

with an appointment at LBL as Faculty Senior Scientist and Leader of the Heavy Element Nuclear and Radiochemistry Group. It was in some ways a difficult decision for me, as I was very devoted to my Division at Los Alamos, but I felt they were in good shape and that it was time for me to help educate the next generation of students in nuclear and radiochemistry. And I also wanted to pursue my interest in the chemical and nuclear properties of the heaviest elements and maybe even help search for new elements. Berkeley was the ideal place for that. So I accepted and then Marvin and I began the difficult process of moving away from Los Alamos after more than 31 years there. So I came to Berkeley in August 1984 and I started my next career as Professor of Nuclear Chemistry. I continued my close association with those giants and pioneers of nuclear science, Glenn T. Seaborg and Albert Ghiorso, with whom I am now privileged to coauthor this book.

## **P.2. Albert Ghiorso**

I was born on July 15, 1915, in Vallejo, California, as the fifth of the seven children that my mother would have. Two died in infancy, leaving our family with three girls and two boys to grow up together in Alameda just across the bay from San Francisco. My father, John, had emigrated from Genoa, Italy, with his family when he was two years old. The family, my grandmother and grandfather with four sons and two daughters, settled on a very small farm in the hills above St. Helena and there they eked out a modest living. Thirty years later I would spend most of my summer vacations at this ranch. My father left the ranch when he grew up and became a jack-of-all-trades, making his living at various times as a taxi-driver, riveter, welder, cook, handyman, etc. Although he never attended school beyond the fourth grade, he knew a lot of lore and respected education. Like most working men of that time, he was a strong union supporter and a political radical and wanted his children to amount to something in their lives. In particular, he wanted me to become a lawyer — an *honest* lawyer, he emphasized!

But that was not in my plans. Although I was good at such things as history and the other subjects that a good student can excel at, I also had a mechanical aptitude that showed up very early. I remember at the age of about five playing with shingles discarded by carpenters who were building houses nearby. I became adept at structures and learned how to make things. One incident that stands out in my mind occurred on my grandfather's ranch. An old automobile had developed a flat tire which had to be pumped up manually. The pumping hose would not stay in place and I was told to hold it there with my hand. I noticed that when the pumping commenced, the hose became very warm, and this aroused my curiosity. I asked my uncles why this happened and they did not know. I soon figured out that it was because the act of compressing the air had heated it. When I explained it to the adults they marveled that a mere child could know these things.

I never became interested in the technical side of radio as a child, although my father tinkered with the new-fangled invention and gradually acquired a lot of equipment as a hobby. Some years later this gear was to have a powerful influence on my career. Instead of radio, I loved making gliders and rubber-powered model airplanes that flew and I gradually learned the rudiments of design by trial and error. For a long time I had set my sights on becoming an aeronautical engineer, undoubtedly influenced by the close proximity of the Oakland Airport. I became a member of the Aviation Club at Alameda High School and was vice-president of the eight-member club in 1932 (Fig. 1). Our house in Alameda was at the east end of that island, directly across San Leandro Bay from the airport. I used to bicycle the two miles to the airport regularly to examine the planes at close range. I remember quite vividly being present with hundreds of thousands of others when Charles A. Lindberg landed there on his barnstorming tour of the country after his historic flight across the Atlantic in May 1927.

My mother was born in Watsonville, California, of pioneering stock that stemmed from the Spanish Land Grant times. She was a tender, pious woman, and made sure that I was baptized in the



**Fig. 1.** Photo of Ghiorso cropped from a picture of the Aviation Club included in the 1932 Alameda High School yearbook, *Acorn*.

Catholic Church despite the fact that my father was not only openly antagonistic to the Church but was also an avowed atheist. I did what my mother wanted me to do and attended Sunday school dutifully and absorbed the dogmas as a willing believer. The only books as a child at home that I had around me were religious tracts and I read all of them voraciously. By the time that I started to attend school and had access to libraries I was quite happily indoctrinated. That was not to be challenged until I was 13 years old when, in a high school history class, I realized that most of the world was not Christian and had beliefs and superstitions that were quite different than those that I had been ingrained with.

I did well in school and always assumed that I would get into some kind of academic profession. However, when I graduated from Alameda High School in 1932 the Great Depression had gripped everything very tightly and our family did not have even the modest sum (\$26 incidental fee!) that it would take for me to enter the

University of California. My father was out of work most of the time and had to resort to bootlegging liquor to keep going, a stratagem used by many people during Prohibition. Meanwhile, my “Big” sister, Genevieve, had applied for a small scholarship for me to go to Cal and this made the difference. Now I could commute to the University by virtue of the excellent public transit system (light rail!) and live at home. But which one of its colleges should I enroll in?

I had a whole summer to decide. Since there was not even a summer job available for me, I had nothing to do except read and think. Among the books that I got from the local library was one called *Letters from a Radio Engineer to His Son*, by John Mills, published in 1922. Mills, a Western Electric Company engineer, wrote this fascinating book of 24 letters to explain to his son in a very straightforward, detailed way how radio worked, and he expanded his tutorial by suggesting simple experiments that could be undertaken to demonstrate the new science. I decided to try my hand at them, since I had the legacy of the gear that my father had played with in the 20s.

The book had a profound effect on me by uncovering a whole new world that I would marvel at and I proceeded to construct the experiments one after the other. Thus encouraged, I enlarged my sphere of knowledge and skills even further by poring over the hobby magazines to make small pieces of simple equipment. I remember making a supersensitive carbon microphone, a thin pencil carbon resting vertically and loosely in indentations in two blocks of carbon that could detect a fly walking on the sounding board (cigar box) that supported it. The experiments were simple but elegant, in that they instructed me in some of the basic principles of radio. I concentrated all of my efforts day after day on learning all about this new science from magazines as well as by doing the experiments; by the end of the summer I knew that I wanted to be a radio engineer. However, there was no such category in the university’s curriculum at that time and I had to settle for electrical engineering. Sixty years later I told this story of how my career was started to an ORNL audience and it struck a responsive chord with my good friend Dave

O'Kelley, who knew that book very well, also as a youth. He went to some trouble to find a copy of the book which had been out of print for decades and sent it to me. I treasure it as the beginning of a new life for me.

