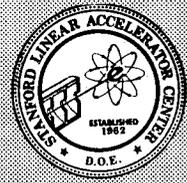




SLAC



VOLUME 14

№ 4

BEAMLINE

FEATURING:



THE SCIENTIST



THE MAD SCIENTIST



THE USER



NORMAN SILVEIRA ©1983

IN THIS ISSUE: THE MONSTER IN MARK II!!

THE MONSTER IN MARK III

Scientists have breathed a sigh of relief upon learning that the scion of the Mark III sighted on the first day of April has been suitably restrained. The cover drawing was based on early reports.

This experiment was given a more traditional introduction in the May-August, 1981, issue of the *Beam Line*. A photograph in that story inspired our cover artist, Norman Silveira.

Silveira is a draftsman and illustrator at SSRL, and a talented free-lance artist. He has recently published a complete story in a comic called "Four-Fisted Tales."

THE COMIC BOOK GENRE

During the 50's the EC (Entertaining Comics) Comics Company came into being, producing such well known books as *Tales from the Crypt*, *Two-Fisted Tales*, and *Mad Magazine*. Bill Gaines, publisher of the EC line, still produces *Mad*.

The EC's were known for their particularly dramatic stories and the high quality of the artwork, a combination which is unmatched to this day.

The *Beam Line* cover is based on the *Tales From the Crypt* format; I've always wanted to do a cover in this form and this seemed the perfect opportunity.

-Norman Silveira

NEXT ISSUE

PEP has jumped another peg in luminosity to 3×10^{31} per cm^2 sec, and the several experiments are taking data about five times faster than last year.

The list of improvements include the moving of focusing quadrupoles and changing the machine tune. That sounds like good storage ring technique. But the list also includes things like 'golden orbits' and the 'green thumb effect.'

A clear technical article describing these PEP achievements was submitted to a recent accelerator conference and will be incorporated into a second issue of the *Beam Line* later this month.

SUPERCONDUCTING CHEVY

The Applied Superconductivity Conference was held in Knoxville, Tennessee, in December of 1982. Steve St. Lorant, who for the past 6 years has been the editor of the proceedings, is head of Low Temperature Materials Research at SLAC, a group that has been active in superconducting magnet development.

Steve planned to combine his conference organizational work with some big helpings of southern hospitality amongst the good ole low-temperature boys. Everything was proceeding nicely until good ole John Alcorn (it's been a few years since he enlivened the SLAC scene) showed up at his old friend Steve's hotel with a plea for help. John had, it seems, found just the 55 Chevy Bel Air hardtop coupe he was looking for, waste deep in a Tennessee farmer's south forty and further. It ran. Would Steve co-pilot it back to California?

Steve cancelled his plane, boxed up the unedited conference papers, and jumped in the vehicle which had been christened Clarabelle and is depicted in the Alcorn cartoon shown below.

Steve lasted as far as Albuquerque. A tornado in Arkansas and a snowstorm in the New Mexico Rockies was more than enough to endure in an old chevy, classic or not, with porous floorboards, mud in the transmission, no muffler, no radio and several broken windows. Not too many of the papers were lost in the tornado. At least Steve has a ready explanation when one his proceedings contributors asks what happened to a paper.

-RCG

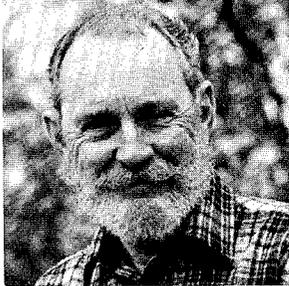


GEORGE OWENS RETIRES FROM SLAC

George was working on the railroad—but not just to pass the time away. He worked for a railroad company in their telegraph services, practicing his skills with words and learning how a rail transportation company works.

George's interest in words naturally led him to the world of books—and where does one find books *par excellence*?

In libraries, that's where.



So George graduated in library science and worked as a librarian for several years before coming to SLAC in 1962. Confronted with the chaos that was SLAC in those days, George took on the challenge and organized what has become one of the most advanced technical libraries in the United States. In the process of doing this he recruited a premier staff, including Bob Gex and Louise Addis, developing the sense of dedication and helpfulness that has been the hallmark of the SLAC library. After organizing the world of books at SLAC, George also tackled the paper chase known as the Publications Department. Again, he worked with the top-notch staff, and laid the foundation for all the editing and illustrations work that is needed to satisfy the demands of a high level research laboratory.

On the seventh day George rested, i.e. he joined the staff of the SLAC Personnel Office. Here he brought his skills in the computer field to a group which was still using quill pens and wore high shoes laced all the way up. No one spoke any of the languages George used, and like Goldsmith's village schoolmaster, all the Personnel Office wondered at how much he knew! With remarkable ease he carved a niche for himself (no one has ever figured out how one 'carves' quicksand, but George firmed up the terra) and became known as the person one goes to for answers on all sorts of personnel data.

Those of us who have worked with George over the years will always remember his wry grin and the twinkle in his eye as he strove to answer the questions we put to him. If George and A. A. Milne had been personally acquainted, George would have appeared as one of the lovable characters in "Christopher Robin" ... says Alice.

—Gerry Renner

JOHN GORDON HARDING RETIRES

A native Californian, born and raised in San Diego, Gordon took an early interest in things electrical. He survived the San Diego school system and went on to Cal Tech before graduating with a BSEE degree from UC Berkeley.

Gordon came to SLAC Plant Engineering in 1968 bringing a lot of valuable experience with power distribution systems; he had worked for five years with PG&E and six years with East Bay Municipal Utility District.



He also had a hand in the electrical power systems of the Bevatron and Hilac at Lawrence Berkeley Lab for 14 years and did a stint with Rogers Engineering.

Many of you have seen Gordon here at SLAC on weekends and in the middle of stormy nights, along about midnight, attending to emergency calls to restore power for some critical operation. Fortunately for SLAC, he lives conveniently close, in Portola Valley. He performed design and construction field engineering during the PEP additions at the Master Receiving Substation, as well as many other continuing projects. One memorable period was his assignment for three years as Engineering Supervisor of the Electrical Utilities Operations Group. For the last several years he has been closely following the data gathering process of forecasting loadshedding (brown-outs) to preserve SLAC's power allocation from WAPA. This meant many phone calls to places like Red Bluff, Sacramento, Fresno, San Francisco Airport, PG&E Load Dispatcher and Ames/Moffett Field for temperatures, loads and weather information to be factored into the crystal ball forecasts on whether or not to 'brown-out.'

At Gordon's retirement luncheon his many friends were anxiously awaiting his retirement speech in anticipation of one of his famous anecdotes. He quoted General Sherman's address to the West Point graduating class of 1870, which consisted of five words: "Gentlemen, you're in the Army." Gordon liked the example and held his remarks to five words: "Ladies and gentlemen, I'm retiring." We will miss Gordon's subtle wit and appropriate quotes and seeing him all over the site wearing his bright yellow hard hat. Good luck and best wishes.

—Alex Tseng & Bill Lusebrink

NEWS & EVENTS...

SUNDAY FLICKS

- o Mem Aud at 7:00pm and 9:30pm. \$1.50
- o Apr.10 Tron
- o Apr.17 Reds (8:00pm only)
- o Apr.24 Animal House
- o May 1 Return of the Pink Panther
- o May 8 Quest for Fire

MUSIC AT STANFORD

- o Thurs., Apr.21..Stanford Symphony Orch., Dinkelspiel Aud, 8:00pm. \$4.00

COMIC OPERA

- o Tenth Anniversary Gala (highlights of previous shows and the complete *Trial by Jury*) by Gilbert and Sullivan. April 15, 16, 17, 22, 23, 24. Dinkelspiel Aud. 8:00pm \$5.00

TRAVEL FILM

- o Mem Aud at 7:45pm, Monday, \$4.00
- o May 2 Holland

CLASSES FOR A SOGGY SPRING

The Stanford Health Improvement Program has assembled an array of spring courses to help those of us who are rained out and under exercised.

To recognize and control stress through the use of new problem solving skills, exercise, communication and time management techniques, a six-week course is being offered.

Exercise is the basic tool which will be used in classes aimed at the control of weight, stopping smoking and stress management. Supervised jogging and swimming offer the 'healthiest lunch break on campus' and if aerobic movement sounds appealing a program of stretching, aerobic and toning exercises to music is also available.

Further information on these classes may be obtained by calling the School of Medicine (87-9649).

GARDEN PLOTS AVAILABLE

The SLAC garden club has a few extra plots this summer. If you want to grow you own vegetables or flowers, call Helen Mogilev, x2266.



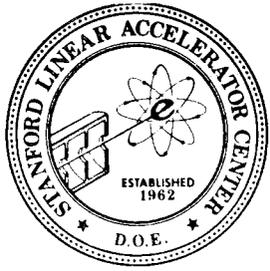
ACCELEPEDE

Pictured above is work in progress on three sections of what will be an antic paradigm of the Stanford Linear Accelerator. This strange device called the Accelepede will crawl the Bay-to-Breakers run (a 7.6 mile carnival in San Francisco on May 15th). Though it is still in initial design phase, it can be divulged that, at *this point in time*, it is made up of white duct beam pipe connected to red modulator boxes, all to be propelled by charged-up SLAC runners.

