departments and academic units. It spans a very large range in scale from traditional, small laboratory experiments to the Stanford Linear Accelerator Center and the general relativity precessing gyroscope experiment in space. I will concentrate on the implications of this transition rather than an historical accounting.

The changes in physics have raised many profound issues relating to the role of Stanford University. Typical issues are: the conflict, on the one hand, or symbiosis, on the other, between research and teaching; the extent to which large science enterprises permit the creative participation of graduate students and junior faculty; the issue of proper recognition of the work of individuals when scientific papers require one page of a journal just to list the authors. Above all, we are facing tensions between the sponsor's quest for accountability, and even control, and the traditional academic freedoms to publish, to choose one's work, and to choose the academic staff within the University.

One must recognize that none of these issues are at all new, and that in spite of all the changes which have taken place, there remains a unifying spirit throughout the physics enterprise at Stanford. This spirit is an attempt to understand inanimate nature in its most fundamental aspects and to communicate this understanding to future generations.

Before the Second World War, physics at Stanford went in several directions. There was the extraordinarily productive work led by Felix Bloch in basic theory and work on spin physics as well as his work in neutron physics using a small cyclotron in the basement of the Physics Corner. There was the microwave work pioneered by Bill Hansen and the fundamental x-ray activities of David Webster and Paul Kirkpatrick. Interestingly enough, during those days specialization was not so great that it prevented collaboration among practitioners in each of these fields when something new and exciting took place. Some of the early papers on the klystron, for instance, included Webster together with Hansen. At the same time, Hansen took a great deal of interest in Bloch's work in nuclear magnetism. Also the fundamental experiment on the magnetic moment of the neutron was carried out by Bloch in collaboration with Luis Alvarez from the "big science" laboratory at Berkeley, thereby combining the expertise and ingenuity of both of these great men. What was different before the war, both at Stanford and at almost all other universities, was that research and education were more intimate than they are today. Part of this is indeed a matter of scale; part of this is related to the means of support.

We like to take pride in the fact that the basis of a great university like Stanford is the inseparability of research and education. In consonance with the declaration of Stanford's first president, our credo is that teaching will be more inspired, and will also be more factually correct, if it is carried out by individuals who have primary experience with uncovering fundamentals of nature. Derivative teaching by professional science educators drifts away from the factual rigidity that is so essential in faithfully representing the nature of science. The great physicist, Richard Feynman, stated: "The whole question of imagination in science is often misunderstood by people in other disciplines . . . we cannot

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allow ourselves to seriously imagine things which are obviously in contradiction to the known laws of nature . . . the problem of creating something which is new but which is consistent with everything which has been seen before, is one of extreme difficulty."

This could only be said by an individual who is both a great teacher and a hands-on researcher. Yet before World War II, and today, too, this basic inseparability is threatened.

Universities rarely dedicate significant portions of their own unrestricted funds to the support of research. Physics at Stanford before World War II was not always small science by choice, but frequently by necessity. Before the war, then-chairman David Webster attempted, with a varying amount of success, to organize large scale research supported by outside sponsors. Outside sources for supporting fundamental work on multi-million volt x-rays proved insufficient, but support for applied work stemming from Bill Hansen's electromagnetic cavity became available from the Sperry Gyroscope Company. This led to a frequently stormy relationship among the physics department, the University administration, and the company. At issue were fundamental questions about who controls the subject matter on which to work, who makes appointments, and the freedom to publish. A key irritant remained throughout: How to reconcile Stanford's need to make money through patents with the traditional academic freedoms.

Teaching vs. Research

After World War II the resources of all universities, including Stanford, were severely strained to revitalize their teaching function and to take care of the large influx of students whose education had been interrupted by the war. At the same time, the power of organized university research had been recognized by the federal government. University scientists had made vast contributions to the development of radar, rockets, atomic weapons and other military devices during the war, yet Stanford had been ill-prepared to participate in such work at home. Instead, Stanford scientists took up war work at eastern universities and at corporations.

