I first met Russell Varian in 1939 on the stairway landing of the Stanford University Physics Building. I had just been hired as a part-time research associate on the new Klystron Project at a then munificent $90 per month, and was going upstairs to meet the Varians and their small staff. I had heard of the invention of this new electron tube that could generate centimeter waves, but I did not know how it worked, and indeed I had never even seen one. Thus, when I stopped and introduced myself to Russ, he thought the first order of business ought to be an explanation of the principle of the klystron, and this he proceeded to do without any hesitation. I remember his saying something like this:

"Just picture a steady stream of flow of cars from San Francisco to Palo Alto; if they left San Francisco at equal increments and at the same velocity, then even at Palo Alto they would be evenly spaced and you could call this 'a direct flow of cars.' But suppose somehow the speed of some cars as they left San Francisco could be increased a bit, and others could be retarded. Then, with time, the fast cars would tend to catch up with the slow ones and they would bunch into groups. Thus, if the velocity of cars was sufficiently different or the time long enough, the steady stream of cars would be broken and under ideal circumstances would arrive in Palo Alto in sharp groups. In the same way, an electron tube can be built in which the control of
the electron beam is produced by this principle—bunching—rather than by the direct control of the grid of a triode."

This direct and simple explanation was an inspiring introduction to Russ and to the Klystron Project; it was destined to lead us to a lifetime profession, illuminated by close friendships with Russ, Sig Varian, Bill Hansen, John Woodyard, and others at Stanford and, later, at the company we started, Varian Associates.

I wish to trace the genesis of the Klystron Project in this article not only because it was such an important milestone in electronics but because, with the benefit of hindsight, it can be seen as practically a textbook demonstration of the validity of some of today's best known axioms about invention and the "management of technology." It demonstrates, for instance, the wisdom of being "coupled to the marketplace," and of identifying societal or market needs rather than merely advancing technology for its own sake. It also illustrates the benefits of working in a creative research community rather than in small groups or in isolation.

The klystron was invented during the summer of 1937 and announced formally to a world on the brink of war by the Varians in the February 1939 issue of The Journal of Applied Physics. Their letter began:

"The very efficient high frequency resonators described in this Journal by Hansen have been made the basis for construction of amplifiers and oscillators of a new type in which the transit time of electrons,
which is usually considered as a source of serious difficulties at very high frequencies, has been turned to constructive use."

Dramatically, this somewhat diffident announcement was apparently overlooked in Germany—but not in England. Already deeply involved in the development of radar, scientists at Bristol University recognized that this ingenious new development would help make airborne radar possible by providing a lightweight source of microwaves for radar receivers. By late 1940—just as the Luftwaffe was switching to deadly night bombing—the RAF succeeded in equipping its night fighters with the klystron radar receivers that would help make it possible to win the Battle of Britain, and also made visible, for the first time, detection of submarines and thus broke the blockade of Britain.

The klystron turned out to be more than an important wartime development, however. It was destined to play an important part in developing the new industry that is now generally referred to as microwave. It helped make commercial air navigation safe, it opened the possibility of worldwide communications for satellites, and it led to a variety of high-energy particle accelerators useful in medicine and in nuclear physics. It thus spawned a new technology and then a whole new industry. On the personal side, it represented the fruits of an ideal collaboration between three remarkable men, each uniquely talented: Russell and Sigurd Varian, and William Hansen. Since their backgrounds are essential to appreciating this collaboration, I will
Russell Varian was the oldest of three brothers who grew up in what must have been an unusually supportive and intellectually stimulating environment (see editorial box). A tall, awkward boy, he had a remarkable ability to comprehend what he read, which more than compensated for a marked slowness in learning the mechanics of reading. Although he was kept back until he was some four years older than many of the other students with whom he entered high school, he was in many ways much more advanced and better informed. The science courses especially excited him, and as a teenager he was already able to explore complex scientific questions on his own.

The depth of his ability to explore a subject was matched by the diversity of fields he was anxious to explore: not only mathematics and physics, but as he grew older, economics, medicine, biology, and chemistry held all equally fascinating for him. Frequently he would pursue what he was learning in several directions, seeking possible alternatives to a given conclusion, whether they might not lead to a different result. He could explore these in his own mind to see if different assumptions or different alternatives in the logic might not lead to a different result.

After graduating high school with an excellent knowledge of science and related subjects, Russell was admitted to Stanford where he worked his way through. He took a normal curriculum but continued to have difficulties with mathematical calculations and with subjects which required much reading; he always understood
the principles but had difficulty in working out assignments. At the end of his sophomore year he decided to major in physics as he thought this would require the least reading! At the university he met some very able students, including Bill Hansen, and although he had to spend much more time than they on homework, his slowness did not prevent him from gaining a substantial depth of understanding and graduating with an M.A. degree in physics in 1927.