We hope to make it infinitely long so that everyone who wants to participate can (29 SLAC runners have already signed up). So make up your mind by April 15, which is the cutoff date for the Bay to Breakers sign-up and therefore also for the SLAC accelepede. If you want to join this madness call Ken Witthaus, x2468 or Bob Gex, x2411. Any design ideas are welcome. We are thinking about flashing lights, graphic dials on the modulators, beepers and signs, etc. Also if you have the time we are going to need some assembly work (painting, soldering, etc.) later in April or early May.

SLAC Beam Line, Bin 80
Stanford Linear Accelerator Center
Stanford University
Stanford, CA 94305

Editorial Staff: Bill Ash, Jan Adamson,
Dorothy Edminster, Bob Gex, Herb Weidner
Photography: Joe Faust, Walter Zawojski
Illustrations: Publications Department



SLAC BEAM LINE

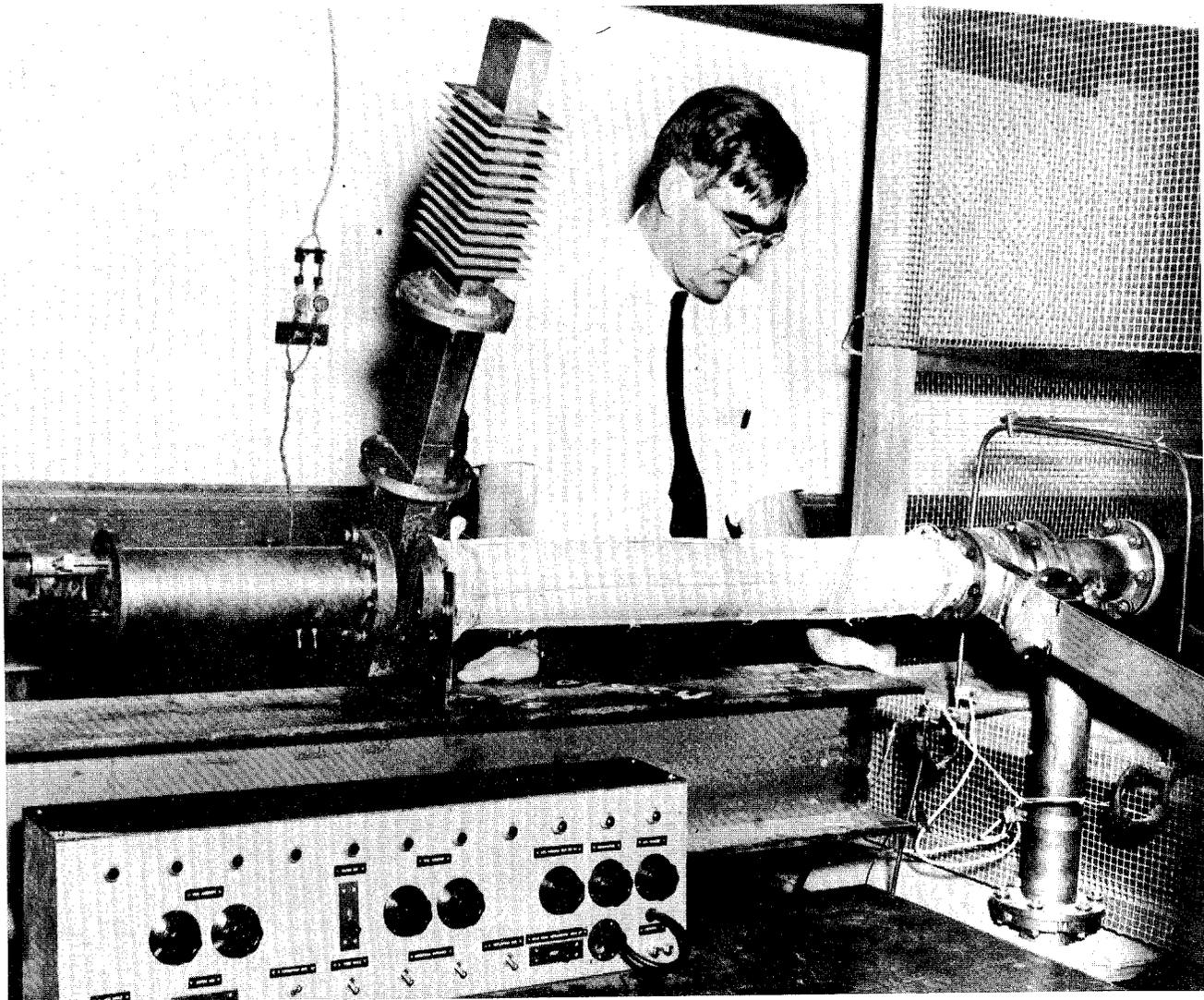
Special Issue, Number 2

April 1983

—AN INFORMAL HISTORY OF SLAC—

PART ONE: EARLY ACCELERATOR WORK AT STANFORD

by Edward L. Ginzton



W.W. HANSEN WITH HIS FIRST OPERATING LINEAR ACCELERATOR

The story of linear electron accelerators goes back nearly 50 years at Stanford and is particularly entwined with one man, Bill Hansen, shown above with his first operating linear accelerator.

EARLY ACCELERATOR WORK AT STANFORD

Edward L. Ginzton

I am pleased to be back at Stanford and to participate in the SLAC Anniversary Celebration. As many of you know, the early period of accelerator development has been central in much of my life, and I should like to share with you some of my recollections. Many of you participated in bringing about the success of SLAC; others have worked on the Mark III accelerator and remember the early period of this development as it demonstrated the utility of linear electron accelerators for a variety of applications.

My involvement in this activity goes back to about 1937: first as a graduate student and shortly afterwards as a participant in the development of a number of accelerators. I should like to tell you something of the events which occurred over a period of time which begins in the mid-1930's with the early work of Hansen and extends through the Congressional authorization of SLAC in 1961. Professor Panofsky will pick up the story at that point and review the rationale and circumstances for starting the SLAC project, as well as outline some of the key events during the construction.

The work I would like to describe goes back to about 1935, something like 47 years ago, and it has involved many hundreds of individuals who have brought about success along the way. To do justice to the work of others, I really should list the key events and mention a great many individuals; this I cannot do in the time that I have, and even if I tried I am sure I would not be accurate nor complete. Instead, I should like to give this talk today without mentioning the names of individuals who made this work possible, except for those occasional names of people whose work is of historic importance.

There are many ways in which you could become acquainted with the history of this work. One, for example, is to read the famous Blue Book, *The Stanford Two-Mile Accelerator* * , which contains some 1200 pages. In addition, one could go to numerous volumes of Congressional testimony containing presentations by many individuals, supporting reports by others, relevant correspondence, and the like. It would truly be impossible to review our history in any coherent way in the time we have available. Therefore, I have decided to pick out only a few isolated topics which seemed to be important to me personally and describe these in some detail. I shall provide a kind of skeleton of events which make it possible for me to relate the incidents I describe to the overall history. I am sure that most of you have a good deal of personal knowledge of this period and will

be able to fill in the gaps.

I mentioned already the Blue Book and the comprehensive record of events it provides. I am also aware of the content of the talk which Professor Panofsky will give later on today. These and other sources provide you a chronicle of events and describe some of the major obstacles which had to be overcome along the way. For one reason or another, the historical accounts I have seen—complete as they may be—do not mention the periods of joy when luck went our way nor periods of anguish or despair at other times. Believe me, there were many moments of both kinds, and I can only wish that in my talk I could convey some of the spirit of events as they occurred.

I should like to start with a biographical sketch of Bill Hansen, without whom there would not have been any accelerators at Stanford. Most of you already know that Bill Hansen was an unusual man with an uncanny combination of experimental and theoretical skills and remarkable ingenuity. Most of us know him as the founder of the accelerator activity at Stanford, but perhaps few realize how much broader his interests were and how much excitement there was in his work in general. My personal acquaintance with Bill began during my first week at Stanford in 1937, when I began taking a course from him in Modern Physics. In ten con-



W.W. HANSEN

* R.B. Neal, editor, W.A. Benjamin, Inc., New York (1968)

secutive experiments, Bill tried to provide his students with some insight into the laboratory environment of contemporary physics. The first experiment consisted of repeating the tests that he and John Woodyard had just completed on the first experimental studies of the newly developed concept of cavity resonators. In the laboratory experiment, we were using the very equipment that had been used for research only weeks before.

Even though it may be trite today, the concept of a metal box acting as a resonant circuit seemed then to be incredibly ingenious, and it exhibited remarkably good properties. For a student to be taken into the forefront of contemporary research only a few weeks old was very exciting. I was indeed glad to be a student at Stanford. I remember writing Bill Hansen a personal letter thanking him for the unique experience he had provided.

