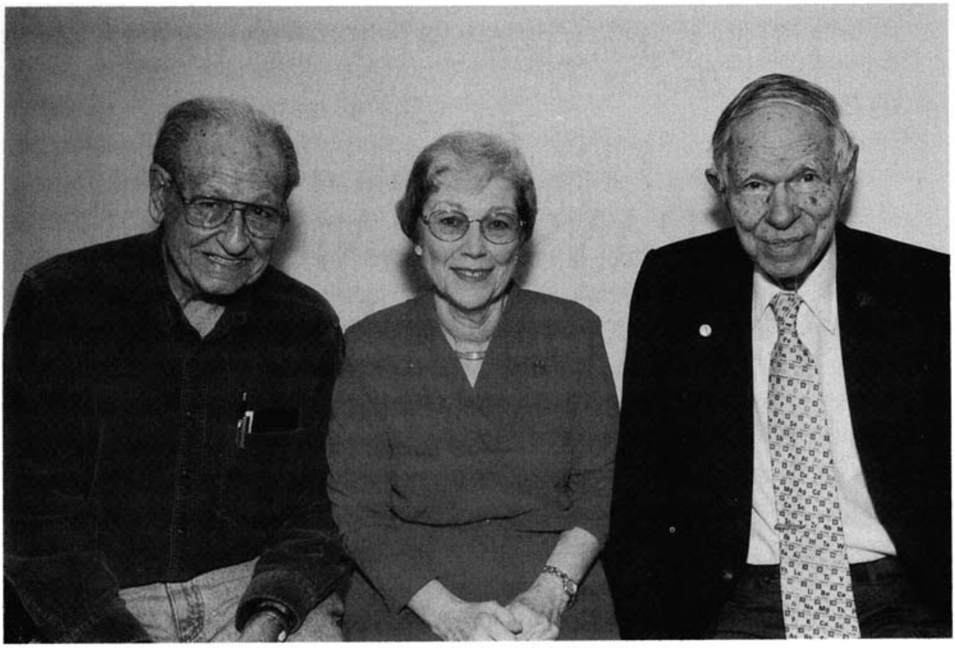
THE TRANSURANIUM PEOPLE



The authors (*left to right*): Dr. Albert Ghiorso, Dr. Darleane C. Hoffman, and Dr. Glenn T. Seaborg.

THE TRANSURANIUM PEOPLE

THE INSIDE STORY

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Glenn Theodore Seaborg (1912–1999)

Glenn Theodore Seaborg, internationally renowned nuclear chemist, Nobel laureate in Chemistry, educator, science advisor to ten U.S. Presidents, humanitarian, and avid hiker, passed away at the family home in Lafayette, California, on Thursday, February 25, 1999, following a stroke suffered while attending the American Chemical Society meeting in Boston in August 1998.

Glenn was born in Ishpeming, Michigan, on April 19, 1912, of Swedish ancestry, a heritage of which he was very proud. When he was 10 years old the family moved to a small town near Los Angeles, because his mother wanted to expand her children's educational opportunities and horizons. He attended high school in the suburb of Watts and in 1929 entered the University of California at Los Angeles (UCLA), where he received his bachelor's degree in Chemistry and then stayed on for a fifth year to take graduate courses in Physics. In August 1934 he began graduate studies in Chemistry at the University of California at Berkeley (UCB) at a most propitious time following the invention of the cyclotron, in what he described as a "glamorous, exciting atmosphere." He completed his Ph.D. degree in May 1937, became the personal research assistant of Prof. G.N. Lewis, and joined the UCB faculty.

In early 1940, shortly after Edwin M. McMillan and Philip H. Abelson discovered neptunium (element 93), McMillian started looking for element 94 in bombardments of uranium with deuterons in the 60-Inch Cyclotron, but he was suddenly called away to wartime radar work at MIT before he could finish the project. Glenn contacted him and received his generous permission to continue this research, and in February 1941 Glenn, together with his first graduate student, Arthur C. Wahl, and fellow instructor, Joseph W. Kennedy, produced and chemically separated plutonium.