My career at UC was not spectacular; it seemed that all of the engineering students were excellent and this raised the class averages such that a high school student who was top dog in some small school did not necessarily rise to the top in a class of a thousand others. Still the courses were usually interesting, because I encountered so much new information. I managed to perform as an above average student for the four years and learned how to think scientifically. I particularly enjoyed freshman chemistry, especially the lectures by the inimitable Joel Hildebrand. My senior year was really exciting, though, because now I could specialize in courses that would affect me more directly. I remember auditing a course given by Prof. Lester Reukema in which he devoted the better part of a semester to the subject of negative feedback. This brand-new invention by Black at the Bell Telephone Laboratories in 1936 was one of the great discoveries in electronics; it was to have a profound influence on the field of amplification and spread to all other fields as well. I attended the Physics Department Journal Club occasionally and heard about some of the new discoveries that were being made by Ernest O. Lawrence's invention of the cyclotron.

I graduated in May 1937 ready to go out into the world and become a great radio engineer, but there were no jobs available as far as I could tell. This was about the low point of the Great Depression. I survived because I still lived at home in Alameda. I got small jobs making amateur radio equipment to order for "hams" who wanted special receivers or transmitters. I had developed a modest reputation in this regard because of my illegal operation of a "bootleg" radio transmitter on the five-meter band. Most unusual for the time, I had built a crystal-controlled transmitter and a superheterodyne receiver for this band to work with a rotating beam antenna and these had proved their superiority over the conventional equipment employed by others.

In particular, this became clear when my brother, Gilbert, and I contacted an amateur station in Ohio for a few minutes one day when the skip distance happened to be just right to bridge the signals across the 25000 miles that separated Ohio from California by bouncing them off the Kennelly–Heaviside layer. It was the first time this had ever been done on the five-meter band and was mind-boggling to everyone since it was well known that the radiations in this band only traveled in straight lines. This was my first venture into the field of discovery, but I never got credit for this exploit since the station was operating without a license. We had borrowed the call letters of a friend.

However, my reputation was now made. Since none of the equipment that I used was available commercially at that time I was asked by a wealthy executive, Larry Barton, of the Clorox Chemical Company, who was a prominent radio amateur, W6OCH, to build him a fancy receiver. He was willing to pay whatever it took and I immediately set about designing and building his receiver.

With this entrée into the world of high class radio equipment I came to the attention of D. Reginald Tibbetts, W6ITH, who was famous as the biggest powerhouse in the world among all radio hams. Having nothing better to do, I went to work for Tibbetts to gain experience. In Berkeley he ran a small business, Communications Supply Co., that catered to special needs. He had pioneered two-way communication setups for the building of the SF-Oakland Bay Bridge, and with his ability now well established he had begun to supply portable radio equipment for emergency services. All of this gear was highly specialized, so it had to be built to order. My job was not only to build but also to design and I had lots of opportunities to create new things to fit the needs that arose. One interesting project was to design and build transceivers for use in the construction of the Shasta Dam in California. The objective was to communicate between the Head Tower and the enormous “buckets” that carried the concrete that was poured into the giant forms. The work was done around the clock and reliability was the keynote. The equipment worked fine but developed problems in the field because the units were housed in

small black metal cabinets. In the hot sun that beat down into the canyon the temperature rose high enough to melt the capacitors. I spent several weeks in the area one summer until I had brought everything under control.

When there was no outside job that needed my talents, I worked on Reg's "ham" transmitter and receiver facilities. These were very elaborate, of course, so it was fun for me to work on them. By 1940 he had moved out to Moraga into a small valley which was ideal for setting up giant rhombic antennae aimed to all points of the compass. This enabled him to transmit to and receive from stations around the world. One of the problems that I was confronted with was how to measure accurately the frequencies of the stations that were received. I conceived of a simple way to accomplish this by measuring the beat frequencies between the received signal and locally generated crystal-controlled signals (which came in exact 10 kHz intervals). I measured the beat frequency with a local precision audio oscillator. This device was very successful and in due time we were asked to submit an article for *Electronics Magazine*. Tibbetts and I were supposed to be coauthors even though the idea and its reduction to practice were mine, but when the article appeared my name was missing. Reg blamed the magazine for the oversight but I doubt that the fault was theirs. He gave me the \$30 that was the going rate for articles at that time, probably out of guilt that he had left my name off the article. That was my very first publication and a good one at that.

Although I was not paid very well, I enjoyed the job and gained invaluable experience from working on so many different projects. One of the most valuable to me turned out to be the engineering and installation of an intercommunication system at the Radiation Laboratory on the Berkeley campus of the University of California. This was set up in 1940–1941 to connect the secretarial desks and I soon made the acquaintance of the two most important people in the Lab as far as I was concerned. One was Helen Griggs, who was Lawrence's secretary, and the other was Wilma Belt, who was Donald Cooksey's secretary. Cooksey was Lawrence's deputy and ran the

logistics side of the Lab in terms of personnel and special materials that had to be ordered or made in the Rad Lab shops. I got to know both of these women fairly well and before long discovered that Wilma and I had many common interests.

In 1941 Tibbetts was asked by the Rad Lab to produce what turned out to be the world's first commercial Geiger–Mueller counter circuit. It was to count particles with a scale of eight and a mechanical register and to have a regulated high voltage power supply for the G–M counter. It seemed that for some reason I was not told that it would be necessary to build hundreds of these devices for Prof. Glenn T. Seaborg's group. It was clear to me that I would have to set up some sort of assembly line to produce the required number in the short time allowed for delivery. The circuit diagram had been published in the *Review of Scientific Instruments* and a working copy of it had been made by one of Seaborg's chemists, Dr. Joseph Kennedy, as a model for us to produce on a mass scale. I remember with horror seeing the model chassis. The circuit was all crammed together in a space that was quite limited, about 12" × 17" × 8". None of the resistor or capacitor leads had been trimmed and everything was jammed together with no room for anything else. However, it did work and I soon decided to re-engineer it so that I could build the units with some assurance that they would all work. This turned out to be quite a job, but a very interesting one since I had never done anything like this before. I was to leave Cyclotron Specialties Co. in about a year and in that time I built some 300 of these units for the Manhattan District Atomic Energy Project. I often visited the Rad Lab for various reasons and I became fairly well acquainted with Seaborg's scientists, in particular Spofford English and Gerhart Friedlander, who became my mentors. Seaborg, himself, I knew only casually.