Bill Hansen was born in 1909 in Fresno, California, where his father ran a hardware business. Bill developed an interest in technical and scientific things very early in life and preferred to play with mechanical and electrical toys, many of which he constructed himself. His father guided him into mathematical games and problems, and his mother was delighted in his interest in electrical equipment and toys. He was a very good student in high school, especially in mathematics, and he graduated at the age of 14. He wanted to become an electrical engineer, but feeling himself to be too young for college, he spent a year at Fresno Technical High School before finally coming to Stanford at the age of 16. He was delighted to be at Stanford where the atmosphere and informality combined with high scholastic standards made his progress rapid and satisfying. In 1928, his senior year, he became a laboratory assistant in the Department of Physics; the fascination of experimental physics caused him to change from engineering to physics.

The primary interest of the Physics Department then was in atomic physics, and in particular in the mechanism of x-ray excitation—work carried out under the guidance of Professors Kirkpatrick, Ross, and Webster. Soon his participation in research turned from that of an assistant to that of a collaborator, and this resulted in a joint paper published with a number of faculty participants. In 1929 Bill began his graduate work in the Physics Department. In his second year of graduate work he became an instructor and participated in teaching and research. With quantum mechanics still in the primitive stage, much of the progress in the field still depended upon careful experimentation. Bill participated in this work and lived through the era when the principles of atomic physics were satisfactorily resolved. Bill contributed substantially to the progress of this research, sometimes as an experimenter and

sometimes as a theoretician. He completed his Ph.D. research, writing a thesis on *Probabilities of K-Electron Ionization of Silver by Cathode Rays* which was published in 1933 with co-authors Webster and Duveneck.

Upon completion of his Ph.D. at Stanford, Bill received a National Research Fellowship which took him to MIT and to Michigan. Aside from broadening his perspectives, it provided him with an opportunity to think about future directions. While at MIT he came under the influence of Philip Morse and studied boundary value problems which were destined to play a major role in his future interests.

Bill returned to the Physics Department at Stanford in 1934 as an Assistant Professor of Physics. Here the situation had already changed, reflecting the progress in physics generally. Quantum mechanics was firmly established by then and could explain in detail atomic behavior. With the interest shifting from atomic to nuclear physics and with Chadwick's discovery of the neutron, there were new challenges and horizons, especially those of producing nuclear reactions caused by artificially accelerated particles. These events caused the faculty at Stanford to want to undertake research by bombarding nuclei with particles accelerated to energies on the order of a million volts, and Bill soon found himself immersed in these new studies. Largely because of the tradition of x-ray research in the department, it seemed natural to extend this new interest in nuclear reactions by examining electron-induced processes rather than those induced by protons or other particles. He soon became a member of a Physics Department committee which looked at a variety of ways in which higher particle energies could be obtained within the limited resources of the Department. The existing voltages in the laboratory were then limited to 200KV; these were first used to obtain weak neutron sources through the D-T reaction.

In considering various ideas for accelerating particles (principally electrons) to higher energies, Bill soon became convinced that the static devices such as Cockcroft-Walton generators would be limited by technical problems such as those of insulation. He decided to follow the ideas of Sloan at Berkeley, where radio-frequency voltages were used to accelerate particles. For this purpose resonant circuits containing inductors and capacitors were needed, with the limitations on voltages being due to unavoidable resonant circuit losses. This soon caused Bill to explore alternative resonant circuits, such as sections of transmission lines, and here his experience with boundary value problems at MIT led him to the conclusion that normal modes of vibration in closed containers could be used as resonant circuits and that a variety of shapes existed which would make

the Sloan accelerator more practical and efficient. Soon the word *Rhumbatron* was coined for the new kind of resonators that Bill was exploring. Whereas Bill's work on such closed resonators stemmed from his interest in acceleration of particles, it soon became obvious that his ideas would also be practical for other radio-frequency applications.

Soon a new development took place in the Physics Department which caused Bill to digress from his principal preoccupation; this was the arrival of his former roommate, Russell Varian, and his brother Sigurd, who wanted to undertake the invention and development of a new kind of radio tube. I shall tell the Varian story in a moment but would like to finish my description of Bill's interests first.

During the time that the Varian brothers worked at Stanford, Bill participated in the application of his *Rhumbatron* to the development of the klystron. The opening of a new region of the radio-frequency spectrum made practical by the klystron required a new kind of thinking which was not known to radio engineers. Bill also introduced new methods of measuring microwave power, wavelength and impedance. As an example of his ingenuity, I want to mention that he recognized that a multi-stage radio-frequency amplifier, when analyzed mathematically, had the formal appearance of a Laplace function which could best be studied by means of an electrolytic tank. This led to the unheard of process of designing radio-frequency amplifiers by studying static electric fields in a water bath.

By the time the war started, Hansen was already a skilled microwave engineer prepared to pursue a number of applications, which he did as a part time employee of the Sperry Gyroscope Company. His knowledge of the microwave field made him invaluable as a teacher, and he was employed as a special lecturer to help convert the hundreds of physicists gathered at the MIT Radiation Laboratory into skilled researchers in the microwave field. His lecture notes, then known as the "Hansen notes," were widely distributed and used to help many people become proficient in microwave engineering.

After the war Bill wanted to return to his first love, the development of the linear electron accelerator. In 1945 he realized that the wartime development of the magnetron coupled with his earlier ideas of the *Rhumbatron* gave a new promise to building a radio-frequency linear electron accelerator. With many of the principal features of the electron accelerator already in mind, Bill returned to Stanford in 1945 and began to pursue his ideas in earnest. He took steps to form the new Microwave Laboratory and invited a few of us to come to Stanford to find new directions in the use of

microwaves. These included, in addition to the electron accelerator, such diverse ideas as helping to develop the nuclear induction experiment with Felix Bloch and measuring the velocity of light with unprecedented accuracy by means of a *Rhumbatron* experiment. He also encouraged Marvin Chodorow and me to continue the exploration of microwave tubes and devices for a variety of purposes.

Bill reached one of his early goals in 1947 by demonstrating that his ideas for linear electron accelerators were practical. He showed close agreement between theory and practice by testing a $4\frac{1}{2}$ million volt electron accelerator. Long before this small machine was completed, he began to develop the ideas of building a billion volt machine. He convinced his associates that the ideas were promising enough so that much of the momentum of the laboratory turned to this specific objective.

Unfortunately, despite his appearance of vigor and strength, he began to develop a chronic lung illness which sapped his strength and limited his physical effectiveness. Only a few of his friends realized how serious this difficulty was. By 1948 he was compelled to wear an oxygen mask which he fashioned himself and which he had to carry everywhere.

Just as the various pieces of the billion volt electron accelerator project were coming together, Bill's illness became more acute, and in the spring of 1949 he died after months of chronic difficulties. His passing was a shock to his friends and associates not only because he was still so young but also because his incredible abilities as an applied physicist as well as a nuclear physicist were only beginning to unfold. It was not obvious how the billion volt machine could be completed without him.

The last period of Bill's life was made more satisfying by the pride he took in the work he was directing and the certainty with which he foresaw the successful completion of the large accelerator. He was doubly pleased that the others recognized the importance of his work; shortly before his death he was elected to the National Academy of Sciences.

I think of Bill Hansen as a truly remarkable man—a man of tremendous richness in spirit and ideas and an uncanny combination of mathematical skills and practical appreciation of engineering. He was an incredibly effective teacher and a stimulator of others. It is certainly to his credit that the foundation he laid for the development of the billion volt accelerator at Stanford could be carried to completion by others without unusual difficulties. Those of us who had the responsibility of completing his ideas certainly realized that our task would have been nearly impossible were it not for the

thoroughness of the theoretical and experimental planning that Bill put into this project.