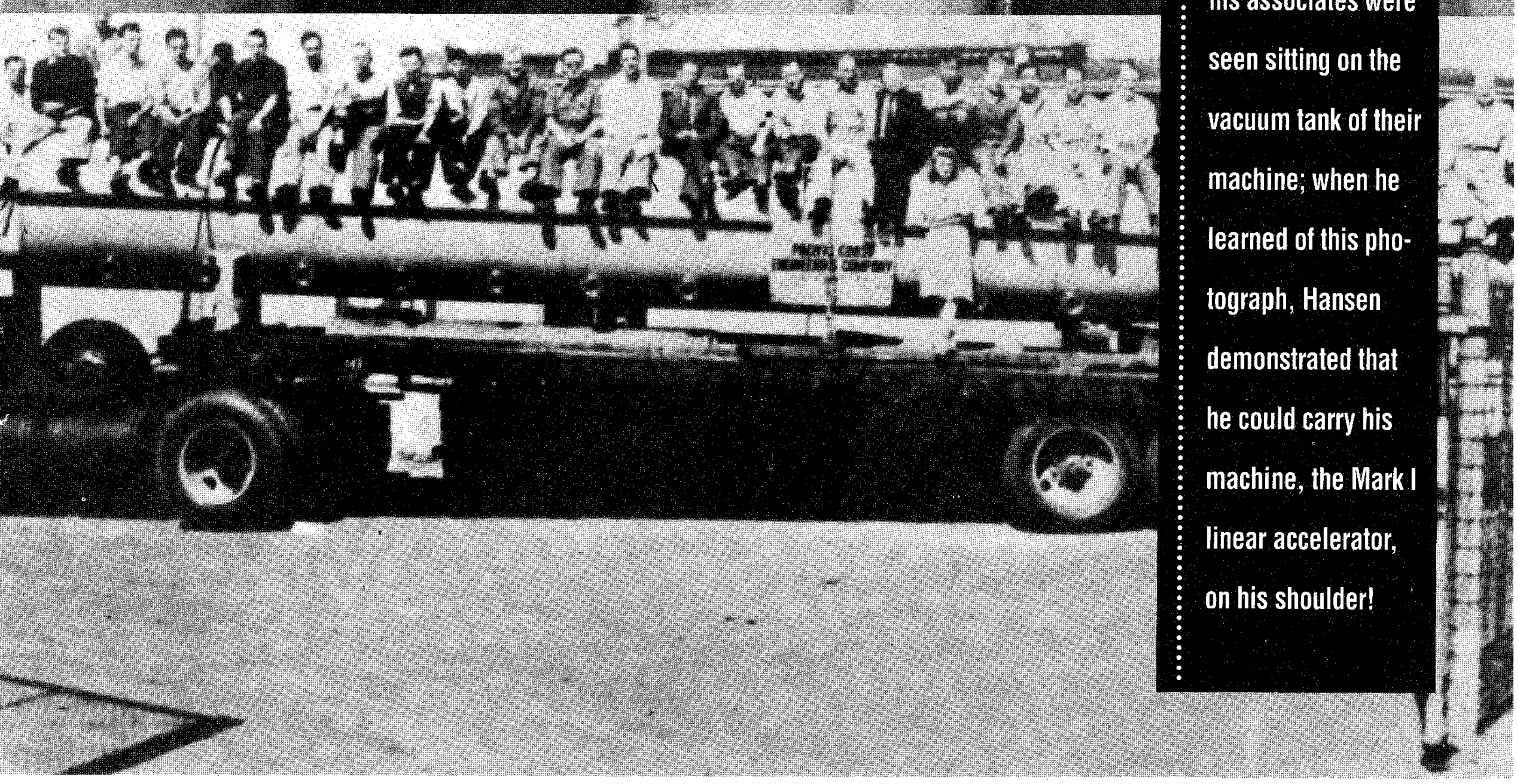
As a professor and later as Dean of Engineering, Fred Terman had worked hard to forge close links between engineering and physics in order to enlarge the impact of basic science on the practical world, and at the same time to expand the resources for science at Stanford. Yet he, too, departed during the war years to take charge of the radar countermeasures at the Harvard Radio Research Laboratory.

After the war the federal government was persuaded that it should support basic research at American universities, and initially the Office of Naval Research became the principal agent to perform that function. At the same time, most universities, including Stanford, concentrated their own resources primarily on classroom teaching. The government investment permitted great expansion of research activities both in coverage of subject matter and in the depth and breadth each research enterprise could employ. However, it also generated an administrative separation between research and education, whose unity distinguishes a great university from a college. The increased emphasis on research after World War II caused the establishment of separate laboratories dedicated to research only, such as today's Hansen Laboratories at Stanford. These laboratories sprang from the Microwave Laboratory established in 1945, directed first by Bill Hansen and later by Ed Ginzton. With the building of the Mark III accelerator complete in 1952, the lab split into two programmatic units — the High Energy Physics and Microwave Laboratories, managed under a common administrative umbrella named after Bill Hansen. Such expansions caused Professor Paul Kirkpatrick to complain that the mails were heavy with outgoing manuscripts and incoming honors, but where is our dedication to teaching? This lament reflected the concern of many. Yet the technical fact remains that in many fields of basic physics larger tools, and the concommitant financial investment and administrative effort, are an absolute necessity if progress in experimental physics is to be made. It is the tools, not the motives of the practitioners, that have changed over time.