Wilma was another matter. I came to know her very well and soon we began dating. She was particularly interested in "hot jazz," something that I knew nothing about. We listened to records and went to the Dawn Club in San Francisco to hear Lou Waters and His Band. I, too, soon became an aficionado of this old/new music. We

also were both interested in the outdoors and we had planned a Sunday (December 7, 1941) for a trip to Yosemite Valley to see what it was like in the winter. We left early in the morning to make the 200-mile trip in my old Chevrolet coupe. In four or five hours we were in the Valley and what a sight that was. There was snow everywhere, no traffic, and no people. It was truly a winter wonderland. Wilma had prepared food and we spent the day exploring the park, never once turning on the radio. We were not the least interested in what the outside world was doing anyway! We were deeply in love. We already knew that WW2 was going badly in Europe and it seemed far, far away.

The end of the day all too soon made it necessary for us to wander back to civilization. An hour's drive took us down into the Central Valley, where finally we turned on the car radio for the first time. What a cacophony there was! For fully two hours all we heard was the fact that all sorts of troops were being called up. Why this was being done was not being discussed at this time. Something fearful must have happened early that morning. Finally, we heard the news. A surprise attack had been made by many planes from Japanese aircraft carriers on a good part of the Pacific Fleet quietly at anchor in Pearl Harbor. They had succeeded in essentially knocking out a large part of the big guns of the American Fleet in the Pacific! Panic was striking the West Coast! Was invasion near? The news was terrifying to us. Obviously, our country was now completely engulfed by WW2.

By the time that we reached Berkeley we had discussed over and over what might happen to us. To forestall the worst scenario we decided to get married as soon as possible. We would worry later about the inevitable draft that was bound to sweep me into the armed forces. For the time being we expected that I would probably be deferred at least temporarily by the work that I was doing for the Radiation Laboratory.

We were married the following month and enjoyed several months of bliss. But the war became more and more grim as the Japanese continued to win everything in sight in the Pacific. Everyone could

see that it was going to be a long hard battle for the U.S. to regain the lost territory. Soon I felt that my chances of avoiding the draft were becoming vanishingly small and I decided that rather than be drafted into the regular army, where my skills would be completely lost, I would be better off trying to obtain an officer's commission in the Navy. There I had a chance to be more useful, I assumed. Accordingly I applied for a Lieutenant, JG, commission, since I had heard that all it took was a college degree and recommendations from a couple of prominent people. Wilma suggested that I use Seaborg as a reference as he was a university professor.

I agreed that this was a good idea even though I did not know Seaborg very well, and I wrote a letter to him, now in Chicago, where he was setting up his group. He sent me the recommendation letter that I needed but he also made me an offer to join his group. He said that he could not tell me anything about the project that I would be working on but that it was important to the war effort and he was confident that I would find it interesting. I quickly accepted, with the request that I not be asked to build any more G-M circuits! I found out later that Wilma and Helen, who had married Glenn after my marriage to Wilma, had decided that I should join the Chicago group. Helen told her husband to hire Ghiorso and he took a chance and did so.

I was the twelfth member of the group and Glenn gave me a personal briefing when I arrived in Chicago a few months later in 1942. He told me in very broad terms about the atomic energy project, that their job was to determine the complete chemistry of an element that no one had yet seen. My job would be to take care of the instrumentation needs that his large group of chemists would use to do their job. He told me about the huge chain-reacting nuclear reactors that were contemplated to produce plutonium, the new element that he had discovered in Berkeley just two years before. And right next door to our laboratory near the University of Chicago, physicists were assembling the first man-made reactor in the world.

All of this information was overwhelming and it took a long time for me to grasp the enormity of the undertaking. It was very exciting to me and I, like everyone else, worked very hard. Six-day weeks were the norm and the necessary meetings that were held to keep the project on track were held at night so as not to disrupt the work. Glenn kept his promise — I did not have to build any new circuits; instead I had to maintain the ones that I had already built! This was in the days before transistors, when vacuum tubes had to be used for everything. The high mortality of the circuitry of the time was something that does not exist anymore.

From 1942 to 1946 I worked at the Metallurgical Laboratory, as it was known, learning the new arts of nuclear science. The work was fascinating and there was a lot to learn. The nuclear tools were rather primitive; hard careful work was necessary to gain the answers that we needed. For instance, to measure the energy of a gamma ray it was necessary to measure its rate of absorption in various materials by noting the decrease in the integral count as detected by a Geiger counter. Contrast this with the ease of making the same measurement in seconds with a modern germanium detector coupled to a multichannel differential pulse-height analyzer. It became particularly difficult when there was more than one component. It should be noted, however, that good work was possible even under these trying circumstances. For example, our discovery at Chicago of  $\alpha$ -particle backscattering that I did under Burris Cunningham was deduced by careful integral counting of the activity emitted by uranium samples in a  $2\pi$   $\alpha$ -counter. This effect was important, because it meant that the counting geometry for a weightless sample of plutonium in our  $2\pi$  chambers was 52%, not 50% as had been assumed, and this affected the assays of how much plutonium was being used in a given experiment.

As time went on, it became clear that my chief value to the project would be not only to keep the equipment operating properly but also to assist in the development of new and improved methods of detecting nuclear radiations. I soon became the head of such a group and spent a lot of time doing experiments aimed at solving some of

the knotty problems concerning nuclear detection methods as they pertained to the research on the chemistry of plutonium. By 1944 Seaborg felt that he and his chemists had the chemistry of plutonium under control so that he could devote some time to looking for new elements beyond atomic number 94. He assigned me the task of developing a method for measuring  $\alpha$ -particle energies with high efficiency and discrimination.

At that time the methods of making such measurements were very primitive and very inefficient. Library research soon showed us that no one had yet succeeded in that goal. We knew that one of the main problems which we would encounter was that, since it would be the target material in any nuclear bombardment and since the chemistry of the transplutonium elements was completely unknown, there would certainly be a large amount of  $^{239}\text{Pu}$  in the final sample. I soon decided that one possibility that offered hope for discrimination from the plutonium  $\alpha$ -activity was a range measurement, almost the equivalent of measuring energy. Since we were pretty confident that the range of the  $\alpha$ -particles from element 95 or 96 would be longer than that of the plutonium alphas, the plutonium alphas would be absorbed before those from the new elements, leaving the new alphas by themselves without any background. In the past range measurements had been made at low geometry, thin absorbers being added incrementally to produce an integral range curve. I suggested that we might be able to make a good range measurement in the  $2\pi$  ion chambers that we used for our regular assays of plutonium. The idea was to make the needed absorbers out of cleaved mica which would be very uniform in thickness and mount them on top of the sample to be analyzed. The end point of the absorption curve would be the range of the  $\alpha$ -activity. Tests showed that the method was a satisfactory compromise for the work and we used it with notable success. The range method served its purpose for the discoveries of americium and curium, the first of the transplutonium elements to be discovered; a couple of years later the method was made obsolete by the invention of the gridded ion chamber by O.R. Frisch.