Seaborg joined the wartime Manhattan Project in 1942 to work on the Plutonium Project, and when the decision was made that he should move to Chicago, he proposed to Helen Griggs, then E.O. Lawrence's secretary. They were married on June 6, 1942, and Seaborg often fondly called Helen "his greatest discovery of all." She and five of their six children survive him.

In 1944 and early 1945, the discovery of the new elements americium and curium was made possible by Glenn's recognition that they were part of an "actinide" series, and, thus, being trivalent in aqueous solution, could be chemically separated on that basis. He published his famous periodic table showing the "actinide series" in *Chemical & Engineering News* in 1945, the same year he returned (taking Albert Ghiorso with him) to become a professor of chemistry at UCB. Seaborg and his group discovered berkelium and californium in 1949–1950, and in 1951 he shared the Nobel Prize in Chemistry with McMillan for research on the transuranium elements. He was to be codiscoverer of five more elements: einsteinium (99) in 1952; fermium (100) in 1953; mendelevium (101) in 1955; nobelium (102) in 1958; and element 106 in 1974. The name for this last element, "seaborgium," was approved in 1997 by the International Union of Pure and Applied Chemistry, and Seaborg regarded this as an even greater honor than the Nobel Prize.

Seaborg served as Chancellor of UCB from 1958 until 1961, when he was chosen by President J.F. Kennedy to chair the U.S. Atomic Energy Commission, a position he held until 1971. He led negotiations resulting in approval of the limited nuclear test ban treaty in 1963 and led delegations to a multitude of countries to promote the peaceful uses of atomic energy. After his return to Berkeley in 1971, he resumed teaching duties, which he carried out until 1979, supervising the Ph.D. research of more than 65 students. He founded the Lawrence Hall of Science in 1982. He authored more than 500 scientific articles and numerous books. Because he had an acute sense of history, he somehow also found the time to edit his valuable extensive and detailed journals, faithfully kept throughout his long career.

It should also be mentioned that Glenn loved sports and was a staunch supporter of the athletic program at Berkeley. Glenn and Helen loved to hike and laid out an interconnected network of 12-mile trails in the East Bay Hills above Berkeley which, when extended to the California–Nevada border, formed a link in the cross-country trek of the American Hiking Society.

In spite of his legendary accomplishments, Glenn Seaborg was basically a humble person who always had time for students, family members, and even nonscientists who wanted to visit with him. He prepared as carefully for lectures to freshman chemistry classes as for appearances before audiences of renowned scientists.

Our last formal collaborative effort with Glenn resulted in this book. Of course, in typical fashion, he completed his portions long ahead of time, but we worked long and hard to get all the pictures in and have him check the final texts before he left for the August 1998 American Chemical Society in Boston, where he was to receive one of his last accolades. He had been voted as one of the "Top 75 Distinguished Contributors to the Chemical Enterprise Over the Last 75 Years" by the readers of *Chemical & Engineering News*. He was presented with this award at a huge ceremony and reception on Sunday, August 23 — the evening before he suffered the stroke which ultimately led to his death.

We have lost a treasured colleague who, besides being a mentor, an advisor, and a unique resource, was above all a dear friend. We will sorely miss him, but he will live on through his prolific writings and in the cherished memories of the hosts of students, scientists, colleagues, politicians, and lay people whom he has influenced.

Albert Ghiorso
Darleane C. Hoffman

April 1999

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Preface

Intimate Glimpses of the Authors' Early Lives

P.1. Darleane C. Hoffman

The story of how I came to be a "transuranium person" and one of the coauthors of this book is not straightforward and certainly not predictable. If you believe in astrology (I don't), perhaps the only omen is that I was born on November 8, a birthday that I almost share with Marie Curie and Lise Meitner. These two famous women pioneers in nuclear science were both born on November 7, although Meitner's birth date seems not to be entirely certain. Of more importance is that I was born into a family that prized education highly — and I recall my mother giving me little "intelligence" tests at a very early age!