In contrast to Russell, Sigurd Varian had little interest in college. Three years younger than Russell, he had been—and would continue to be—the activist of the pair. Whereas Russell tended to be shy and absent-minded, Sig was gregarious, good-looking, full of life and humor, and an incredible story-teller who was never reticent about taking the center of the stage. Strongly inclined mechanically, he would be the one to build whatever models Russell might design so that he and his younger brother, Eric, could play with them. Sig would often initiate a project by asking his brother how or why something worked. This would start Russell figuring out the
answer, and frequently he would invent a different approach. As young men they thought about a different way of building a radio compass for air navigation; an airplane engine synchronizer; a groundspeed indicator; an altimeter; and a variety of other airplane instruments and devices. Although few of these ideas proved of particular worth, and there is no evidence that any were patented, or used, it indicates the nature of their relationship and the intense interest they had in pursuing ideas which were new to them.

When Sigurd graduated from high school he wanted the excitement of getting out in the world and earning money. A job as a lineman for the California Edison Company brought him near an airfield and he promptly lost his heart to aviation. He saved what money he could for lessons, and spent much of his spare time hanging around airfields, absorbing all he could about airplanes, flying, and mechanics. In 1923 Sig went to Los Angeles, learned to fly, and soon bought his first airplane—a World War I Jenny. He and his friends flew in air meets, stunted at county fairs, and had their share of the crashes, mishaps, and adventures that marked those wild early days of flying.

Eventually Sig decided to get a steadier job to support himself and his airplane. He became a flying serviceman for Southern Sierras Power Company and used his plane for making quick trips to locate line troubles and transporting materials needed for construction or repairs. In his spare time he gave flying lessons.
In 1928 Sig's flying career was interrupted by a prolonged bout with tuberculosis (his second) and he was confined in a sanitarium for a year. Fortunately, this restored his health and in 1929 he was able to go to work flying for Pan American on the company's new Mexican and Central American routes. He was one of the first pilots to learn blind flying and he helped make instrument-flying an accepted practice.

Sig's flying experience in Mexico and Central America was exciting but well-paid, but dangerous. There were few believable navigational maps, no weather forecasting, and initially no radio navigation aids; as a result much of the flying was done by dead-reckoning. Frequently, he would encounter electrical storms and be lost over uncharted countryside. On several occasions he came back alive by the skin of his teeth, and once his plane crashed in the jungle. As I will make clear shortly, these experiences played a significant part in stimulating the invention of the klystron.

While Sig was flying, Russell joined the fledgling Farnsworth Television Laboratory where the brilliant inventor Philo Farnsworth was pioneering the development of television and a variety of the necessary electron tubes and devices. Here Russell began his inventive career, producing at least eleven patentable devices or processes and many more which overlapped the ideas of others of which he was unaware.

In 1933 he returned to Stanford to continue his graduate work in physics. The following year he and Bill Hansen roomed together and began to exchange ideas on
the generation of high voltage x-rays, cavity resonators, and many other exciting projects. This association developed into a lifelong respect for one another's skills and ideas. It was the basis for their collaboration on the klystron and, later, on nuclear resonance and many other scientific ideas. Hansen was a brilliant student who graduated high school at the age of 14 and was sent to Stanford at the urging of his teachers. Immediately after receiving his A.B. in physics in 1929 he began his graduate work in the x-ray field. Quantum mechanics was still in its early stage of development, there were no reliable theories to explain x-ray phenomena, and unusually careful measurements were needed to compile adequate information. During this time, his childhood training with tools and mechanics blossomed and he became an excellent machinist. Later these mechanical skills became important in the klystron project as he was able to sort out Russ' ideas in regard to practicality and also to help Sig design his equipment.

In addition to his productive research, Bill developed a flair for teaching and was made an instructor in the Physics Department almost immediately upon beginning his graduate work. The combination of his teaching skills and an extraordinary understanding of basic laws of physics make it possible for him to pioneer teaching courses in microwaves, first at Stanford to his students and colleagues, and later, during the early days of the second World War, with a course at the M.I.T. Radiation Laboratory attended by some of
his own professors. His notes on microwaves, simply known as "Hansen Notes" were classified as "secret" and served as a foundation for radar research conducted at M.I.T.

In 1934, in addition to his teaching, Hansen became part of "The Supervoltage X-Ray Committee." Comprising the entire six-man physics faculty, the Committee was formed to do the preliminary engineering for a 3-million-volt X-ray tube with which to more adequately explore the mysteries of the atomic and nuclear worlds. Hansen's recollections of this period provide a fascinating insight into the exigencies of university research during those Depression years. In a 1949 memo he recalled that "any given faculty member might count himself very lucky to have $500 to spend in one year and this money included everything, for example the brass and small tools used in the shop. As to assistance, he might have about one-fifth of one machinist (they also had to work on apparatus for instruction) and perhaps one or two inexperienced students.