**Fig. 2.** My wife, Wilma, daughter Kristine, and myself, about 1945.

In 1946, with the War over, Seaborg decided to return to Berkeley to resume his job as a university professor and I was one of those invited to return with him. It was a difficult decision for me, as Wilma and I had become acclimated to the University of Chicago environment and set down roots. Our first child, Kristine, had been born there in 1944 (Fig. 2 is a photo of the three of us in 1945) and, believe it or not, we had come to even enjoy the City and its miserable weather. However, for me the career that I had embarked on as a scientist was too attractive for me to abandon and we, too, rode the train back to the West Coast to help set up a new laboratory.

Our son, William Belt Ghiorso, was born late in 1946. Thirty-two years later Bill was to become a member of the Laboratory, also, and join me in a notable experiment to produce element 110. My life changed in many other ways, too. Because of Wilma I became interested in classical music, art, and opera, and now, with more time available, we began to take advantage of the cultural vistas afforded by the Bay Region. In 1951 we bought our first modestly priced oil painting and that opened our eyes to the virtues of having original art as an important part of our surroundings, and about ten years later we started hanging some of our collection in my part of the Laboratory (Bldg. 71) to the delight of my colleagues. One of the great joys of the Berkeley region was its proximity to the high sierra

country and we soon began to spend our summer vacations camping in such places as Tuolumne Meadows in Yosemite National Park. In those early uncrowded days it was a wonderful place to relax and learn something about the wonders of the world around us and the important lessons of ecology.

As time went on and I gained more experience my role as an innovator became ever more important. The discovery of elements 95–101 was made possible by their chemical separation from the other elements utilizing Seaborg's actinide concept, but their actual detection was made possible by the development of new sensitive instrumentation and this is the area that I was to specialize in.

By 1950 another problem had arisen; we knew that at some point we would have to resort to particles heavier than helium to produce heavier elements and Seaborg suggested that it would be a good idea for me to explore whether the venerable 60-Inch Cyclotron could accelerate useful quantities of heavier ions. I did not know anything about the details of cyclotron operation, so I initiated a program of heavy-ion research by spending one day a week at that machine working with Bernard Rossi, who was in charge of the accelerator, to see if we could accelerate ions like  $^{12}\text{C}$ . After a few months we were successful enough to be able to make several nuclear reactions in the transuranium region and show that a heavy-ion accelerator with more intensity and control of its energy was absolutely essential to our program if we were to go higher in atomic number.

Our success prompted Seaborg to request that a new accelerator be built at Berkeley for this research. Luis Alvarez, who had just invented the proton linear accelerator, suggested that Berkeley should build a linear accelerator for heavy ions and that it should make use of magnetic strong-focussing, a new principle that had just been invented at the Brookhaven National Laboratory. The AEC decided that two of them should be built, the other to be at Yale University. A study group was set up at Berkeley to design such a machine using scientists and engineers from both Yale and Berkeley.

The design effort went very well and in about a year the new linac was on the drafting boards and well into the initial phase of construction planning, etc., when an unforeseen stumbling block appeared. I attended the first Open House at the Laboratory, a joyous occasion as I remember, and everyone was there, including the Director, Ernest Orlando Lawrence. He took this occasion to ask me how the design of the new linear accelerator was going and said that he had been thinking about that machine and thought that it might be better if we built a cyclotron, instead! He suggested that a small group of interested people should get together the next day and discuss the idea. I was frightened by this proposal, because the design of our new machine was almost finished and the machine was ready to be built.

But this was E.O.L. himself, a very powerful individual and usually not thwarted, so the meeting was held. Ernest usually dominated technical meetings by the force of his personality and the fact of his many accomplishments, so it was not surprising that he would have an important influence in this battle of linac vs. cyclotron. I could see that the linac would go down to defeat unless it could be shown that it had some special advantage. It was a close call and the choice to a large extent depended on how one wanted to use the machine. Ernest was very enthusiastic about building a cyclotron, of course, and I could see that his point of view would carry the day unless I could point out something special about the linac. As the meeting went on I suddenly realized what that special advantage was.

At that time the problem of extracting the beam from a cyclotron had not been solved. The normal brute force method worked with very low extraction efficiency, a few percent usually, whereas in a linac it was obviously 100%. This argument was a powerful one and I kept playing this trump card whenever I felt that the cyclotron was winning out. The final outcome was that the linac design was allowed to go ahead. Twenty years later one could have made a better case for a cyclotron because of the development of the sector-focussed devices and ion source improvements, but at this time the

linac was certainly the best decision for Berkeley. If we had built a cyclotron, a chain reaction of later developments would have been precluded: the HILAC, the Omnitron, the SuperHILAC, the BevaLac, and RHIC at Brookhaven. Certainly, our heavy element program would have been delayed by several years. It is hard to know what the future would have been like; it certainly would have been different. After thwarting E.O.L. from making the HILAC into a cyclotron I thought that I would be *persona non grata* to him, but that was not the case at all. After the HILAC was built and operating he would often come around to find out how things were going, usually at night. He had the delightful habit of prowling the Hill at night occasionally to see who was working!

So construction proceeded and by 1957 the Berkeley machine (which I soon christened HILAC, for Heavy Ion Linear ACcelerator) was operating. The first experiment happened to occur on the same day that *Sputnik* was launched by the USSR. Thus was initiated at the same time the exploration of space by satellite and the exploration of the heavy element region by heavy-ion bombardment. Over the next ten years the HILAC allowed our team to produce for the first time elements 102–105. Element 106 would be made with the SuperHILAC.

While the HILAC design and construction was going on we exploited the 184-Inch Synchrocyclotron, using it to explore the region of elements between lead and uranium. The result was that a fascinating field was opened up, bringing to light a couple of dozen new nuclides as members of collateral  $\alpha$ -decaying series. The information enabled us to study in great detail the systematics of  $\alpha$ -radioactivity under the leadership of Prof. Isadore Perlman. In this work we took advantage of  $\alpha$ -particle recoil to demonstrate the family relationships of the members of the series. Our research was then extended into the rare earths, where we found a whole new region of  $\alpha$ -emitters.