My father, Carl Benjamin Christian, was born on October 7, 1898, to Bessie Ingelena and Albert Gustav Christian on a farm near Decorah, Iowa, a predominantly Norwegian community, and his parents were both of Norwegian ancestry. He had an older brother and two younger sisters. My mother, Emma Elvina, was born on March 2, 1900, to Mary Jane (Taylor) Clute and Eugene Clute on a small farm in Elk Creek Valley near the little town of Colesburg, Iowa. This was a largely German community, although her parents were of English and Dutch descent. She had an older sister, Nellie, born in 1897, and a younger sister, Minnie, born in 1903. The three girls were left orphans when their mother died of "consumption" in 1906. Although Nellie tried to help care for them, their father was unable to cope with the situation and found refuge in alcohol. My mother, who was an extremely attractive child, was soon adopted in February 1907 by a German couple named Emma Minnie and Henry John Kuhlman, whose only daughter had died as a child. The other sisters were taken into foster homes just before being taken off to the orphanage, but were never formally adopted. The Kuhlmans decided to rename Emma Elvina "Elvina Emma" to avoid confusion with her new mother's name, but somehow in the process of recording it on the adoption papers, it was recorded as "Elverna Emma" — and that is how it stayed! Her life in her new home was a happy one — her new parents adored her and she, now being the only child, was in her own words "spoiled rotten." However, she sometimes found it difficult to fit into the German community, in which German was spoken often at home, at Church, and even at school.

My parents both graduated from Upper Iowa University: my mother with degrees from both the School of Music and the School of Oratory, and my father in Mathematics and Education. They met again later in Chicago, where my mother was working and my father was taking graduate work in education at the University of Chicago. They were married a couple of years later, in June 1925.

Fig. 1. Elverna E. and Carl B. Christian, 1925.

Photos of them at that time are shown in Fig. 1. That August they settled in their new home in the small northwest Iowa town of Terril, where my father at age 27 had been elected to the position of superintendent of the consolidated public school there. I was born at home on November 8, 1926, with my mother's oldest sister, now a registered nurse, and my grandmother Kuhlman in attendance to care for us. I became very close to my grandma, as in subsequent years she stayed with us on many occasions and often took care of me. I also sometimes visited my grandparents Kuhlman in the big city of Waterloo, Iowa, where she would take me to the movies on Saturday afternoon. In the evening we would often sing and they taught me little songs in German.

I was a frequent, extremely young attendee at a variety of school functions in Terril, including sports events, high school plays, musical events, and box socials. Often a couple of the unmarried women teachers "roomed" in our home and would baby-sit me in the evenings when my parents had to go out to "adults"-only functions. It was a special treat for me to be allowed to visit in their rooms and talk with them. My only sibling, my brother Sherril, was not born

Fig. 2. Darleane Christian at five years old with baby brother, Sherril, December 1931.

until September 28, 1931, when I was nearly five years old. A picture of me taken with him just after my fifth birthday is shown in Fig. 2.