"Thus, to go into a year's research, a faculty member had his own mind and back—but spare time only—a few hundred dollars, a small fraction of a man-year of machinist's time and whatever apparatus might already be around.

"I remember spending a day or so with a sledge-hammer breaking up a concrete pier—plain ditch-digger's work. And many other days of manual labor. I remember, too, that the very first time I ever met the head of the department of physics he was painting the laboratory
In recalling a vacuum tank project, Hansen continued: "I had first to learn enough mechanical engineering to be able to produce an adequate design. An annoyance was the fact that the engineering faculty was acquainted only with the problems of pressure vessels, i.e. boilers. Next I had to do the drawings. Then I had to find someone to make it. This involved several thirty-mile trips to San Francisco, each taking a day. I then became purchasing agent pro-tem, next saw it through the shops of Western Pipe and Steel who made it, and finally took delivery outside the physics department. Next I had to devise ways of moving this ton or so of steel inside and downstairs— without using heavy equipment. Finally, we needed to weld various pipes into the tank. The way to get these joints vacuum tight is to use atomic-hydrogen welding, and suitable outfits for the purpose are sold by G.E. But we couldn't afford one, so I had to design and build such an outfit, and had to become a skilled operator with it.

"The above sounds like a lot of work. It was. But it was a big job for us. The price of the tank was $245.15."

... It was thus a notable achievement that a complete design for the tube was produced within a year. During that same year Hansen arrived at his famous rhumbatron cavity resonator design which would prove so important to the subsequent invention of the klystron. It had occurred to Hansen that high voltages developed in a
resonant circuit might be a good way to accelerate particles—perhaps in some similar way to the acceleration of protons by the cyclotron at Berkeley. Hansen and his former roommate, Russell, "speculated on various dodges that might be employed to get the high velocity electrons without using a lot of money." Gradually Hansen gravitated to the idea that a closed concentric resonant line might be the right solution. As surprising as it may seem now, the idea of a microwave cavity resonator was not yet known and Hansen was exploring the utility of the idea both intuitively and by complex mathematical analysis employing boundary value approaches. Soon it began to appear that the resonator needed for x-rays would be extremely low loss and that it could be made to have unusual properties, even at short wavelengths such as centimeter waves, by leaving out the inner conductor of the coaxial line!

It is important to recognize that Hansen's search for a low-loss resonator for x-ray applications actually set the stage for the microwave advances which were about to follow—both by inventing the cavity resonator and by developing methods of analysis for a large variety of microwave circuits and devices.

Recognizing a need for radar, while Hansen and Russ Varian were deep in their physics, Sigurd was finding his flying beginning to pall. Despite the adequate income and the pride he took in flying Pan American, Sig and his wife realized they wanted a change. After extensive correspondence with Russ about a variety of technical ideas, Sig decided to join Russ and Eric in starting a
laboratory at their boyhood home town of Halcyon, 220 miles south of Palo Alto. Initially the Halcyon laboratory was intended to get the brothers into the business of making optical diffraction gratings. They thought they could employ straightforward kinematic design to develop a new kind of ruling engine without being dependent upon extreme precision for the various parts. But the project soon became greater than they had anticipated and progress slowed. Meanwhile international events were causing Sig great concern. During 1936 cities were being bombed in Spain and China, and Sig felt there was no way to combat attacking airplanes that might approach a city in bad weather or at night. From his experience as a flyer, he was sure he could find a target in overcast weather and that no enemy ground defenses could ever hope to detect him. He was groping for an answer to this potential danger and frequently discussed it with Russ. As so often happened in their boyhood, Russ began to think about the problem. From his knowledge of basic physics, he recognized that the only way an attacking airplane could be detected through overcast was by means of radio waves and that accurate determination of direction with equipment of any reasonable size would require radio waves in the centimeter wavelength region.

Early in 1937, Russ and Sig began to consider developing radar—as yet unnamed—but there was no source of the necessary centimeter waves. Russ was well aware of the limitations placed upon generation of short radio
waves by conventional methods as a result of the difficulties created by building suitable resonant circuits attached to conventional tubes. He knew that at shorter wavelengths the efficiency (or the Q) of resonant circuits would be very low, but he did not realize initially that the flight time of electrons in a conventional tube would become even a greater fundamental limitation.

As a result of his collaboration at Stanford with Hansen, Russ was aware of Hansen's development of unusually effective resonant circuits in the form of cavity resonators. Russ thought that if radar requirements for microwave power were to be satisfied, some way of employing cavity resonators had to evolve. However, he was dubious about undertaking such a project because he felt it might be too big for the brothers' slender financial means. Nevertheless, by February 1937, Sig's feelings about the importance of doing something about combat airplanes under conditions of poor visibility became so intense that he finally persuaded Russ, in spite of his misgivings, to come to Stanford and try to work on the project intensively. They met with Hansen and D. L. Webster (head of the Physics Department) and found both men extremely receptive. (As an amateur pilot himself, Hansen was quick to appreciate the needs for aids to navigation and defense).