In 1952 there occurred one of the most exciting incidents of my career, which culminated in the discovery of elements 99 and 100. The first hydrogen bomb test, conducted in the South Pacific by the

Los Alamos Scientific Laboratory, was analyzed by the Argonne National Laboratory and Los Alamos jointly and, initially, Berkeley did not even know about the operation. The amazing story of how we became involved is told in detail in Chapter 6 on einsteinium and fermium.

The bomb discovery of elements 99 and 100 was secret for a while and we began to worry that some other laboratory might find neutron-deficient isotopes of these elements by means of heavy-ion bombardment of uranium and, naturally, not knowing of our work, would want to name the new elements themselves. This would bring about a serious conflict, since we had already christened them einsteinium and fermium, so we decided to forestall this by finding these heavy-ion produced isotopes ourselves first. Since this work would not be classified, we would be able to publish it with a note that there was prior classified work on these elements which had priority of discovery. Our heavy-ion development at the 60-Inch Cyclotron paid off at this juncture by enabling us to find isotopes of 99 and 100 by  $^{14}\text{N}$  and  $^{16}\text{O}$  bombardment of  $^{238}\text{U}$ . This work was published and it did accomplish its purpose of reserving the names einsteinium and fermium.

Within a couple of years we found that we could make the same heavy isotopes that had been produced in the Mike explosion by long and intense reactor neutron bombardments of  $^{239}\text{Pu}$ . This material came from the so-called “napkin ring” bombardments in the MTR that had been initiated by Argonne and Berkeley around 1951. Micro amounts of einsteinium and fermium were now becoming available and we began to speculate as to how we could use this material to extend the periodic table.

For the elements beyond atomic number 100, however, conventional methods were not very efficient because of short half-lives and small amounts of activity. It became imperative that we develop new methods if we intended to climb up higher in atomic number. For the next element to be tackled, atomic number 101, I proposed that we bombard an unweighable target of  $^{253}\text{Es}$  with a superintense beam of helium ions at the 60-Inch Cyclotron. Most important, I

suggested that we take advantage of the recoil produced by the transmutation process to separate the product atoms of element 101 from the target. It turned out to be very difficult to apply the principle because of the very low recoil energy but after we had overcome that problem by making very thin targets it was a very effective technique. This pioneering experiment of identifying a single atom at a time is described in Chapter 7.

To make the experiment possible Bernie Rossi, the Operations Chief, and I, working with the chief designer at the machine, "Chuck" Corum, had to make some major modifications to the cyclotron to obtain the necessary high beam density required for the experiment. Finally we were successful after many months of hard work and we were able to bombard our precious target of einsteinium with  $10 \mu\text{A}$  of  $\text{He}^{2+}$  ions in an area that was only a few square millimeters in size. I was so impressed by Corum that a few years later I hired Chuck to come and work with us at the brand-new HILAC. He was responsible for designing the complicated equipment that we needed for the heavy element experiments at that accelerator.

In October 1957, at Seaborg's insistence I attended the Materials Testing Reactor-ETR Users Meeting in Cincinnati, which would profoundly affect the future of the heavy element research effort. I had been asked to contribute a short paper on what the needs of our group might be in the future for larger amounts of transplutonium elements. Seaborg insisted that I go and "stake out our claim." Without thinking that it might ever happen, I gave a rather fanciful talk, extrapolating our needs for *multigram* amounts of the heavy curium isotopes and, as best I could, what might be accomplished with such generosity. My seat happened to be next to Dale Babcock of the Savannah River Project (SRP), where Du Pont had built and was running several huge nuclear reactors devoted to making *tons* of plutonium for the military program. After I gave my glowing pep talk in which I had plugged for a step-up in the production rate of the transplutonium isotopes by several orders of magnitude via either the  $^{241}\text{Am}$  or the  $^{239}\text{Pu}$  route, I sat down and Dale astonished me by saying that the Savannah River Project reactors could easily

produce the amount of material that I had been suggesting. All that would be required would be to have the MTR fuel assemblies refabricated to the one-inch diameter required by the those reactors and irradiate them further. I became quite excited and at the next break I sought out Bill Crane to tell him the news. Bill got his Ph.D. in Chemistry at Berkeley under Seaborg and had worked closely with us on a number of heavy element studies. After graduation he had taken a position at the Livermore Laboratory and now he was attending the meeting as their representative. He became enthusiastic also and the three of us joined forces in formulating a possible scenario and timetable for making macro quantities of americium and curium at the SRP. This would be used as target material for use in the projected High Flux Isotope Reactor (HFIR) at ORNL which, with a neutron flux of  $\sim 5 \times 10^{15}$ , would be able to make berkelium, californium, and einsteinium in large quantities.

The whole idea was quite stimulating to us. Here was the beginning of a grand new program that looked to be both scientifically and politically feasible. In fact, I was so taken by it that on the plane back home I wrote a memo to Glenn in which I espoused our proposed plan and delivered it to him the next day. The next step, Seaborg soon decided, was a letter to Lewis Strauss, the Chairman of the U.S. Atomic Energy Commission (AEC). In this October 1957 letter he recommended the need for a “very high flux reactor” (the HFIR which ORNL had proposed) and a twofold program to: (1) irradiate  $^{239}\text{Pu}$  in a high flux production-type reactor (the SRR) to produce  $^{244}\text{Cm}$ , and (2) irradiate curium in the “very high flux reactor” to produce berkelium, californium, and einsteinium in substantial quantities (milligrams!).

This was the beginning of a huge program that was to be so important to future developments in the heavy element field and it was going to take many years and cost many millions of dollars. Fortunately, Seaborg himself was to become the next AEC Chairman, so he was in a perfect position to oversee it and make sure that the program stayed on course. And that it did. The HFIR was built and operating by 1965. In addition, the Transuranium Processing Plant

(TRU), needed to process the transplutonium products from HFIR, was built and operating by 1966.