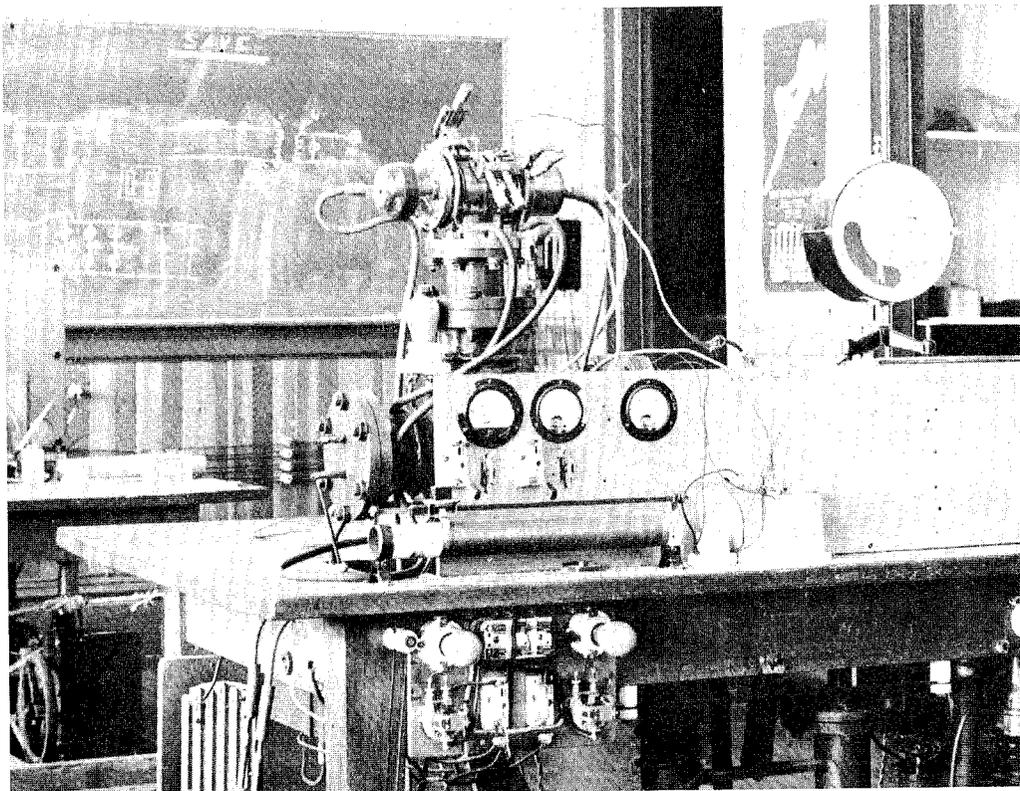
Varian and the Klystron

Let me now return to the period starting with the early 1930's and to the story of the Varian brothers. The elder brother, Russell Varian, was a graduate student at Stanford with Bill Hansen. Upon completion of his graduate studies, he took a job in San Francisco to help develop television systems. His younger brother, Sigurd Varian, did not have much scholastic training as he preferred the adventuresome life of a pilot. He bought World War I Jennies, repaired them, and flew from place to place, living a life not unlike that of Charles Lindbergh. In the course of time, he took a job flying for Pan American and helped to establish routes into Mexico and Central America. This was adventuresome flying to say the least, as there were few navigational aids, virtually no weather forecasting, and no devices to help with landing. He also became aware of the threat posed by the rapidly developing German Air Force. As a consequence of German and Italian participation in the Civil War in Spain, he believed that the devastation of Spain could spread to the United States, and he saw no effective way to combat the airplane raids that might be carried out from secret Central American bases against

the principal cities in the United States.

He discussed these issues with Russell through letters, and finally convinced him that the two of them ought to try to solve both of these perceived needs. Russell realized from basic considerations what was needed: a new radio tube that could generate waves short enough to be focused in the form of searchlight beams and that could penetrate darkness and clouds. From the known size of aircraft, probable scattering cross sections, and distances involved, it was easy to compute not only the wavelength of a desired radio tube but also its required power. Thus the central problem in their project became the innovation of a tube which seemed to others to be impossible because of fundamental transit time limitations in conventional tubes and because of the losses of resonant circuits.

In 1935 the two brothers established a small laboratory on the coast of California in Halcyon, where they had spent much of their youthful years. The first year of their work brought negligible results and led them to a state of discouragement. They decided to attempt to move their laboratory to Stanford University, where they felt that Bill Hansen and other faculty might be helpful in stimulating their work by discussion and criticism, and where the facilities would be better for experimental progress. In discussing their



Klystron development work in Room 404 of the Stanford Physics Department.

ideas they gained the support of President Wilbur of Stanford. Soon arrangements were made to have the Varian brothers continue their work in Room 404 of the Physics Department, where they would work without salary but with a contribution of \$100 for materials and supplies. In return, Stanford was to receive half of the royalties from any inventions that were made.

Working in the Physics Department for a few months was all that was needed to bring the results to an exciting conclusion. After Russell had devised some twenty two different schemes which proved to be impractical, the twenty-third idea was evaluated by Bill and found to be most promising. This was the idea which overcame the difficulties of the conventional tube because it made use of velocity modulation and thus made the transit time effects a useful mechanism instead of a basic limitation; and, of course, Bill's Rhumbatron cavity was just what was needed to overcome the losses in resonant circuits of the period. Sigurd built a model of the newly invented klystron and found that it worked almost immediately—in August of 1937.

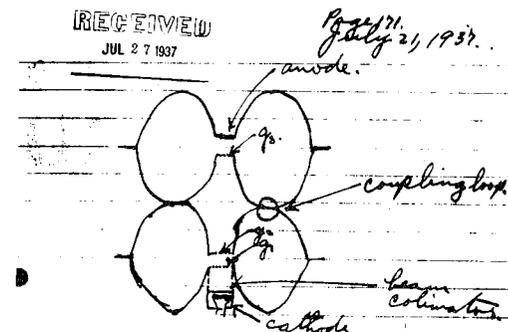
For the next 3 years Bill Hansen, the Varian brothers, and a number of graduate students worked on various aspects of the klystron design to convert the Varian invention into a practical radio frequency device. We demonstrated a large number of klystron tube types, the use of the klystron as a microwave detector, as a superheterodyne receiver, and as a power amplifier; and we tested its utility for continuous wave radar, instrument landing, and point-to-point communications. By 1940 the Varian brothers had realized their dream, and the klystron was ready to take its place in the rapidly developing program to help make microwave radar practical. The principal role of the klystron in wartime was that of the local oscillator developing just a few milliwatts of power. As such it was a partner in radar systems to the magnetron which, by the end of the war, could produce more than 1 megawatt of pulse power.

Because of the importance of the klystron and its possible application in a variety of military needs, the Stanford group was asked to move to the East Coast, where it helped convert the mechanical engineering specialties of Sperry Gyroscope Company into a modern electronics organization which participated in the development of many kinds of radar systems. At Sperry the Stanford group supervised many of the microwave research activities of the company and helped develop not only the klystron tube itself but also its applications in Doppler radar, instrument landing, and similar applications.

At Sperry the Stanford group believed that the klystron could also be made to generate very large

amounts of power, but the wartime applications of the klystron did not require such development. It was my fortune to travel to England during the war to exchange information on microwaves and Doppler radar. While on a visit there in 1944, I found the EMI Laboratories had developed a klystron capable of producing 20,000 watts of pulse power. This device was so simple and successful that it occurred to me to try to extend the work of EMI at the first opportunity, and to try to generate levels of power which would exceed that of the magnetron. Since the klystron was basically an amplifier, many klystrons could be operated in parallel, thus suggesting the possibility that unlimited amounts of microwave power could be generated if that were needed.

In odd moments at Sperry, Bill Hansen and his associate John Woodyard began to re-examine the feasibility of building a linear electron accelerator with the aid of the wartime magnetron and decided that such a device would be a good start towards making the accelerator a useful device for physics. Hansen and Woodyard were not the only individuals who believed this to be possible; several sizable projects in the United States and England were started with similar objectives. As a separate matter, a successful proton linear accelerator was being built at Berkeley under the direction of Luis Alvarez. This machine, in addition to becoming

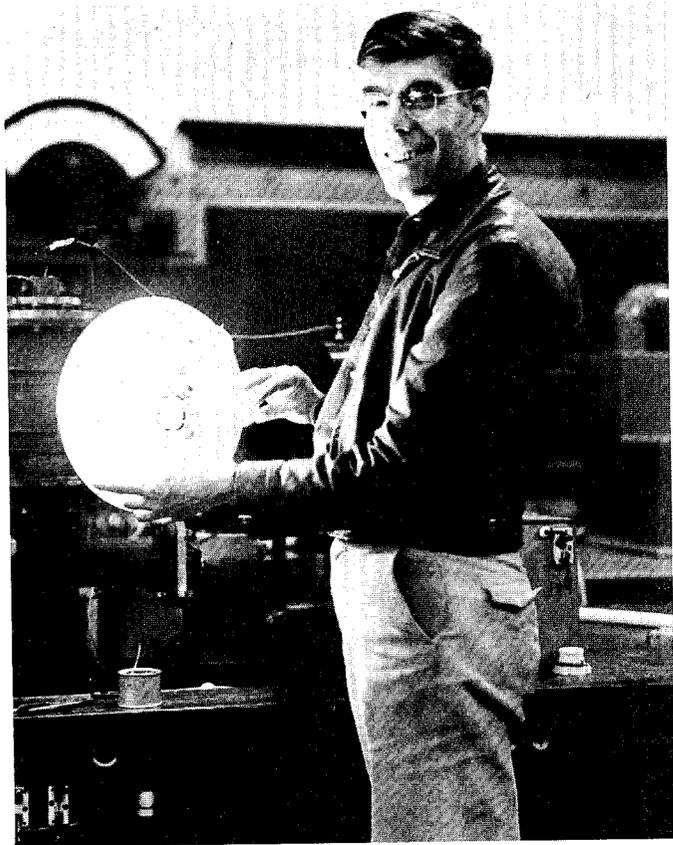


R. H. Varian, July 22.
A Rhumbatron Oscillator or
Amplifier using a single
Rhumbatron.

● Hansen has found that he
can calculate the case of a
spherical rhumbatron with
one cone reaching to the center.
Such a rhumbatron might
make it possible to make an
oscillator or amplifier. If
one could introduce electrons
into a rhumbatron.

R. H. Varian.

The basic idea of the klystron as recorded in Varian's notebook.



SIGURD VARIAN

an important tool in its own right, served as a prototype for all the linear accelerators now injecting protons into large circular accelerators. The magnitude and purpose of the proton accelerator work at Berkeley encouraged Bill Hansen to attempt to build a large machine as well.