When Bill Hansen invented the electromagnetic resonant cavity before the war, he was interested in generating high voltages for particle accelerators with relatively modest inputs of power. However, the initial uses of his invention were not for basic research with particle acceleration but for microwave power sources and radar. Following his war work in the east, Hansen returned to Stanford to refocus his microwave efforts on a linear accelerator with the objective of obtaining high particle energies for fundamental physics.

Measuring Big vs. Small Science

At the same time, work on linear accelerators related to protons rather than electrons was started in Berkeley by Luis Alvarez and his associates, of whom I was one. A proton linear accelerator was designed and built to operate at 32 MeV. In the late 1940s, that proton accelerator was a much larger device than Hansen's electron machines at Stanford because, for technical reasons, a frequen-



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cy 15 times lower was chosen for its operation. In one memorable photo, Alvarez and his associates were seen sitting on the vacuum tank of their machine; when he learned of this photograph, Hansen demonstrated that he could carry his machine, the Mark I linear accelerator, on his shoulder! The comparison is but one example of big vs. small science.

Another way to measure evolution from small science to big science since the war is from the size of progress reports. Hansen's report to the ONR announcing the successful initial operation of the 6 MeV Mark I accelerator consisted in its entirety of one line: "We have accelerated electrons." By contrast, the Environmental Impact Statement alone of SLAC's latest construction venture, the Stanford Linear Collider, is a book almost one inch thick.

INVENTING "THE MONSTER"

The Stanford Linear Accelerator itself was a result of the great success of the Mark III accelerator. Throughout the mid-1950s, Bob Hofstadter encouraged the development of higher energy machines, primarily as a needed extension of his own research. The principal motivation was to use ever higher energies to increase the sensitivity to the detailed structure of the fundamental particles at small distances. A volunteer design group working to evolve a design for a larger machine constituted itself from the personnel of the High Energy Physics and Microwave Laboratories. Their work resulted in what was then called Project M (affectionately called the Monster.) Today we call it SLAC.

Ever since SLAC was started, I have frequently been asked, how long will the lab last? My answer has always been, "Ten years or so, unless somebody has a good idea." This answer was first given over 25 years ago, and has been repeated many times. There have been many good ideas and they have led to the evolution from the original two-mile machine through the SPEAR and PEP storage rings to the Stanford Linear Collider which has recently produced new pioneering results. This evolution is part of the "innovate or die" syndrome of much of modern big physics. The frontier of the science is advancing rapidly; new and bigger tools are needed. Yet these tools are expensive and require elaborate long term planning and organization.

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Roughly speaking, it takes five years to plan a new facility — planning for Project M started in 1956, the proposal to the government was submitted in 1957, but ground was not broken until 1962. Construction of all projects tends to take about four to seven years. In fact, it is remarkable that the actual construction of new facilities starting from the first nuclear physics instruments in the 1930s to the monster machines of today takes about the same time. This happens because we tend to marshal a magnitude of effort proportional to the task. The reason is about the same as why a flea and an elephant can jump to about the same height — the amount of muscle they have and their weight are approximately proportional to one another.

It is very desirable to have the construction period not be too long; this is because one of the important factors responsible for the high productivity of large physics has been that the builders and users of the large facilities tend to be the same. Yet young people cannot afford to be both builders and users if the building takes too long. Most directors of the high energy physics laboratories are adept at both the construction and use of large facilities. For example, Burt Richter, SLAC's current Director, is known worldwide both for his design of colliding beam accelerators and for his discoveries using high energy accelerators and colliders. This unity between users and builders provides a strong motivation for building such modern facilities in the most efficient and economical manner, and we are proud of the fact that the original SLAC machine, as well as its succeeding installations, was built on schedule, on budget, and close to expected performance — a situation which contrasts very favorably with other large "high technology" enterprises in space and military activities.