The National Transplutonium Program was a cooperative one, aimed at benefiting all the laboratories in the US, so in 1964 the Transplutonium Program Committee was set up with members from the principal laboratories involved in heavy element research — Argonne, Berkeley, Livermore, Los Alamos, and Oak Ridge — to advise the director of AEC's Division of Research on how the actinide products should be allocated. There was usually an equal division of the production to the five principal members: but that was often modified when a particular laboratory had a special experiment which needed all of the current production. I served on the Committee continuously until it was disbanded. I recall asking the Committee in 1967 for the allocation of *all* of the first batch of pure  $^{249}\text{Cf}$  (amounting to several hundred micrograms) that would be milked from the  $^{249}\text{Bk}$  "cow" which had been purified and set aside for the purpose of growing  $^{249}\text{Cf}$ . I told the Committee that it was to be used in our initial attempt to make element 104. The Committee, under the capable leadership of first Alexander Van Dyken and then John Burnett, was very proud when I was able to report back to them a year later that we had succeeded. Van and John were very popular with the members for the wise roles that they played in satisfying the varied, sometimes conflicting, interests of the members during the many years that the Committee existed. Often, in the early days of the Program when the actinide element shares were meager and the competition between the laboratories was keen, there were lively debates over where the "goodies" should be shipped first. These disputes were always settled amicably by cooperative agreements and a trading of priorities, the guiding rule being that the materials should be used to produce the best science.

The equipment needed for us to discover elements 104, 105, and 106 became more and more complicated, of necessity, because the bombardment yields decreased steadily as the atomic number increased. Fortunately, the development of solid-state detectors made it possible to design experiments which were marvels of sensitivity.

The last of these, for element 106, was even able to demonstrate the presence of the great granddaughter of  $^{263}\text{106}$ . The apparatus had the designation "VW." This stood for "Vertical Wheel," a descriptive term for an apparatus with unparalleled sensitivity at the time. It was the culmination of a line of instruments that identified  $\alpha$ -particle activities by their  $\alpha$ -energies and by the genetic relationships that they had to other  $\alpha$ -activities. With this instrument we were able to refine our research and characterize isotopes with great accuracy, even when there were interfering activities present.

Though we specialized in  $\alpha$ -emitters we did not neglect those nuclei that decayed by spontaneous fission. G.N. Flerov and his group emphasized SF emitters from the beginning of their foray into the heavy element field and continued to make claims that had to be contended with, starting with element 104. It was necessary for us to find out whether their claims were correct, so we gradually devoted a fair amount of time to checking them. We soon found out that it was not possible for anyone to be absolutely certain of the isotopic or element assignment of an activity of this sort with the exception of the few times when it was possible to carry out well-established chemical procedures. We soon let it be known that we considered that one  $\alpha$ -particle was worth at least ten thousand fissions! However, we did much research in this area under the tireless leadership of Matti Nurmi and were able to find several important isotopes that decayed principally by spontaneous fission.

Two very important events happened during this period. Iz Perlman had asked me if I would watch over the HILAC and I did. Soon, however, I found that the actual technical job took too much time away from my research, so I hired a young man, Bob Main, away from TracerLab to take care of these day-to-day duties. That was an excellent move, because Bob (see Fig. 3) was not only an excellent physicist and engineer, he was also a businessman and that skill soon became very important as we got involved in the design of new machines. Though I no longer had to watch over the detailed operation of the HILAC, I remained interested in accelerators in general and I would often discuss fine points with Bob. Thus,



**Fig. 3.** Robert Main, in charge of the operation and development of the HILAC and the SuperHILAC. Coinventor of the Omnitron.

when Glenn, who had become the Chairman of the Atomic Energy Commission in 1961, informed me in a chatty letter from Washington that the Argonne Laboratory was thinking of building a large cyclotron for heavy ion acceleration, I took it as a challenge and began to ponder what we might do for Berkeley's future. I posed the problem to Bob and found that it was a very expensive proposition to make a magnet with a pole diameter of the order of 200 inches! We were thinking of a magnet that large so that we could take ions out of the HILAC up to higher energies principally for the use of the biomedical community; and with such a huge magnet we knew that we would be able also to accelerate the abundant low charge state ions to get large beam currents of ions with energies suitable for making nuclear reactions. Bob had invented a new method of winding magnet coils with heavy copper tape which promised great savings, but a careful study quickly uncovered severe problems and we abandoned that approach. Once we had got our feet wet in the field of heavy ion accelerators there was no turning back and we returned in our thinking to the cyclic machines, still guided by the desire to accelerate high energy heavy ions for biomedical usage.

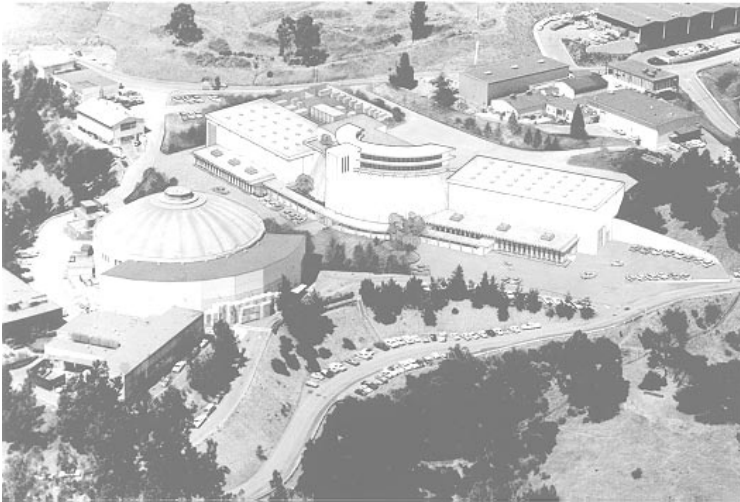
There were a few more iterations in our thinking until one memorable hour in 1964 when Bob Main, Bob Smith, and I invented a new

type of accelerator which we called the Omnitron, one which could accelerate all of the elements to either low or high energies. It was a real breakthrough, far ahead of its time, and was one of the world's first complicated accelerator concepts. This machine would have accomplished its purpose by the use of two large synchrotron rings of magnets in which the particles were accelerated and/or stored. The particles could be passed easily from one ring to the other so that a cyclic regime could be set up in which particles could be accelerated first in a low and then in a high charge state. We even pointed out that radioactive ions made by fragmentation could be accelerated efficiently — a modern concept. If we had built the Omnitron there is no question but that the history of the Lab would have been changed drastically, because many powerful tools would have become available two or three decades before their time.