Shortly after his return to Stanford, during the period of 1946-47, Bill Hansen and three graduate students were able to demonstrate both theoretically and practically that the acceleration process in a disc-loaded waveguide was feasible, and construction of various models and prototypes began. By 1947 electrons were accelerated, and eventually the structure was extended to become a 12-foot machine producing 6 MeV electrons when powered by a 1 megawatt magnetron. We called this accelerator Mark I, the first of about ten accelerators to be built at Stanford. In reporting his results to the Office of Naval Research, Bill's progress report contained only four words, "We have accelerated electrons." The Chief Scientist of ONR, Dr. Emanuel Piore (in the audience today), must have chuckled.

In parallel with the accelerator activity in the newly formed Microwave Laboratory, Marvin Chodorow and I could not resist beginning work on the extension of the EMI ideas of building a very large klystron. Our objectives were twofold: to see what limits, if any, existed in generation of power by means of a klystron, and to try to build a klystron which might be suitable for use with the linear electron accelerators being investigated by Bill Hansen.

Shortly after his four-word report to the Office of Naval Research on acceleration of electrons, Hansen began to think of extending his ideas to the construction of a truly large accelerator. His colleagues supported him in a general way but also more specifically by encouraging him to believe that a truly high power klystron was possible.

With amazing courage, while already ill, Bill Hansen prepared a proposal in March of 1948 entitled *Proposed Development of a Billion Volt Linear Electron Accelerator* and submitted it to the Office of Naval Research for consideration. I should like to read the front page of this proposal.

The problem of production of high energy electrons by means of linear accelerators has been under study at Stanford University under ONR auspices (ONR Contract N6-ROI-106) for over a year. In this time we have come to understand the theory, the constructional problems, and the operation of such a device quite well, and feel that the present operational accelerator which produces 4.5 MeV, with a single stock magnetron, and with 119 untuned sections, demonstrates this understanding. With this background of experience, we have been studying the design of an accelerator capable of producing electrons of an energy of one billion volts or more and are now submitting a design which, we believe, will surely achieve this. One cannot, of course, guarantee success in any venture and when the proposed voltage is 220 times that presently realized, one must put great reliance on theory and in one's judgment. Nevertheless, we believe that the design is as safe as anything can be that has never actually been tried and feel confident that the design voltage can be obtained.

carried out including the development of the theory of velocity modulation at relativistic velocities. Many of the needed components were studied in substantial detail and led us to the conclusion that the prospective klystrons should work. Some of these topics could be studied theoretically, but others had to be studied experimentally. For example, we needed to know whether a pulse transformer could be made to deliver 400,000 volts and whether the cathode bushing on a klystron could be made to stand such a voltage. The dielectric strength of materials at such voltages and with short pulses was not known, and we were compelled to carry out suitable experiments ourselves. The key piece of equipment we devised for testing dielectric strength was a pulser built by us at the Ryan High Voltage Laboratory. We used an existing overhead transmission line, charged it to a million volts from existing power transformers, and discharged the line through a one-meter sphere gap into the load under test. This machine was indeed awesome, and the noise it made was memorable. A few of you, I am sure, will remember these experiments. They were as impressive as anything I have ever seen in my life, before or since.

Now, helped by the knowledge of dielectric strengths, a modulator and a pulse transformer were designed and built. The work was done by graduate students who had had no prior experience in this field. Fortunately, the designs worked and we were able to obtain voltages up to 500KV. The first klystron we designed and built in 1948 gave no emission, and upon opening the tube up for examination we found failure in the glass bushing. Many guesses were made concerning the cause of the failure, and changes were made in design on the basis of averaging the group opinions. The second tube built failed for other reasons, and again additional changes were made. This time we found that the tube emitted electrons as expected and that some amplification was possible. After months of disappointments we finally tested enough tubes to be more sure of our grounds.

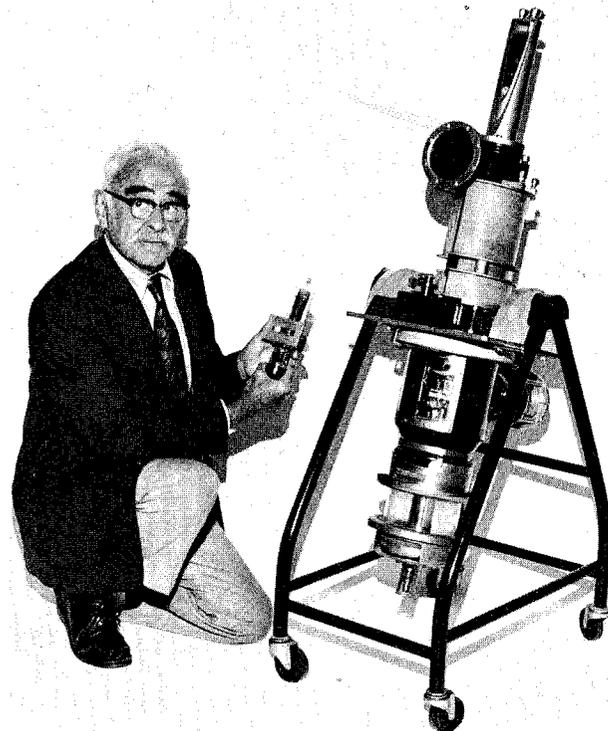
Marvin and I remember in March 1949 the good news we received of the operation of the new klystron at full power. He and I were attending a convention in New York City and received a telegram from Professor Sonkin which we remember said something like this, "The new 14 megawatt baby was born this morning and is doing nicely." We now were certain that we could produce between 5 and 15 megawatts of power output and that the results were comparable with those predicted by theory. The various difficulties were gradually resolved but only after a great deal of work. Retrospectively, we now look upon our work with a great deal of satisfaction. As luck would have it, extrapolation of power output from a klystron tube by a factor of 2000 proved to be practical. It was indeed a

pleasure to report our success to Bill Hansen towards the end of his life, as this accomplishment was a keystone to his ideas of building the billion volt accelerator.

Meanwhile, the development and construction of the 1 GeV accelerator itself continued. The work itself was carried on principally by one faculty person and 29 graduate students, together with a supporting staff of about 35 mechanical and electrical technicians.

Mark II Accelerator

In order to make the work move more smoothly, we decided to build a prototype accelerator which we called Mark II. It was to use a single klystron, a 14-foot accelerator structure of the type to be used in the 160-foot machine, and many of the components being made ready for the billion volt accelerator. The Mark II accelerator operated in 1949 and achieved energies of about 33 MeV. Its success was a milestone in our program and greatly reduced the general anxiety. Incidentally, the Mark II machine was used for a variety of research purposes and, finally, after being refurbished, was sent to Brazil where it was used for research for some time.



Professor Marvin Chodorow holds a typical small klystron next to a model of the large multi-megawatt klystron developed for use in linear electron accelerators at Stanford.

Mark III Accelerator

The fabrication of the Mark III accelerator (as the billion volt machine was then called) proceeded rapidly once the general uncertainties of the klystron and the Mark II machine were resolved. The Mark III accelerator first operated on November 30, 1950, with a total length of 30 feet powered by three klystrons running at the 8 megawatt level, and produced a 75 MeV beam. Not much by present standards, but it was an eventful day.

In the following months the accelerator grew in length as additional sections were completed. By April 6, 1951 the Mark III reached 80 feet and delivered 180 MeV. An important change in our plans was then made: to use the partially completed machine for research.

The construction of the Mark III accelerator now proceeded at a rate that was determined by the need to apportion the available funds between research and construction. Part of this research is well known to you, as it resulted in the Nobel Prize in Physics awarded to Professor Hofstadter for his pioneering studies of electron scattering to determine the size and structure of the nucleus. Other important research topics were conducted under Professor Panofsky's guidance. The highest energy obtained with the 80-foot portion of the Mark III was about 200 MeV on January 14, 1952.