Russ wrote later that "The credit for starting this project mainly belonged to my brother, since he was the one who was most insistent that this development
should be attempted."

Stanford's President, Ray Lyman Wilbur, arranged
for the Varian brothers to become research associates in the Physics Department without salary to work on the project. The University would contribute laboratory space and $100 a year for materials and supplies! The department shops were also to be at the disposal of the project. If any financial returns were to be derived from this work, the proceeds were to be equally divided between the University and the two Varian brothers. Later, Hansen was given a small share of potential proceeds. This turned out to be a great bargain for all concerned. It means an honest-goodness project for Russ, and for Sig an opportunity to pursue an exciting objective with a potential for making some money in the end as well. I can't imagine that in today's world a university would grant the use of its facilities to two unknown young men who wanted to pursue a major objective with negligible resources.

As for the University, it eventually received over $2-1/2 million in royalties, as well as many fringe benefits from the momentum gained by pioneering microwave research. Much of Stanford's present fame in electronics and physics can be attributed to the collaborative efforts of these three men and the students who worked with them.

At this point it is important to recognize that the Varian brothers were not "randomly panning for gold," but knew almost exactly what they were looking for. For instance, they knew that in order to detect an
aircraft in darkness and overcast, the signals which would be sent out would have to be radio waves rather than light or something else. Moreover, they would have to be short-centimeter wavelengths so as to it possible to locate the object with some precision; physical optics in Russ' training told him exactly the relationship between the physical size of the equipment and the desired wavelength. Furthermore, Russ was able to calculate the power that would need to be available for such a system to detect an object at some reasonable distance. This pretty much spelled out the characteristics of the new type of radio transmitter that had to be developed; now all they had to do was to figure out how to do it. And this was not at all obvious; other people have tried to invent microwave devices without much success.

In March 1937 they came to Stanford and began their research for a practical way to generate radio waves. In his letter to his wife, still at Halcyon, on March 8th, Sig wrote:

"If everything works out right we are on the task of something awfully big. It could be used for almost unlimited purposes, mainly of interest to me in seeing airplanes, etc. in bad weather."

Some inkling of the proper direction was provided by Russ's realization that one ingredient appeared to be already at hand: Hansen's newly invented cavity resonator. Very soon, however, he realized that there was a major difficulty: the transit time effect. As Russ mentions in his notes, in order to achieve the
frequency they were interested in they would have to 
eliminate "the trouble caused by the flight time of 
electrons in the ordinary grid control vacuum tube."
Some 20 years later, he recalled:

"We spent many hours thinking of all the types of 
controls that could be used. We very early arrived 
at one definite conclusion—that a cavity resonator 
must itself provide the electron control and that the 
electron control must be of a new type."

While Sig chafed anxiously, impatient to get started, 
Russell began to record ideas in his notebook. I have 
this notebook before me as I write, and I am struck 
by the fact that it was kept in ink, with a carbon 
copy, indicating that Russell had a clear realization 
that the ideas they were exploring might lead to 
patentable inventions and useful results.

Russ' notebook on March 11, 1937, describes a system 
for locating airplanes with a radio beam and examines 
the resolving power of the antenna and the wavelength 
that would be useful. The note indicates they were 
searching for a wavelength of about 1 centimeter.

Next, a scheme employing Hansen's resonator in the 
Farnsworth electron multipactor, a secondary-emission 
uhf oscillator, is examined. Other ideas follow in 
rapid succession, interspersed with other inventions 
unrelated to the task at hand, such as a method of 
electrostatic separation of sand particles!

A[redacted] idea emerged in April when Hansen conceived 
the idea of a circular swinging beam tube which would 
energize the rhumbatron through a series of holes in
the rhumbatron along a circular path. This idea looked
enough good to start experimenting with and, in
May, Sig started the actual work on such a rotary beam
frequency multiplier. This introduced Sig to vacuum
techniques and the mysteries and frustrations of the
vacuum; I remember him saying later that he wondered
at the time that anyone could ever get a vacuum at all.

Many other ideas relating to generation of microwaves
were examined conceptually and, in some cases, mathematically. Bill Hansen was not actually involved in
the process of invention but was always available to
examine Russ' ideas and was quick to point out their
limitations. As weeks went by, so did the ideas until
June 5, 1937 when the thirteenth idea was conceived.
Remarkably, this was not an extension of previously
known concepts, but was a radical departure in the
control of an electron stream. This was the bunching
principle which, combined with the rhumbatron, became
immediately the klystron tube.