TEAM WORK

As physics installations become larger and larger, they must be more extensively shared among different academic institutions. It is no longer possible for each research university to have its own accelerator "in the basement" which operates at the frontiers of science, as used to be the case in the 1940s and '50s. Rather, a lab like SLAC has to be a national laboratory, or in this case, more precisely, a national facility. This means it must be available to scientific users from across the country, and in fact from all over the world, with the choice of experiments being based only on scientific merit of each proposal and the demonstrable ability of the proposers to get the necessary work done.

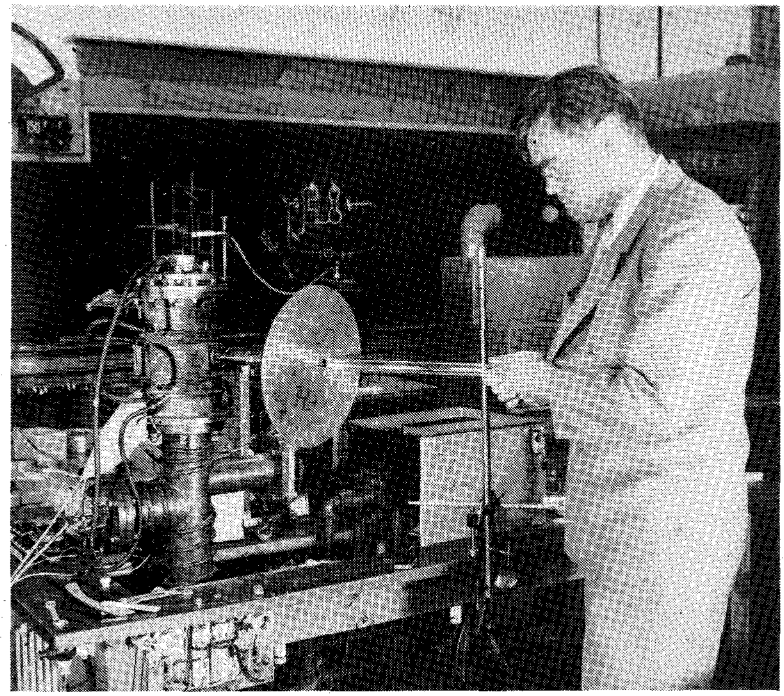
This selection process brings on many new problems. It means that physicists must submit competing proposals for experimental work. This is quite a departure from the more gentlemanly academic tradition in small science, where work is done at will by faculty and graduate students. Senior faculty members at Stanford have tenure, which means that once they are appointed to an appropriate rank on the faculty the merit of their work is no long subject to review; the idea is that they are qualified to do research and teaching, and posterity shall be the judge of their contributions. In contrast, today's university research worker, whether he or she be a faculty member or not, has to continuously run a gauntlet of outside reviews evaluating proposals, both for government funding and for acceptance of experiments at the large physics facilities. Simply stating that one's next proposal is great because one has done great work in the past is no longer sufficient for acceptance. While almost everyone recognizes that this process is necessary, it is distasteful to some and has caused some physicists to defect from the ranks of high energy physics. In fact, initially when SLAC was proposed a minority of the Physics Department wanted this machine to be a proprietary tool of the Stanford Physics Department. Some initially interested members of the department lost interest in participating in SLAC work when it was eventually judged that operating SLAC in this manner was neither feasible nor desirable.

Another issue is that work in big physics tends to be done in large collaborations. The design, construction and exploitation of the large detectors used at a lab like SLAC is an enterprise of comparable magnitude to constructing the accelerator or collider itself. Many dedicated professionals—engineers, mechanics, craftspeople, administrators and operators—support the work. Therefore, many physicists who wish to reap the benefits of the installation collaborate in developing the concept of a detector, taking responsibility for the design and construction of its myriad of components, integrating these components into a working whole, and working together on designing the on-line and off-line computer programs needed to extricate the information from the data. The fundamental purpose of doing the work remains the same, but the means demand this type of complex, large scale collaboration. How, then, does individual talent manifest itself under these circumstances?