The other event of great importance was the publication in the same year of the seminal work of Bill Myers and Wladek Swiatecki which suggested that a possible closed neutron shell at 184 neutrons and one at 126 protons could lead to a region of very stable super-heavy elements (SHE). Some of the best combinations of elements needed to make the SHE were of high atomic number, so it became immediately clear that our proposed machine, the Omnitron, would be the ideal accelerator to implement the fusing of the necessary atoms to reach this Magic Island. However, our original reason for inventing the Omnitron was to accelerate heavy ions such as neon to energies high enough to penetrate the human body for the treatment of cancer.

Fortunately, Seaborg was the Chairman of the AEC and he was definitely interested in our pursuing the search for SHE, so it was not difficult to get him interested in the capabilities of our novel machine. Funds were soon made available for us to do a design study and this led to a full-fledged proposal to construct the Omnitron. Figure 4 shows an architect's model of the Omnitron superimposed on the hill area near the 184-Inch Cyclotron. The study, very carefully done by a group of the best accelerator people in the US, concluded that the Omnitron would have a construction

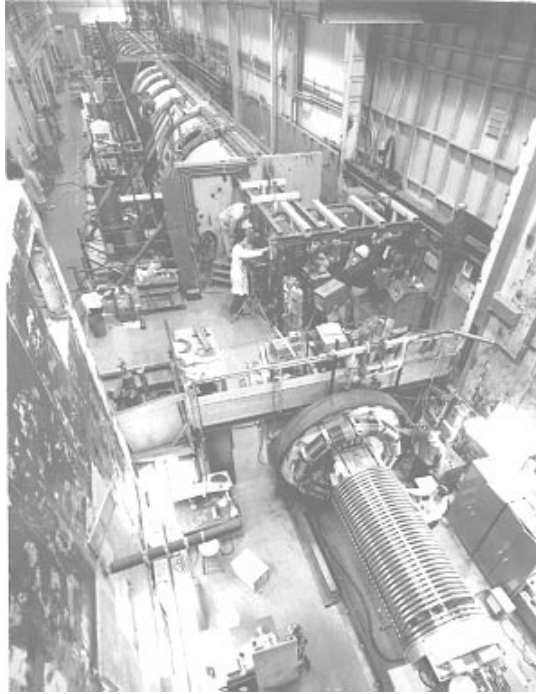
cost of \$28 million but operating costs that were quite low, only about \$3 million/year; it was deemed to be a highly worthwhile project. Twenty-five years later the design was found to be still excellent and not at all outdated. Figure 5 is a photograph of the author at about this time.



**Fig. 4.** Architect's drawing of the Omnitron model superimposed on the hill area near the 184-Inch Cyclotron.



**Fig. 5.** Giorso, approximately 1969.



**Fig. 6.** The SuperHILAC in the final stages of construction, 1971. Some of the iron shielding is not yet in place. The ADAM injector is at the lower right.

Unfortunately, the accelerator was never built because it came into competition with the terrible holocaust war in Vietnam, where the US was spending the equivalent of three Omnitrons per day to devastate the country! We had prepared a “fallback” position in case we failed to get approval, in the form of important improvements to the HILAC that would cost only \$3 million. We were able to get these funds and the SuperHILAC came into being in 1971. Figure 6 is a photograph of that machine in the final stages of construction.

In retrospect, I think that we pursued the wrong strategy. I now believe that we should have continued to press for the Omnitron. There is no question that we would have failed to get approval for

the project in the following year or two, but it was clearly a superior accelerator with no competition in its field and I firmly believe that eventually it would have won out. It was 25 years ahead of its time and would certainly have changed the course of nuclear physics at LBL.

Though not as versatile as the Omnitron, the SuperHILAC was a great improvement over the existing HILAC and with it we were able to pursue our heavy element research. It was with this machine that we were able to discover element 106 in the form of  $^{263}106$  in 1974.

In 1971 the BevaLAC was conceived. At the time this was a startling concept, and it came about in this way. I had felt guilty that we had not been able to build the Omnitron for the biomedical community and kept promising Cornelius Tobias and John Lawrence that I would find some way to increase the energy of the heavy ions that would come from the SuperHILAC. At first, we thought in terms of just adding more linacs, but it became clear that this would be impossibly expensive. However, one day when I was pondering a layout of the Lawrence Berkeley Laboratory that showed all of the accelerators on the Hill, I noticed that the Bevatron in plan view seemed very near to the SuperHILAC. Somewhat flippantly, I made what seemed like a good joke by saying that we ought to inject our SuperHILAC beams into the Bevatron! It seemed like such a novel idea that I thought we had better calculate why this was not feasible, but a few minutes of calculation by Frank Selph showed that it could be done. Figure 7 is a schematic diagram of the BevaLAC arrangement. It was thus that the BevaLAC was born and on the spot I coined that acronym. With Seaborg's help in securing funds from the AEC (he had just completed his ten-year stint as Chairman in Washington and had returned to Berkeley) the necessary transfer line was constructed to connect the two accelerators, the BevaLAC came into being and my promise to John Lawrence was fulfilled. It was very successful and after a learning period of several years it was shown to be an important biomedical tool. In addition, interest was aroused in using it as a probe to form ultradense matter, the

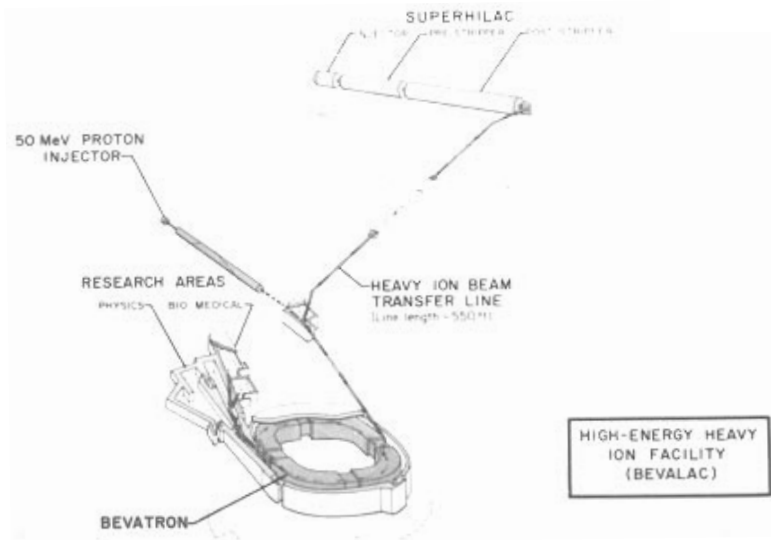


Fig. 7. Schematic diagram of the BevalAC arrangement.

famous *quark-gluon* plasma which must have existed for a brief instant at the time of the big bang.