The steps that led to this discovery were recalled
by Russell in a 1957 paper on the history of the
klystron:

"One day, after we had thought of a number of schemes,
I was occupied in developing a classification for all
the schemes we had thought of so that we could system-
atically investigate them all and not discover later
that we had overlooked some of the most promising ones.
In the process of developing this classification I
suddenly thought of the velocity-grouping principle.

From a psychological viewpoint it is rather interesting
that this attempt at classification actually produced
the invention of the klystron. The velocity-grouping
principle did not fit any of the schemes of classifica-
tion that I had contrived and I rather think that the
idea occurred to me because I was unconsciously at-
tempting to test the validity of my classifications.
Hence I thought up an exception to the classification
which actually turned out to be the basic concept of
the klystron."

Russ explained velocity grouping in his notebook:

"The new method is a sort of grid control, but
of the electrons are prevented from passing the grid.
They are merely slowed down or accelerated... Under
these conditions, the electrons after passing the con-
trol grids will have variable velocities depending
upon the phase of the oscillating circuit when the el-
ectrons went through.

"If the electrons continue in a straight line the
accelerated ones will tend to catch up on the retarded
ones, and the stream of electrons will transform from
a uniform beam to one consisting of a series of con-
centrations or waves of electrons having the same fre-
cquency as the exciting frequency.

"One of the beauties of the system is that a mutual
conductance is independent of frequency."

Immediately following, Russ' notebook shows a mathe-
matical analysis of this principle and explains how
the mutual conductance and the amplification factor
would depend upon the various operating parameters.
concept of a device into a design which might have a good chance of working the first time it was tried?

It all depends, of course, upon how novel the concept is, but I am still amazed that only a few days of spirited debate followed Russ' conception until he, Bill, and Sig concluded that the invention could easily be transformed into an important device. The design work was started immediately, with Hansen's knowledge of the characteristics of the cavity resonators being an essential ingredient in the design. In a little over a week, there appeared in Russ' notebook the design of a klystron that is reproduced in Fig. X. It is remarkable that even to this day klystrons do not differ materially from this design.

What came next has been described by Sig, who wrote (in 1944) that he remember June 5 well because having replaced some fuses with brass pipe he burned up 15 feet of cord and blew most of the breakers in the University power house. His recollection continues:

"The invention of the klystron caused considerable interest about the department. Hansen calculated the 5 dimension for a 10-centimeter cavity and then we blew the dimensions up to the dimensions that John Woodyard could test with his wooden rhumbatron setup, (which consisted of a large wooden box lined on the inside with copper foil).

"Hansen and Russell Varian calculated that the cavity would have to be of the reentrant type and when they furnished me with some dimensions I folded up one out of copper-lined cardboard. Woodyard's test on a model
proved Hansen's calculations to be correct. The cavity of the klystron looked so much like a spittoon that it took several months to erase the name from that local nomenclature."

Now that the basic idea jelled, Sig was able to go to work and construct the actual experimental tube. Here his ingenuity and manual dexterity matched Russ' intellectual concepts in furthering the invention. Tiny hexagonal "honey-comb" copper grids were hand-filed by Sig ("with enormous labor," wrote Hansen in later when he could "still recall the hamburger celebration that the completion of the first grid called for"); a cathode was fabricated, machine parts were made of unfamiliar materials, and microscopic adjustments were provided outside of the vacuum to tune the cavities. Vacuum problems were solved—which was not easy.

Hansen's 1949 report explains: "Perhaps the biggest troubles came from lack of a glassblower. Because of this lack it had become customary around the physics department to make practically all vacuum systems out of sections of flanged pyrex pipe, bolted together along with various fabricated metal sections. This was fine for x-ray tubes, but not very flexible for anything else, especially since the maximum clear inside diameter was only about 2-3/4 inches. But S. Varian finally devised an assembly with a cathode, two resonators (one adjustable) an adjustable coupling loop and various other gadgetry all of which would go inside a flanged pyrex pipe and with all leads and adjustments coming out of the metal and plate."
S. Varian made most of this device himself, except for the resonators which were spun in S.F."

Sig recalled, "I had to water cool about everything in the tube because the diffusion pump I was using was of the original homemade pump type which the Physics Department had built. The pumping speed was nearly nil, any slight source of gas would stall the pump.

"To add a little zest to our experiments, there was a 200 000 volt generator for x-ray use, over my head. Every few minutes, nearly always during our tests, or tense moments, there would be a crack like a rifle and a 200 000 volt arc and myself would jump about the same distance."