Distinguishing the "Great" Discoveries

There are several ways. First, during design of the apparatus the real ideas and inventions make all the difference. Then, there are times when great discoveries are made after both the accelerator and the detector have been built, but where a small dedicated group of individuals working in the traditional faculty and student pattern "mine the computer tapes" in order to extricate previously hidden information. A classic example is the discovery of what is known as the tau-lepton by Martin Perl of SLAC and his colleagues. After the storage ring SPEAR (into which the SLAC accelerator injects electrons and positrons) had been producing data for several years, Perl and his associates noted that two types of particles seemed to be produced in coincidence more frequently than could be attributed to sheer chance. They then spent several years thoroughly analyzing this apparent excess of coincident events and finally decided that indeed this represented a new phenomenon. The result was the discovery of a new family of particles whose existence had never been previously conjectured, but whose existence now has been confirmed at many laboratories.

How does one recognize real intellectual leadership and contributions apart from faithful and more routine professional contributions? Publication lists alone do not suffice. In the case of the tau-lepton discovery by Martin Perl, the answer is clear. But that kind of discovery of unsuspected phenomena tends to be the exception rather than the rule. At SLAC, we try very hard to have as many of the younger participants in a collaboration as possible present results at conferences all across the world so that their detailed contributions to and understanding of new experiments be known. Yet beyond such exposure, professional recognition tends to depend more and more on personal communication and recommendations rather than on a formal examination of the publication and research record.



Physicist W.W. Hansen with the klystron, c. 1937. Hansen's discoveries in the lab, as well as his theoretical studies, led to key developments in the acceleration of electrons.

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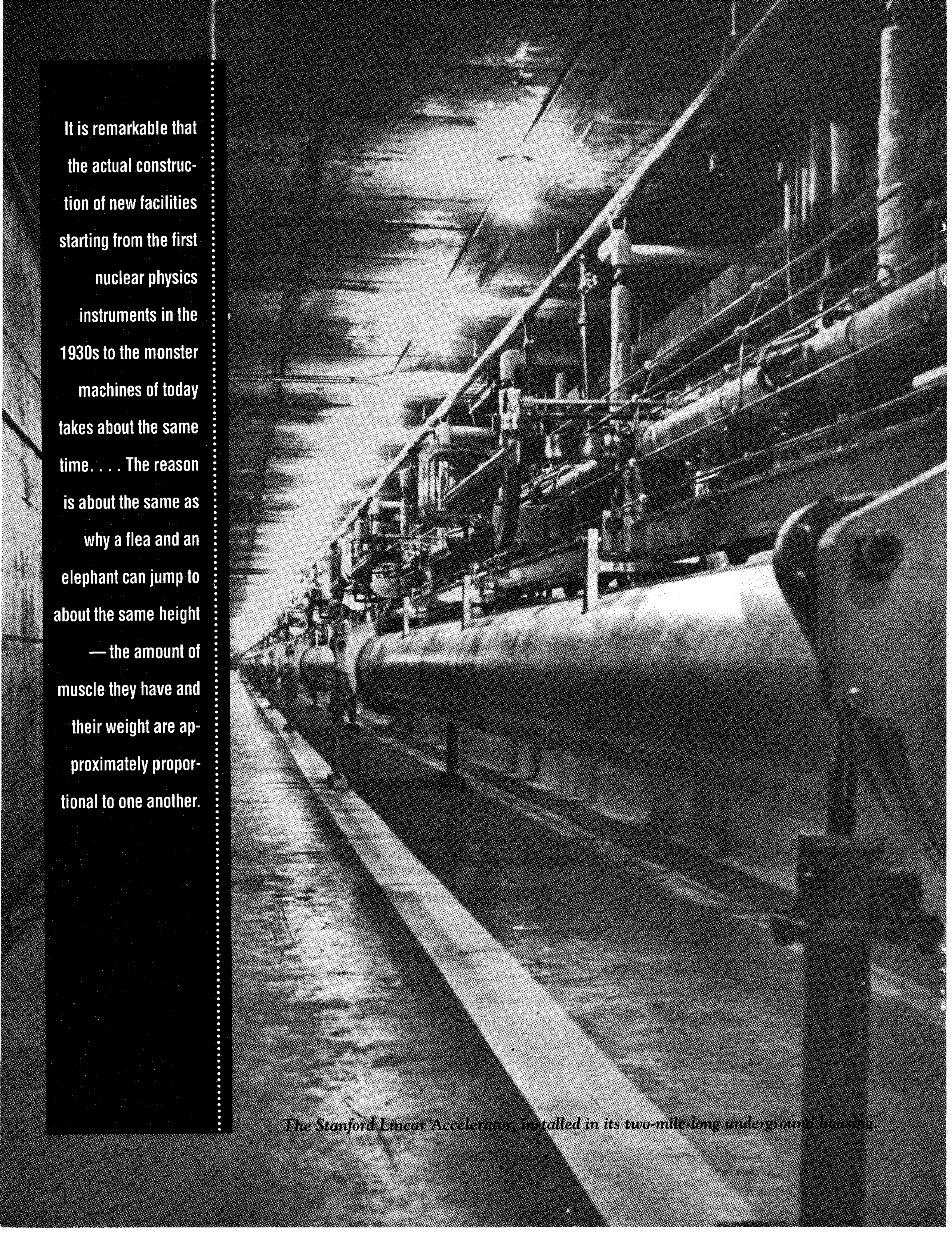