I did not start first grade in the Terril Public Schools until the following year, when I was nearly six years old, because my father was a firm believer that *nobody* should start school until they were five years old, and since my birthday was not until November, I had not been allowed to start the previous year. Of course, I could already read and was ready to start, but he surely could not make an exception for his daughter! I remember my first and second grade teachers very well, as they had been among those who had stayed in our home. They were both excellent, "no-nonsense" teachers who kept track of three different levels of reading and arithmetic groups and still found time to help those who needed extra attention or to give me extra work to do to keep me from being bored. In those years our newspaper carried items such as "First Grade — *In a phonics test given this past week five children made no errors. They were Lillian Walton, Darleane Christian, Betty Blum, Wayne Glover and DeLane Anderson.*" (I still correspond with DeLane, who was my best friend.) And "*Our honor roll for this period is: Darleane Christian, 55 points; Mildred Wray, 52 points; Ruth Rouse, 44 points. There were three A's this period. Darleane Christian and Mildred Wray had A in numbers and Mildred Wray had A in drawing and handwork. Dean Higley and Wayne Glover have been promoted to the A Division.*" Maybe if we still gave frequent public recognition for excelling in academic subjects as well as in sports, our present day students would have more regard for learning! Similarly our poetry was sometimes published. One of my second grade contributions which should have convinced everyone that I was not to be a poet was "*Spring is here at last, / And the winter is long past, / April is here with flowers, / To bring May's fair flowers. / Birds are singing in the trees, / And back have come the bees.*" A photo of my second grade class together with the first grade class is shown in Fig. 3. I am in the third row from the bottom — the second from the right.

During these years the U.S. was in the middle of the Great Depression. Consequently, my father was not paid during the summer

Fig. 3. First and second grade classes (1933–34) at Terril, Iowa Consolidated Public School.

months, so a couple summers we lived in a cottage at nearby Lake Okoboji, where our family and my father's brother and his family lived in a cottage on the lake and ran the nearby gasoline station. This was a wonderful time for me, as my cousins were the same ages as my brother and I. We played we were "Indians" in the woods and I learned early to swim in the lake and developed my lifelong love of swimming. We learned to play various card games, read — and on special occasions went to a nearby amusement park where we were not allowed to go on the roller coaster! Another summer, several years later when I was about nine, just our family ran a concession stand on the beach, selling snacks, cold drinks, popcorn, and renting out boats. That summer I helped at the stand, baby-sat my little brother, and on occasion rescued toys and even a few toddlers that had floated out to deep water, sometimes being rewarded by their parents with "tips." We often went out in the boat fishing with my parents. My main incentive for this was to help with the rowing and if I would sit quietly for awhile, which I hated, I was then allowed to dive and swim off the boat in the deep water. These were idyllic times, in spite of having very little money to spend. I remember having saved some of my "tip" money and debating whether to buy a new bathing suit (which I badly needed!) or a large beach towel. I finally chose the beach towel, because it was less expensive, I knew I wouldn't grow out of it, and once in the water no one could see my bathing suit anyway!

We also visited my grandparents Christian on the farm near Decorah, where I found out about farm life — watching the cows follow the lead cow home from the pasture, helping my grandma "milk" them, churning butter, feeding the lambs milk from a bottle, taking lunch out to the men working in the fields at harvest time, sitting atop the huge draft horses, and lying on a blanket on the grass on summer evenings to watch the stars and listen to my father's two sisters tell me about the constellations. Sometimes at Christmas we would drive to visit them and my grandpa Christian would bring the horse-drawn sleigh to pick us up at the main road to take us through the deep snow down the hill to their home — one of my most treasured experiences. In the summer of 1933, I was allowed to accompany my parents and my aunts on the drive to Chicago, where we stayed for several days to attend the World's Fair. What an unbelievable and fabulous experience for me!

In 1934, after my second grade in school, we moved to Coon Rapids, a somewhat larger town (about 1500 people at that time) south of Des Moines, the capitol city of Iowa. Again, my father was school superintendent, but he also took on the job of coaching the girls' basketball team, since no one else could be found to do it. My mother would go to the games and run the popcorn popper and sell popcorn to make money for the sports program and new uniforms for the girls. My father was very successful as coach and soon everyone was attending the girls' games and leaving when the boys' teams came on to play! I played on the girls' junior high team, but it became obvious I would never be a basketball star — at barely five feet tall I simply couldn't compete with the nearly six-foot forwards that were the mainstays of girls' basketball at that time! However, I participated in a wide variety of musical activities, learning to play the piano, playing the saxophone in the school band, and singing in the chorus and small groups. In the summers my friends and I bicycled, swam at the local pool, played croquet, played the piano and sang, and I read nearly every book in the local library. I attended third through ninth grades in Coon Rapids and had many close friends and was president of the freshman class. So it was a great shock to me when my father accepted a position as Superintendent of the Public Schools in West Union in northeast Iowa at the end of my freshman year in high school. I'm sure this was partly because both my parents had grown up in that area and we would be closer to my father's aging parents, who lived only 30 miles away and not far from my mother's parents in Waterloo. We moved there in the summer of 1941 and I began my sophomore year in high school.