Despite such distractions, in an amazingly short time—about a month—a klystron evolved as a complex mechanical device encased in a vacuum bell jar. The total cost was just half of their appropriation—$50, mostly for the power supply condensers! As Russell later pointed out, this made it probably the cheapest project every completed in microwaves. Interestingly, it wasn't until their paper, "A High Frequency Oscillator and Amplifier" had been submitted to the Journal of Applied Physics in January 1939 that they learned that velocity modulation had been discovered in 1933 by Arsenjewa-Heil and Oscar Heil in Europe.

There was one very important problem besides deciding on the type of oscillator to build. That was to find a way of knowing that it oscillated. As Russell described the situation in his 1957 paper: "None of the measuring instruments now available in the microwave
region had been developed, and the only detector we had that could be considered for the purpose was the old galena crystal detectors of early radio. We did not even know that these would function at all at microwave frequencies, and if they did function any meter that we could attach to them would be slow acting and the probability extremely high that we would never detect oscillations. I finally decided that we could allow a small part of the electron beam used to drive the klystron oscillator to pass through a hole in the last resonator and be deflected into a space beyond by a magnetic field so that it would land in a moderately small area of fluorescent screen. This would provide a quick and sensitive detection system for any oscillations which occurred. As it turned out this invention was probably about as important as the klystron invention itself, because without it we probably would never have discovered the oscillations although they would have been occasionally present. The first model we built produced some oscillations which my brother saw on the fluorescent screen, but the tuning mechanism was not capable of going smoothly through resonance and so we were never able to repeat the result. It was about the third model we built which gave reproducible evidence of oscillations."

The Model A klystron oscillated the first time the evening of August 19, 1937. Sig wrote a few years later, that:

"We observed repeatable flashes on our detector screen, but everything was very unstable and rather
disappointing. Cathode emission died and came back with tuning. About August 21, I took the tube off the pump and replaced the tungsten grid wires with copper head grids and installed a micrometer adjustment to the tuning. This was a major operation. On the morning of August 30, 1937, I was ready to try again. I threw the switch, tuned the tube a little, and there were the oscillations spread all over the fluorescent screen. We dug up an old dime store cat's whisker crystal detector and a galvonometer and picked up r-f energy all over the room. We made a quick check on the frequency by moving the crystal detector through the standing waves in the room. In our excitement we figured the wavelength to be 6.5 centimeters and were very embarrassed later to have to admit we measured half wavelengths or 13 centimeters being the correct wavelength."

In September the name klystron was formally adopted. It came from the Greek verb klyzo, expressing the breaking of waves on a beach, and had been one of several words proposed by Prof. Frankel of the Stanford Classics Department. September 1937 was also the time when I first heard of the klystron. I had just come to Stanford as a graduate student with Professor F. E. Terman in the Electrical Engineering Department. The first course I took in "Modern Physics" was a new course being developed by Bill Hansen who believed that one useful way of introducing physics to graduate students was by exposing them to the ongoing research in this field. Thus, one
of the early experiments we were asked to understand and to repeat was the behavior of the rhumbatron which Hansen had so recently invented. Whereas I had been accustomed to high Q circuits—perhaps in the vicinity of several hundred—we discovered that a simple metallic container could resonate at the unheard of frequencies with Q's in the order of 30,000!

I remember Terman telling us about the invention of the klystron tube and of the remarkable properties it was supposed to have. But neither I nor other students heard much more about this, for the idea was not discussed openly since the impending war in Europe already had cast a blanket of semisecrecy in the U.S. What I did hear caused me to be interested but not terribly excited. Perhaps this was because I did not know enough about it, or perhaps because in the mid-1930s there were a great many other advances in electronics, and important discoveries were being made frequently. For example, just then, a group of us in the E.E. Department were exploring the various aspects of negative feedback; and, Bill Hewlett had just demonstrated the utility of the resistance-tuned oscillator which was soon to be the foundation of the Hewlett-Packard Company.

The fall of 1937 was spent by the Varian brothers on the development of additional models to test the operation of the klystron. By November, the Model B tube (see photo) was completed under a bell jar. Now displayed at the Smithsonian Institution, this tube was regarded as a major accomplishment. Since it was easily demountable and could be changed to permit testing a
variety of improvements, it gave Russ, Sig, and Bill Hansen the means with which to do many of the things they had long wished to do with microwaves. As a result, what had been merely ideas and aspirations quickly became programs with well-defined objectives.

During the next two years, two programs became prominent, with lots of subsidiary projects related to methods of amplification, detection, and of measurement, as well as converting the rudimentary tube structures into devices that could be used in the field. The first program of major importance was the development of a practical radar system—of the Doppler variety. Russ and Bill Hansen developed some ideas about using an oscillating klystron as a detector in such a way that the velocity of a moving object could be detected. Refinements of this idea, using more than one transmitted frequency, were developed so that the distance of the object could also be determined. The second program involved development of klystrons suitable for instrument landing of aircraft, which quickly aroused the interest of the CAA and resulted in collaborative work in this field.