This early work with the BevalAC aroused a lot of interest among the high energy nuclear physicists and after a few years prompted the development of a heavy ion capability at the existing large hadron accelerators to achieve much higher energies. No *quark-gluon* plasma evidence was seen and it was realized that even higher energies would be needed. This insight led to the construction of the heavy ion collider, Relativistic Heavy Ion Collider (RHIC), at Brookhaven to go in the existing tunnel that had been constructed there for Isabella, a high energy proton accelerator that was never built. Thus, it is evident that the BevalAC not only kept the Bevatron going for another 20 years, it also opened up a completely new field of physics and thus had a very great impact on worldwide research. The irony of it is that when the Bevatron was shut off in 1993 the SuperHILAC was also shut down as its injector, even though there were many important experiments that could have been done at the

SuperHILAC that could not be performed by any other accelerator in the world at that time.

My promise to the biomedical community was fulfilled, but what about the low energy uses that the Omnitron was designed to handle? The SuperHILAC was meant to accelerate copious beams of heavy ions with atomic number up to 36, i.e., krypton. This usage of the machine had many growing pains and though it was designed to work in a time-sharing mode with three different injectors so that one beam could be injected into the Bevatron while a different beam could be used for low energy experiments at the SuperHILAC, it was very difficult to accomplish this all of the time. It seemed that the highest priority usually went to the Bevatron, since it was the most expensive part of the complex to operate.

However, some of the time it worked very well for us. We were able to mount a very excellent experiment in 1974 in which we discovered element 106 by bombarding  $^{249}\text{Cf}$  with  $^{18}\text{O}$  ions (see Chapter 10). This was done following many different types of experiments aimed at making the longed-for superheavy elements. None of them worked and in retrospect it was mostly time wasted that could have been devoted to more productive research. But there was no way that we could foresee that this would be the case! When we finally tried the element 106 experiment, we were on more familiar ground which was an extension of what we had been doing for years.

During the period that the Omnitron was under development by a corps of some of the best accelerator physicists in the world who had been recruited by Bob Main, I followed the work with great interest and as a result came up with an excellent idea for a new kind of separator. I was struck by the fact that a fast-moving ion of a heavy element that emerged from a foil into vacuum had a higher mean charge state than when it emerged into a region of low gas pressure, and furthermore, the charge state attained depended on the atomic number of the ion. Might this be a way of separating a heavy ion recoil from the beam that produced it?

I was struck by the simplicity of the idea, so I did a simple experiment at the exit of the HILAC in April 1967, before it was

converted into the SuperHILAC. I mounted a thin target of  $^{165}\text{Ho}$  at the entrance to the steering magnet at the end of the accelerator and bombarded it with a beam of  $^{40}\text{Ar}$  ions. An aluminum catcher foil was mounted downstream at the end of the magnet so that an  $\alpha$ -radioautograph could monitor how much the 40 MeV  $\alpha$ -emitting astatine recoil products of the bombardment were bent relative to the argon beam by a given magnetic field when the pressure was about 1 torr of helium. An excellent separation was obtained. A later experiment using 67 MeV  $^{12}\text{C}$  ions to bombard a gold target also worked extremely well, with the 4 MeV astatine recoils also being completely separated from the beam. In addition, it was noted that a sharp image of a collimator was obtained and this indicated that charge-exchange oscillations occurred very frequently along the recoil trajectories so that the magnetic rigidity was constant. I learned somewhat later that my work had been preceded by Fulmer and Cohen at the Oak Ridge National Laboratory when they applied the principle to the separation of fission products recoiling from a fission source.

Unfortunately, this promising technique was not adequately followed up until much later, because the successful experiments on elements 104, 105, and 106 took precedence and they used our more conventional techniques. It was not until 1972 that our first steps were taken to make a gas-filled recoil separator that would be suitable for work with heavy ions. A device called SASSY (Small Angle Separator System) was constructed using available magnets from the Bevatron. It had some good ideas, such as the measurement of the time of flight (TOF) of the recoils, but suffered from the fact that the discrimination at the focal plane from beam particles was insufficient at high beam levels. In addition, the silicon detector array was not large enough to catch all of the fusion recoils.

Some years later another version of the idea was constructed, called SASSY2, which remedied these faults. It had a larger acceptance for the recoils and these were bent through a much larger angle. By this time I had "retired" and so had no access to funds which would have allowed the instrument to be constructed in the

normal way. Faced with this situation, I had no choice; if I wanted to pursue the heavy element research in this way, I would have to build it myself. I had often built parts of the instruments that I used but now I would have to build the major parts of SASSY2. For instance, the pole pieces would have to be milled in a complicated way to provide the necessary double-focussing. This was a daunting task but my son, Bill, persuaded me that it could be done and offered to help me with both advice and labor. He had joined the Lab about ten years before and had become an acknowledged expert in mechanical, electrical, and computer technology. His help and enthusiasm were prime reasons for the successful operation of SASSY2 and we did succeed in making a viable instrument that was used for one important last experiment. This was a search for element 110 with mass 267 produced by bombardment of  $^{209}\text{Bi}$  with  $^{59}\text{Co}$  ions. In a very difficult 40-day period we did find one event which we attributed to this atom and that experiment is described in Chapter 12.

Subsequently, the SuperHILAC was shut down, prematurely ending our encouraging experiments there. However, SASSY2 was especially important in that it led directly to the design and construction at the 88-Inch Cyclotron of the Berkeley Gas-filled Separator (BGS), the next generation of gas-filled separators that came on line early in 1999. (See Chapter 14.)

With this device we were able to mount an experiment to look for the isotope,  $^{267}\text{110}$ . In a very difficult 40-day period we did find one event which we have attributed to this atom. The experiment is described in Chapter 12.

### **P.3. Glenn T. Seaborg**

I had almost no exposure to science in my early years. I was born of Swedish ancestry in Ishpeming, Michigan, a small iron-mining town on the Upper Peninsula. My father, H(erman) Theodore Seaborg, was born in 1880 in Ishpeming. His parents came from Sweden to Ishpeming in their youth and met and married there in 1872. His