Mathematics was my father's teaching specialty and was always a favorite subject in our home. We were entertained on long drives by doing the squares of the numbers up to 20 in our heads and calculating square roots with pencil and paper. (My brother later went on to major in Mathematics as an undergraduate, but switched to Physical Chemistry as a Ph.D. student at Iowa State University.) I took all the mathematics courses our schools offered. I even took advanced algebra, although that meant I had my father as a teacher, which I considered a very difficult situation as I was afraid the other students would think I was being favored! Not only that, I didn't even dare ask for help with my homework. (I took a trigonometry course one summer by correspondence, since it was not offered in our school.) I continued my interest in music and art, learning to play the flute and oboe in addition to the saxophone, and became a member of several choral and instrumental groups which won prizes at high school music competitions. I also took private art lessons. At my mother's insistence, I participated in dramatics and oratory groups and although this training was no doubt good for me, it was not my favorite extracurricular activity.

By February 1941, Glenn Seaborg had already become a "transuranium person" when he, together with his colleague Joseph Kennedy and his graduate student Arthur C. Wahl, made the first chemical separation and unequivocal identification of the new element plutonium. But I was still in high school and oblivious to all those developments. However, I vividly remember the bombing of Pearl Harbor on December 7, 1941, which was announced while I was at a Sunday afternoon Community Chorus rehearsal of the *Messiah* at the local high school. The subsequent two years of high school were darkened by the pressures of a global conflict as we watched all the men teachers and many senior students go off to war while at home we coped with rationing of food, gasoline, and a variety of shortages, and waited for the lists of war casualties. I corresponded with several young men throughout the war — fortunately, they all came home safely. My father tried desperately to enlist but was repeatedly turned down because of his age and position.

I graduated from high school in 1944. A young man and I were covaledictorians with the highest grade averages ever recorded in the high school there. I decided to enter Iowa State University at Ames, Iowa, but had difficulty trying to decide whether to major in Applied Art or Mathematics, but finally settled on Applied Art. In those days Applied Art was in the College of Home Economics, and fortunately for me, I was required to take Home Economics Chemistry. (I had never studied Chemistry as it was not offered in our high school, but I did take Physics from a substitute teacher and perhaps because of that found it less than exciting.) The beginning Home Economics Chemistry at Iowa State was taught by Prof. Nellie Naylor, and largely due to her outstanding teaching I found myself more interested in Chemistry than anything I had ever studied. She had a way of making it all seem so beautifully logical as well as relevant to a host of everyday problems. Consequently, I decided by the second quarter that I would switch my major to Chemistry. This somewhat unconventional choice caused my Applied Art Counselor to ask me, "Do you really think chemistry is a suitable profession for a woman?"! I replied that I was quite sure it was. After all my excellent Freshman Chemistry teacher was a woman. However, both these women were what we used to call "spinsters," and I vowed that I would not necessarily emulate them in this respect but would maintain other interests and continue to date a variety of young

Fig. 4. Darleane Christian, photo for Iowa State College Yearbook, 1945.

men and might even consider marriage as well. So in Spring quarter 1945, I became a Chemistry major in the College of Science. (See my class photo taken in Spring 1945 in Fig. 4.) From that time on I was usually the only woman in most of my classes, but this bothered me not at all, nor did it seem to bother the young men in my classes. After all, during the war years young men were in relatively short supply, although at Iowa State there were a number of officer training programs and the veterans began returning to school on the "GI Bill of Rights" at the end of the war.