Once it could be demonstrated that the klystron would oscillate, it became imperative to understand its general behavior, establish its electrical characteristics, and explore its utility for useful applications. Only a few people were involved up to this time, with almost all of the model construction being done by Sig. Some additional financial support was obtained and it was now possible, as well as necessary, to employ others—
some trained in electronics and others in the art of tube making.

I was one of only about 16 graduate students in the E.E. Department, where I was working on a thesis on negative impedance. Fred Terman must have recommended me to the Physics Department early in 1939 Professor Webster asked to see me. He told me about the project in some detail and I became so fascinated by the ideas that when he offered me the job as a half-time research associate I promptly accepted. Although the $90-a-month salary was great, actually the job itself—my first—was the main attraction; the opportunity to work on something new and exciting, and to be paid for it, was hardly believable.

John Woodyard, also a student from the E.E. Department, was already working with Hansen, and I became the second professional employee on the project. The duties became considerably more extensive because Sig Varian was ill with tuberculosis and remained either bedridden or inactive for almost two years. Aside from two machinists, Woodyard and I were the only active employees of the project during this formative period.

By 1939, the general characteristics of the klystrons were fairly well understood and their wavelength and power were measured by rudimentary means. Now we wanted to test the utility of klystrons in the usual radio circuits, such as master oscillator/power amplifier combinations and superheterodyne receivers, and in Doppler radar and other experimental systems. In addition, ideas for a variety of other klystrons abounded,
and much work was done on such things as single-cavity klystrons, and klystrons with multiple cavities for use as high gain devices and/or receivers. We built the klystrons in small batches—two or three at a time—in the basement of the Physics Building, where the klystron project maintained a small shop. Most of its equipment was very old, as but negligible funds had been spent on Physics Department facilities over the years. The klystron project did buy one new Monarch lathe and it did have a Litton spotholder, a Litton glass lathe, several hydrogen bell jars, and a few vacuum systems with hand-made pumps. Despite the rudimentary shop facilities, in the hands of skilled machinists—John Schultz and one or two assistants—superb vacuum tubes were made even though the act of making them was strictly empirical.

Soon the klystron became not just an individual tube, but a circuit component, and we could explore its utility in a number of conventional combinations. Almost everything we tried—amplifiers, receivers, superheterodyne detectors, etc., worked immediately and quite well. We were able to demonstrate that almost anything one could do with conventional radio tubes could be done by the klystron at microwave frequencies. In addition to the invention of the klystron, which took about two years to complete, our group was probably the first to demonstrate that it was possible to generate microwave signals, amplify them, detect them, and configure microwave circuits in a way which would correspond to the conventional requirements of normal radio systems.
There was one other major achievement during this period: the location of significant financial support! By the time the klystron invention was reduced to practice, the Varians had exhausted their own limited resources and the situation had become critical. In describing this period in 1944, Sig wrote:

"At this time we became more and more worried about the international situation and the danger and tragedy of aggressive air power. We did our best to interest the parties who should have been vitally interested.

"The first serious interest in our tube was furnished by Messrs. I. R. Metcalf and John Easten of the Bureau of Air Commerce and Mr. H. H. Willis of Sperry Gyroscope Company. The Sperry Gyroscope Company was immediately interested [they saw it as a replacement for their search lights] and it was not long until a contract was signed between the Varian brothers, Stanford University, and Sperry Gyroscope Company. Russell and I were happy to be put on a salary as we had been supporting ourselves for over three years on savings we had accumulated."

Sperry provided about $20,000 per year for further research on the klystron project and this was sufficient to fund about a dozen of us in a style to which we had not been accustomed. "With this change," wrote Hansen, "we had innumerable conferences on the business end, all of great future importance. During this period we were all business men, amateur lawyers and patent attorneys in addition to doing our research."

In the Fall of 1940, Sperry asked the entire Stanford
klystron group to move its newly-founded Garden City Laboratories. The Varians, Hansen, Woodyard, I, and several others moved to New York to spend the war years helping develop the klystron and related equipment for obvious war-related applications. This period is a whole other story and I will not attempt to cover it here. Suffice it to say that the klystron proved to be an important ingredient in the war effort, teaming up with the magnetron to make microwave radar practical. The Varian-Hansen group also continued its efforts in Dopplar radar, and demonstrated the usefulness of this principle, especially in the form of coherent pulsed systems.