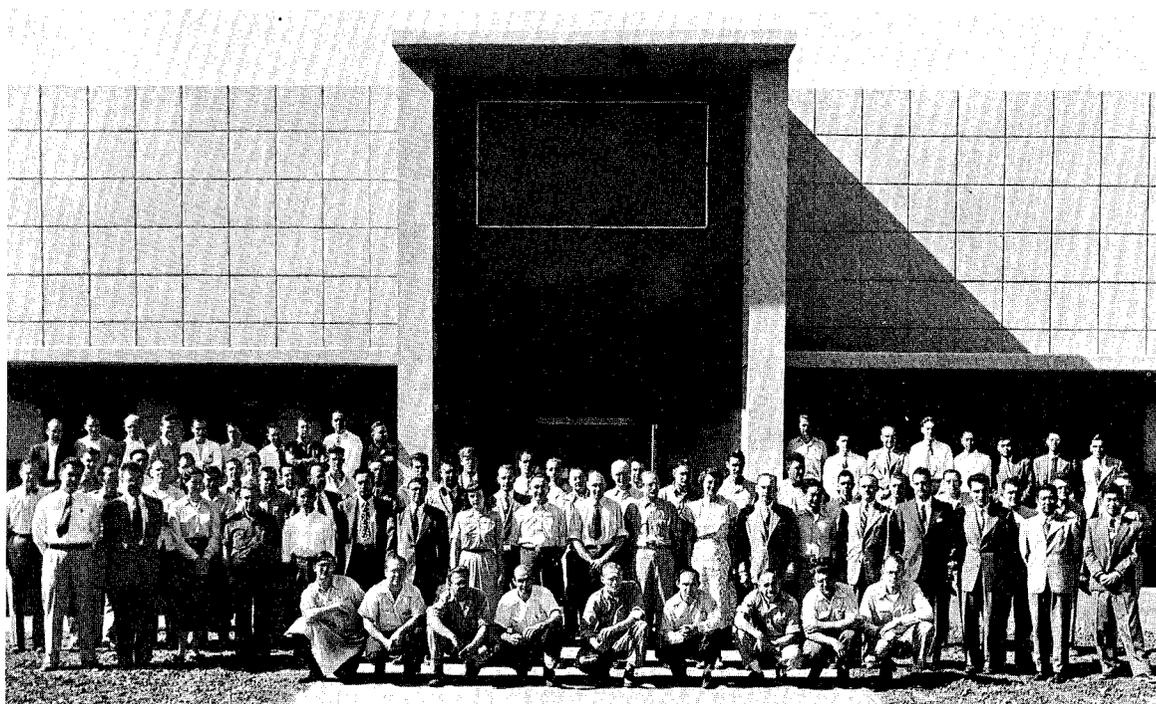
It is difficult to say whether Bill Hansen's early objectives were fully met by the progress up to that time. The construction proved to be relatively straightforward, and operation of the partially completed machine was satisfactory enough to allow research to go on. Nonetheless, the reliability of the machine was poor, and the so-called pessimistic lower limit of 1 GeV with 160 feet as conceived by Hansen was not realized. But there was no question that the main principles were proven.

By the end of November 1953, the Mark III had twenty-two 10-foot sections in place and produced about 400 MeV with 14 operating klystrons. By December of 1955 the accelerator was operating routinely with a full complement of 21 klystrons and yielded 600 MeV electrons. By December 1957 an additional 90-foot extension was proposed; this was completed in 1960 thus permitting routine operation up to 900 MeV. When all klystrons were fully effective, 1 GeV could be obtained.

I should now like to turn to the events which preceded SLAC.

SLAC

As soon as Mark III had proved its utility and potential in mid-1952, the idea of building a much larger machine was a matter of common discussion. To my



The staff of the original Microwave Laboratory at Stanford. The building was later remodeled and became the High Energy Physics Laboratory of the W.W. Hansen Laboratories of Physics.

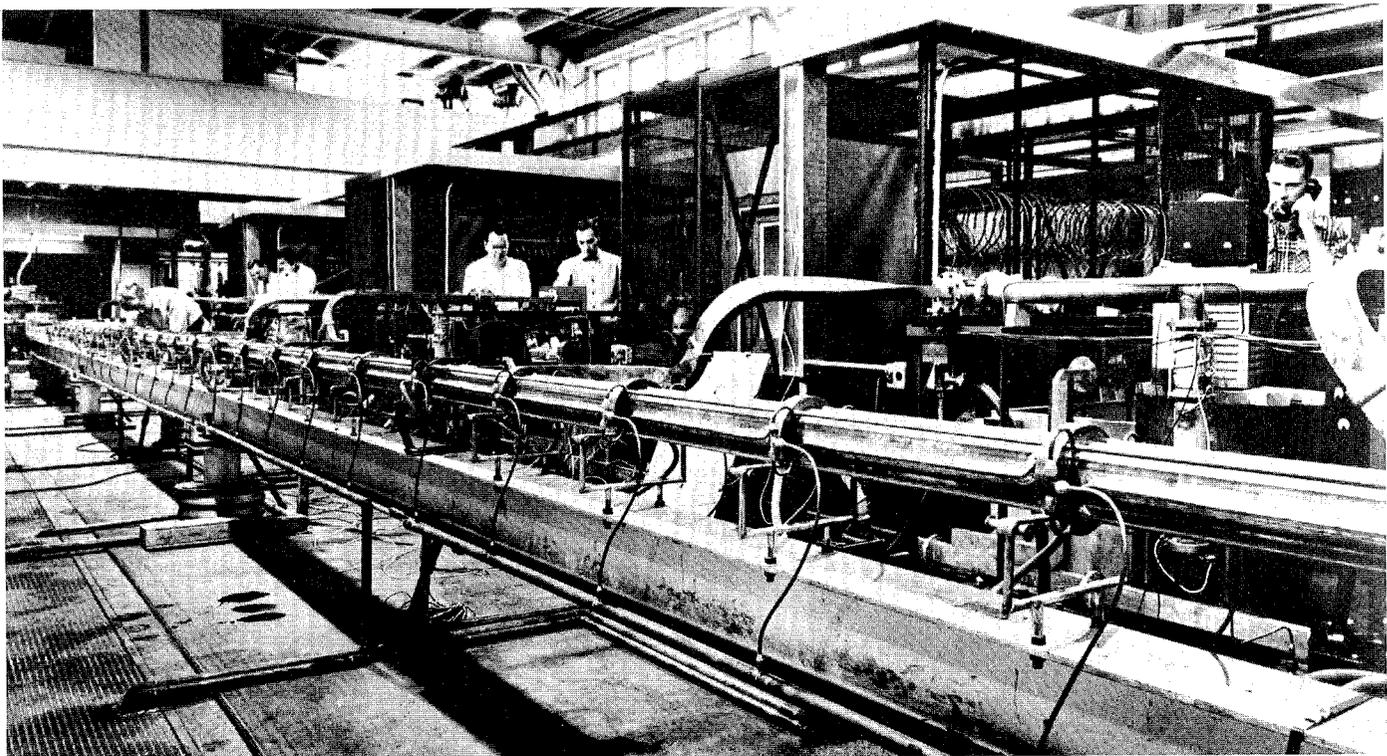
knowledge the first serious suggestion of building a machine much larger than the Mark III was due to Professor Hofstadter in 1954. He conceived of a machine perhaps ten times larger than Mark III and gave a number of plausible arguments for its need and success. At about the same time we were able to obtain support from the Atomic Energy Commission to build still another accelerator, called Mark IV, which was to be a 20-foot, 80 MeV, 2 klystron machine to explore a variety of accelerator improvements. Clearly, we wanted to push the technology further to improve the existing accelerators and to lay the groundwork for additional improvements which would be essential in the construction of a much larger machine. With the Mark IV accelerator we could do much experimentation that was no longer possible with the Mark III after it was dedicated to research in physics. In addition, Mark IV was used extensively for initial studies of electron therapy of cancer under the direction of Dr. Henry Kaplan of Stanford University Medical School. In the early stages of SLAC itself, the existence of Mark IV was invaluable in testing individual components.

With a bee in our bonnet and with Mark IV under construction, ideas for a much larger machine began to take more concrete form. Throughout 1955 and early 1956 a number of individuals from the Physics Department and the Hansen Laboratories met to dis-

cuss the wisdom of taking the idea seriously. This group agreed that a large machine would be feasible, scientifically justifiable, and economically practical. After all, if a Mark III could produce 1 GeV at a cost of \$961,000, then a 10 GeV machine could obviously be built for \$10 million—a real bargain.

It was finally agreed that the idea of building a much larger machine should be taken seriously, and a formal study was undertaken by our group early in 1956. We have a record of a Project M meeting at Panofsky's home on April 10, 1956. In addition to the Physics Department faculty, about eight others attended and formed the nucleus of the study. The Minutes of the first meeting showed that:

The purpose of this gathering, the first in a series of weekly meetings, was to discuss plans and form objectives which will hopefully lead to a proposal for construction of a multi-GeV linear electron accelerator. The participation of members of this group is entirely voluntary and on their own time as there are no funds available to support this program ... Should such a program materialize, it should be administratively distinct from Hansen Laboratories and the Physics Department.



A section of the Mark III accelerator at Stanford, shown without its concrete shielding blocks.

Professor Ginzton has agreed to serve as Director of the proposed accelerator activity during the design and construction phases and Professor Panofsky as Assistant Director for at least one year. Professors Schiff and Hofstadter would act as consultants.