The Stanford Linear Accelerator, installed in its two-mile-long underground housing.

It is customary that when new results from a complex particle detector are being published, all the contributors to the design and construction of that detector sign the publication. This, of course, implies that all collaborators must have enough knowledge of all the complexities of the apparatus so that they can feel comfortable sharing in the responsibility of the new result.

But in fact, no one person in a large collaboration can know all the details which affect the final result. It is unavoidable that in big physics the "immediacy" between the process of nature and the knowledge of the physicist will be impaired. Bill Hansen once said before World War II, "Give me another machinist, don't give me another engineer." He, being a superb physicist and machinist, did not want an engineer as an intermediary. But today, the contributions of engineers, designers and programmers are absolutely essential, and all possible effort must be made that through good communication the physicists intimately know the critical information which affects the results.

This complex pattern has been cited as a possible disincentive to enter the field of big physics, or even physics at all. Yet the facts do not support the assertion that the professional career risks in big physics are any greater than in most other academic endeavors. In fact, they are generally less so. The average time for a new graduate student to get a Ph.D. degree at Stanford in high energy particle physics is no longer than in other, small physics endeavors, and tends to be shorter than it is for the humanities. The placement record of successful Ph.D. students in big physics is, if anything, better than that of other students: a large fraction of big physics students tend to remain in academic pursuits; for those who don't stay in academe, industrial demand is large since the experience of big science students in collaborative efforts is much appreciated by industry, even though the specific technical experience may not be directly applicable.

While the highly collaborative effort of big physics currently is not a valid basis for discouragement, the problem is expected to become more serious in the future if the typically relatively short construction time of new facilities cannot be maintained; for instance the expectation is that the Superconducting Super Collider, or SSC, will take close to a decade to become a reality.

Accountable, Yes — Controlled, No

Interestingly enough, the charge that big science would lead to loss of traditional academic freedoms—since the sponsor who pays the piper calls the tune—is contradicted by the experience of physics at Stanford and specifically at SLAC. The work at SLAC is carried out under a contract between Stanford University and the U.S. Department of Energy, which is renewable every five years. That contract is based on "mutuality"; that is, the University proposes the broad content of the research and the government funds the work at the level it chooses. There is a lot of "red tape," i.e. there are lots of program reviews, requirements for reports and for detailed financial accounting. However, the government has no right to require the University to undertake classified work, to hold up publication, or to require its approval of any appointments other than that of the laboratory director or his deputy. Selection of experiments and the conduct of the work within the funds provided is up to the laboratory. In other words, there is *accountability* to the government but no program *control* by the government.

Notwithstanding the change in scale, the government-university relationship is governed by the same broad policies at SLAC as it was when the Office of Naval Research represented the government in the days of the smaller linear accelerators. These arrangements are in actuality more liberal than they were in the old, small science days when Sperry Gyroscope supported the work of the Stanford Physics Department before the war.

This discourse illustrates some of the changes which have occurred in the transition from small physics to big physics. There is no question that these changes have indeed been large, but one should be loath to conclude that they are changes either for better or worse. One should also recognize that these changes have been evolutionary and are, to a varying degree, affecting all branches of the science. Administrative inventions are needed continuously to make this evolution compatible with academic spirit and purpose, the needs of the participants in the science at all stages of their careers and to maintain the opportunities for personal creativity. Yet we must remind ourselves over and over that what is changing are the tools of physics as the science advances, not the motivation to expand our knowledge about the nature of matter.

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Wolfgang Panofsky, Ph.D., is director emeritus of the Stanford Linear Accelerator Center. His article is based on a talk given Jan. 20, 1990 to the Stanford Historical Society and the Stanford University Library Associates.

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