Russ had told his friends in 1940 that the move to the East was not to be the end of their ambitions to work in their own laboratory. He said that he and Sig intended to come back and build a research laboratory which would explore new ideas—their own and others developed at universities—and to pioneer practical applications for advances in science. After the war, the Varians, Hansen, and almost the entire Stanford group did come back to California, attracting many other capable people with whom they had worked in Garden City. Some of this group came to Stanford either as faculty or as graduate students. Others came to found Varian Associates, in 1948, with six full-time employees and $22,000 in capital. In this way, Russ and Sig finally fulfilled their ambition to have a laboratory for their own collaboration which could provide a research and development atmosphere for
exploring new ideas of scientific and practical importance. By now, the group had enough confidence in their own ingenuity and ability to know that they were certain to continue the process of invention, innovation, and reduction to practice.
The roots of many of the strengths and talents possessed by Russell, Sigurd and Bill Hansen were evident in their early childhood. The senior Varians were Irish emigrants who settled in Palo Alto in 1903 when Sigurd was 3 years old and Russell 5. Their father, John, earned all the money in the family as a professional masseur but he was not licensed and did not have much education. Their economic status was reflective of a gentile poverty that was common in the United States in those years and the family was able to provide a warm and supportive environment for the children. The lack of money was not much of a hardship for the growing boys, but it did require them to build their own toys and to invent. Russ designed many things, such as cigar box planes, which Sig built.

As a small child Russ and his father were particularly close. His slowness in learning to read and to do simple arithmetic led to difficulties at school, and perhaps because of this he tended to listen intently to whatever was being said around him. His mother, Agnes, would read to him, and his father liked to expound his political and social theories to him. While Russ made friends and participated in the usual activities of small boys—making models and brewing chemical mixtures—he had the habit of asking lots of questions and he spent much of his time discussing his ideas with his father.

Russell compensated for his slowness in reading
by developing a remarkable ability to comprehend what he learned. He often took a long time to read a particular assignment but what he read he virtually committed to memory. By the time he reached high school, he was already a real scientist in the sense that he was always asking, "Why?" He would never take a statement in a textbook for granted unless he knew how the author reached a particular conclusion; this was partly because of his skepticism about the depth of research done by the author, but sometimes because he knew that knowledge in a given field is never complete and that there are always loose ends to be thought about and explored.

Shy and awkward as a boy, with somewhat of an inferiority complex, I am not sure his peers or his teachers appreciated his unusual points of view until much later in life. He was still ridiculed and laughed at at Stanford, where only a few people—such as Bill Hansen—really appreciated his uniqueness. However, Russ attained late in life, particularly when I knew him between 1939 and 1960, a quality of respect and elder statesmanship because of his great breadth of knowledge and his power of synthesis.

When the boys were teenagers, the family moved to the small theosophist community of Halcyon, some 220 miles south of Palo Alto. There they ran a general store and the post office, and their home became well-known as a stopping place for a circle of friends which consisted of liberal and professional people, including Fremont Older and Lincoln Steffens.
Fortunately for the boys' education these visitors met frequently in the Varians' living room and talked animatedly on just about every subject of concern at that time—politics, economics, sociology, religion, labor problems, women's rights, women's suffrage, socialism, and education. Russell's widow, Dorothy, relates that Russell often sat in on these discussions, eating up all the ideas proposed, until the inevitable cloud of smoke would get too much for him and he would run around the block a couple of times before returning to listen some more. These discussions, and his parents' own liberal—almost socialistic—ideas influenced Russell very much and contributed to his own liberalism in adult life.

As Russell and Sigurd grew up they developed remarkably distinct characteristics. Sigurd, as described in the article, was the extrovert and activist as well as the gifted mechanic who would build what Russell designed. Russell took life the way it came, moving from one immediate opportunity to another; on his own he might not have started the laboratory at Halycon, might not have moved their joint efforts to Stanford, and perhaps would not have embarked upon the major enterprise of getting Varian Associates under way. But, with prodding from his enterprising brother, Sig, he was willing to come along and try projects which he was not all that certain of on his own.

Bill Hansen was born in Fresno, California in 1909. Although his father had a limited education and had
to go to work at the age of 12, he augmented his
education at night school and developed a good know-
ledge in mathematics and mechanics. Perhaps because
of his native bent, he was able to encourage his son
in several technical directions. Bill was given
mechanical toys to play with but soon began to prefer
electrical toys, many of which he made
himself. Encouraged by both parents, he
practically filled their house with a variety of
electrical equipment which he reworked and experi-
mented with. His interaction with his father was
particularly satisfying in the area of mathematical
problems and games, and this childhood practice de-
veloped into an absorbing passion which lasted his
lifetime.

Bill Hanson entered high school early and graduated
at 14 years of age. His teachers considered him so
outstanding that they urged his parents to send him
to Stanford. He went with the intention of becoming
an electrical engineer, but soon became attracted
to more basic sciences than were then being offered
by the School of Engineering. In his junior year,
he was offered a position in the Physics Department
as a laboratory assistant and this opened for him
the field of experimentation on a scale he had never
seen before. His personal strengths of inventive-
ness, intuition, and ability to analyze mathematically,
were later to prove indispensable to the klystron
project. — E. L. G.