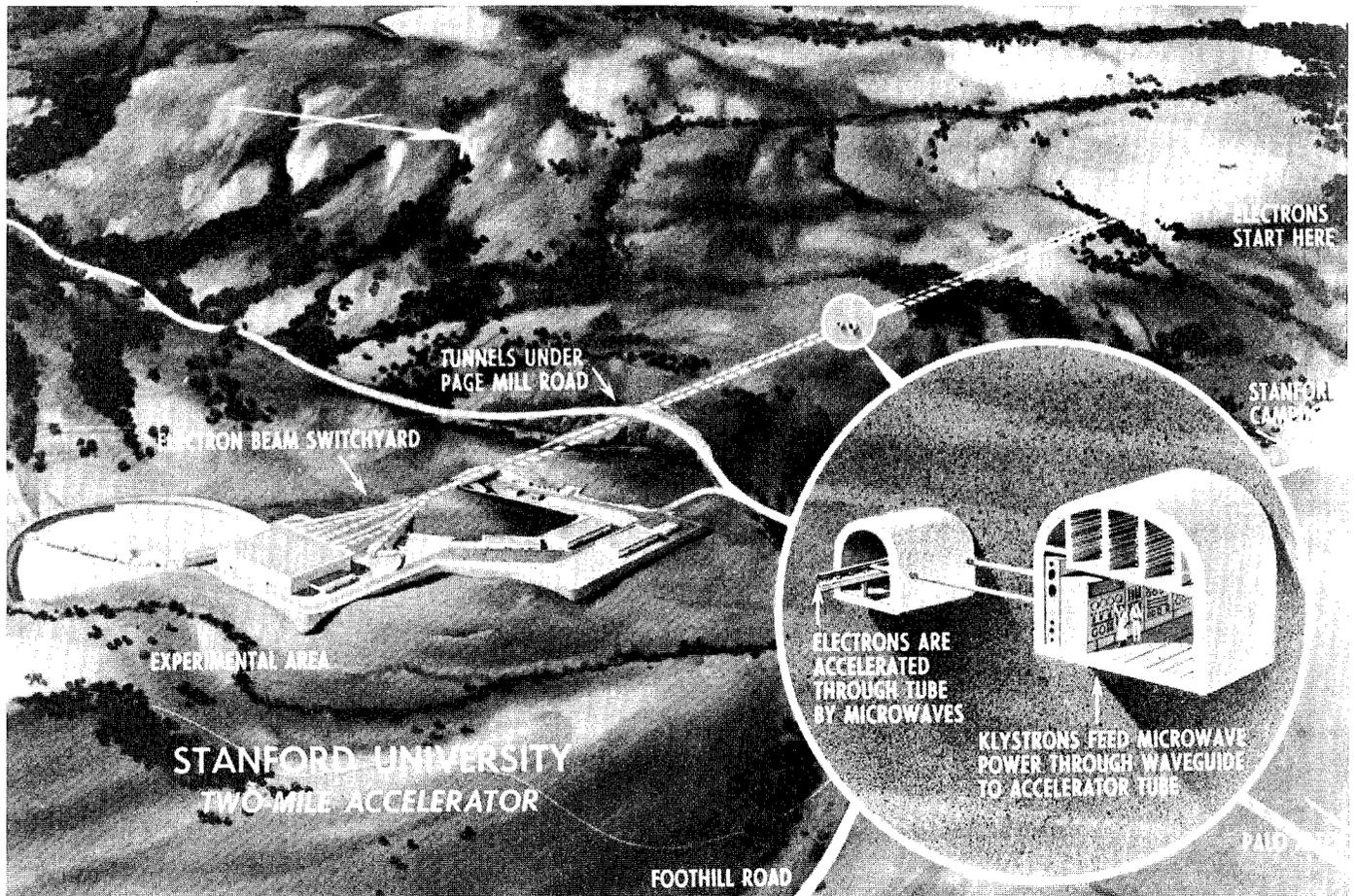
The Minutes also show that

The primary objective of the proposed large accelerator was declared to be basic physics research. There should be no security measures except to protect personnel and property, no classification, and freely published results; the facilities should be available to qualified research

visitors ... The following possible accelerator characteristics were listed to orient future thought: length 2 miles; energy 15 GeV; expandable to 50 GeV

We readily agreed to call the new machine "Monster," although a few of us now seem to doubt that that was our intention. In passing, we were often asked how we arrived at the various desired characteristics. The full scientific answer would take too long to give now, but the length was 2 miles simply because that was the longest straight path we could identify on the map of the Stanford grounds.

During 1956 a great deal of work was done by the study group with the assistance of many other individuals and organizations. A number of special studies were carried out to investigate beam dynamics, review plausible accelerator structures, components and the like. Much time was spent to investigate the practicality of an available site at Stanford, and members of the



The site originally proposed for "Project M" (SLAC) and the original scheme of housing the machine in side-by-side tunnels.

Civil Engineering and Geology Departments provided much help and support in carrying these studies to completion. Bechtel, Utah Construction, John Blume and Associates, and Varian were also most helpful.

Our informal group met many times in subcommittees to study individual problems and as a committee of the whole to hear reports and take stock of progress. Many of these committee meetings took place at Rossotti's Beer Parlor on Alpine Road. It is hard now to convey the spirit of these meetings. There was a sense of excitement, of promise, and of personal pride in participating in a project of great importance. Good beer didn't hurt any.

The drawing at left shows the site of the original proposal for our machine. It ran parallel to Foothill Boulevard (now Junipero Serra) and was to be underground from its beginning near the golf course to an end station near Page Mill Road. The idea for placing the machine there was largely my own, as the land seemed to be available, close to the campus, and permitted the use of tunnel construction. The idea of building the machine in tunnels was also largely mine. On a trip across the United States by train, the background music was interrupted shortly after leaving Denver with a description of the seven-mile tunnel which the Union Pacific Railroad then used. The costs of construction were given to impress the passengers, and these costs seemed so low that placing our accelerator into tunnels was almost an obvious idea. The use of tunnels was indeed attractive, as this method of construction could also provide the needed shielding. This continued to be a viable suggestion until geophysical exploration proved the soil conditions at the site to be unsuitable for tunnel construction.

As a result of the studies conducted in 1956 and early 1957, a formal proposal for the two-mile accelerator was prepared and submitted by us in April 1957 to the Atomic Energy Commission, the National Science Foundation, and the Department of Defense for their consideration. The proposal was analyzed by several advisory committees of the government which recommended that the project be undertaken.

Sometime in the spring of 1959 President Eisenhower decided that the AEC should be assigned the responsibility of administering the possible project at Stanford because of the extensive experience of this agency in construction and operation of large accelerators. While the AEC was not actively seeking the responsibility for this undertaking, it expressed a sincere appreciation of the importance of the program and a willingness to support it.

The Congress authorized the project on September

15, 1961, after four years of protracted study, hearings, and negotiations. SLAC does exist today but it is probably not commonly known that there were many times when the future was in grave doubt. During the four years of uncertainty and frustration, some time was spent, justifiably, to ascertain the scientific merits of the program, to review the ability of the nation to support still another major undertaking, and to review the details of the project, such as its cost, location, management, and the like. But part of the time was also spent on matters of less obvious importance, largely of a political nature. In part, this was due to some unrelated circumstances. Congressional hearings held in July of 1959 to assess the merits of the Stanford proposal, at the end of a congressional year, were pre-empted by the May 14 address of President Eisenhower in New York at the AAAS meeting. He announced that he was recommending to the Congress the construction of the Stanford linear accelerator. The Joint Committee on Atomic Energy, then in session, wanted to know why the President could not have informed Congress of his intention to approve the program earlier in the year, so that an orderly process of consideration could be pursued.

Almost immediately the project became hostage to a political argument involving something which was later called the "Hanford Compromise." It seems that a reactor at Hanford was to be built both to supply plutonium and to provide electrical power. In those days, President Eisenhower was opposed to further development of public power and therefore did not want to approve the Hanford proposal; the Joint Committee, on the other hand, took the position that it would not approve the "President's Stanford project" unless the Hanford project was also approved. Initially, this was not a public debate, and it was hard for us to understand the mysterious forces at play. While the President and Congress fought over the public power question, the Joint Committee put many delaying roadblocks in the way of the Stanford proposal by posing questions that seemed to us to be irrelevant.

As an example, Senator Anderson wanted to know why the government should now be asked to put a large facility at non-sectarian Stanford when it was just in the process of turning down another facility at Catholic Notre Dame. The question of klystron royalties and the role of Varian Associates and the Varian brothers were also thrown in as issues to be explored, because Congress obviously did not want to authorize money to flow to private parties as a result of the Stanford program. Among the things that proved to be controversial and difficult was the fact that Senator Anderson thought that the Varian brothers would gain financially from construction of the machine. Even though the facts were badly confused, this concern on his part was

easy to anticipate because I was then both the Chairman of the Board of Varian and the Director of Project M. To relieve Congressional concern on this issue, Varian Associates voluntarily adopted a Board resolution disqualifying itself from any participation in Project M contracts. Even this action, however, did not entirely satisfy Senator Anderson and he found other reasons for concern.

During this period of Congressional Hearings many other issues came up for consideration, such as the suitability of the site at Stanford because of the earthquake hazard—at least a partially well-taken concern. Another was the general location: why should the project be at Stanford and not some other place? And, if tunnels are to be used, why drill new tunnels; why not use some abandoned tunnels such as those in the state of Washington or in Nevada or even in West Virginia? Many of these questions could not be answered without appropriate studies and frequently the Atomic Energy Commission appointed special task forces to explore the merits of alternative suggestions or possibilities. As expected, the Stanford position was that the accelerator was a Stanford proposal and we believed that it was imperative for the machine to be built at Stanford. Somehow, eventually, we prevailed.

Parenthetically, it is difficult to identify the actual

act of Congressional approval of the Stanford project. In reviewing the Congressional Record of the time, one finds that Congress finally agreed to decouple the Hanford electrical power reactor from the remaining items of the AEC budget which included the Stanford proposal. The actual authorization of the program, even after four years of study, never took place in an explicit form.

I do not want to leave the impression that the four years between the first request and final authorization was a wasted period. Under AEC support the Mark IV program continued to provide important information in regard to the development of klystrons, modulators, accelerator structures, instrumentation, and still other items of importance. The time spent also was used to clarify the suitability of various sites at Stanford and to select the most attractive method of construction. The parameters of the proposed accelerator also did not remain unchanged. For example, the 1957 proposal was based upon a design employing 480 klystrons which were to be used at 6 megawatts in order to obtain a reasonable life. During the pre-construction period, it was possible to change our minds and reduce the number of klystrons to 240 without changing the energy of the machine. Experience with klystrons gave more certainty about their power output and life, and reduced considerably the eventual cost of the machine.

Palo Alto Times

AN INDEPENDENT NEWSPAPER

Palo Alto News and Palo Alto Shopping Review

PALO ALTO, CALIFORNIA, FRIDAY, MAY 15, 1959

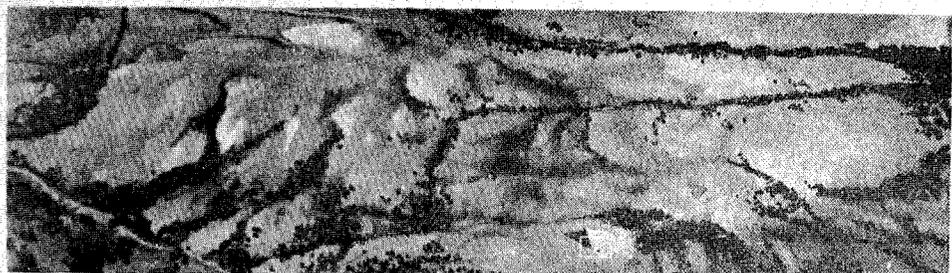
Phone DA 6-1200

10¢ Copy
\$1.50 month

Ike to ask \$100 million for Stanford A-smasher

Building time set 6 years

President Eisenhower will ask congress during the current session to approve con-



The final contract entered into between Stanford and the Atomic Energy Commission provided 114 million dollars for construction of the machine and 18 million dollars for pre-construction research and development. It would take a long time to fully justify these numbers. In part they were very accurate estimates on the part of Dick Neal and others; in part they were just guesses. At one time, while testifying in Washington, I was called at midnight and told that President Eisenhower would not approve a machine if it were to cost more than 100 million dollars. Knowing that victory was near, I asked, "Did President Eisenhower really mean exactly 100 million dollars. Wouldn't 105 million do just as well?" The answer at midnight was, "Okay, but no more." The extra 5 million helped.

Getting Under Way

What happened thereafter is a long, long story. Several thousand people at Stanford and elsewhere were employed in bringing about the completion of the accelerator. Professor Panofsky will describe some of the key events in his talk later on today, so I shall stop now.

After 20 years of construction and operation and important additions to the plant, it is clearly possible to say that the scientific success of the program clearly supports the initial rationale for building the accelerator, even though some of the specific arguments used in the early days may not have been entirely correct. During one of the Congressional hearings I was once asked, "Dr. Ginzton, can you tell us precisely why you want to build this machine." As I remember, my answer then was, "Senator, if I knew the answer to that question we would not be proposing to build this machine." It was a snap answer, but thinking about it now, I believe that it was the best answer I could have given on the spur of the moment and that it was as good an answer as any.

I am delighted to have had a part in the accelerator history at Stanford. It meant a lot to me personally to participate in this exciting program and to know that others have carried on the construction and research so successfully. I am sure that Professor Hansen would indeed be proud to see such a happy conclusion to his early dreams. Happy Birthday, SLAC!

EDWARD L. GINZTON

The following biography is adapted from Modern Scientists and Engineers, McGraw-Hill, 1980.

While still a graduate student at Stanford, Edward Ginzton was asked by the Varian brothers and William W. Hansen to work in the physics department to help explore the characteristics of the klystron. This research led to what is probably the outstanding accomplishment in Ginzton's career, the demonstration of the high-power klystron.

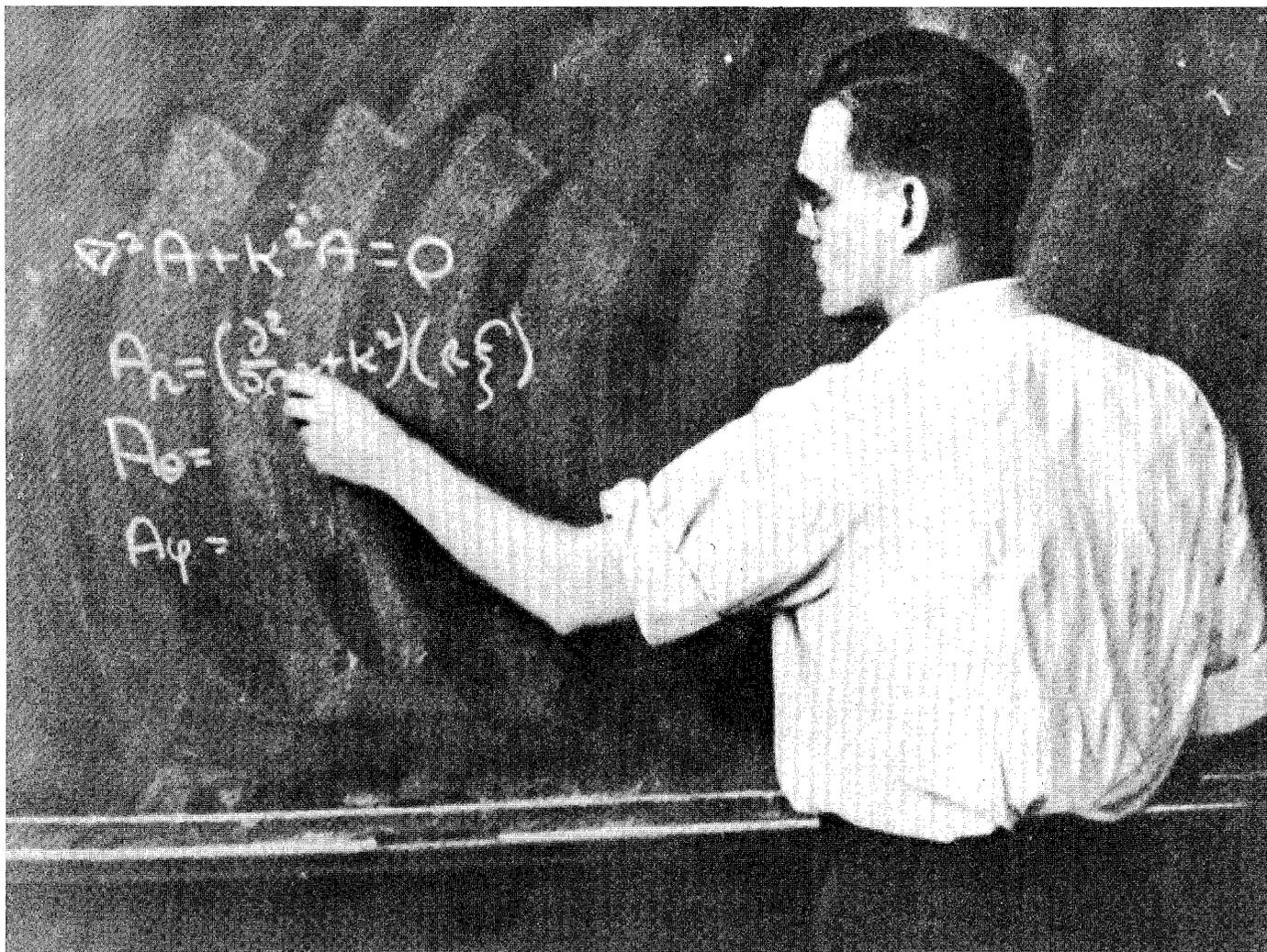
In 1957 a group of Stanford physicists and engineers began to study the practicality, usefulness, and costs of a machine several miles in length. Under Ginzton's supervision the preliminary design of this machine was finished, and the project received congressional approval in 1961.

In 1948 Ginzton became deeply involved with the formation of Varian Associates. He was appointed chairman of the board in 1959, was president from 1964 to 1968, and served as chief executive officer from 1959 to 1972.

For his contributions toward the development of the linear accelerator, Ginzton was awarded the Medal of Honor of the Institute of Electrical and Electronic Engineers in 1969. He was elected to the National Academy of Engineering in 1965 and the National Academy of Sciences in 1966.



EDWARD L. GINZTON



ACCELERATOR PIONEER BILL HANSEN IN THE CLASSROOM

This article was based on a talk given by Edward L. Ginzton at SLAC's Multi-Anniversary Celebration held on August 14 and 15, 1982. Subsequent articles tracing the construction, linear physics program, and the development of the storage ring are planned. These will appear as Special Issues of the SLAC monthly Beam Line.

-Bill Ash, Editor