By 1945, Seaborg had been leading the effort at the Metallurgical Laboratory in Chicago for several years to develop a process to separate plutonium from the large amounts of fission products and the uranium in which it had been produced by neutron irradiation. The process had been tested in a pilot plant at Oak Ridge and the large processing plants at Hanford Washington had gone into operation in December 1944. Albert Ghiorso joined the "Met" Lab in 1942.

During my first years at Iowa State (1944, 1945), we would often hear all kinds of wild rumors about what might be going on at "Little Ankeny." This was an installation of some rather drab, temporary-looking buildings on the edge of the campus from which some rather spectacular flashes of light were seen to originate from time to time that illuminated the night sky. It was not until much later that I learned of the "project" at Iowa State College under the leadership of Frank H. Spedding and Harley A. Wilhelm to solve the problem of reduction of uranium to very pure uranium metal and deduced it had something to do with that (Spedding had come to Ames after receiving his Ph.D. at Berkeley under the direction of G.N. Lewis). For a time, Spedding was also director of the Chemistry Division of the Metallurgical Laboratory at Chicago. He was Director of the Institute for Atomic Research at Ames when I first joined that institute as an undergraduate research assistant in the summer of 1947, after my junior year at Iowa State. During Spring quarter of that year two openings for undergraduates in the Atomic Research Institute were posted and announced in our Chemistry classes. I especially remember this being announced in my advanced inorganic lecture class by Prof. John Wilkinson, a very hard taskmaster indeed, but one who encouraged me to apply, probably because I was one of the few students in his class who could quickly recite all the known acids of sulfur and phosphorus! Although I was somewhat pessimistic about my chances, I did apply because I was increasingly tired of working at odd jobs in the dormitory dining room, the Botany Department, and grading papers in the History Department, and figured I had nothing to lose! (Although I had a tuition scholarship, I still had to help with the expenses of my room and board.) To my amazement, I was called to an interview with Dr. Don Martin, Jr., Professor of Inorganic and Nuclear Chemistry, who had been at Los Alamos during the war. (Rumor was that he had ingested some quantity of separated polonium, although I never asked him if this was true.) He asked if I would be willing to help make and test the Geiger counters that he was building to be used to assay samples for radioactivity. I was thrilled with the idea and recalled the biography of Marie Curie I had read in eighth grade and how fascinated I had been with her studies of radioactivity and her painstaking isolation of the new elements radium and polonium and her use of radium in medicine. When I was offered the position and found that as a full time employee in the summer I would, in addition to being able to do research in the Institute, earn $170 per month for something I would have been happy to do for free, I was doubly thrilled. (This also released me from a very dull summer job as a bank teller in my home town for only $85 per month, a salary I previously had thought was quite good as before that I had been a waitress in the local "best" restaurant in town at $7 per week!) I still did not know about the discoveries of neptunium and plutonium, the first transuranium elements, but another plus of working at the Institute was that they obtained a Q-clearance for me. After that, I became privy to some of the new information that was coming out. One of the odd quirks that I encountered was that on my classified notebook I had to put three initials and since I had no middle name I chose the letter "X" and told them it stood for Xanthasia, which seemed to satisfy the system. I think it was with this position at the Institute that I really started down the inevitable path that led me to become a "transuranium person." I continued to work at the Institute part-time during my senior year, splitting mica for windows for the Geiger counters, annealing the copper electrodes, etc., and making measurements of various radioactivities. I also learned micro ion exchange resin column separations for rare earth elements (a research interest of Spedding, who had developed very large-scale column separations for the rare earths), and reduction and separation of certain lanthanides with sodium amalgam. Knowledge of these techniques later became very useful to me in the transuranium field.

It soon became clear to me that I wanted to continue to do research in nuclear and radiochemistry, and Prof. Martin suggested that he would be happy to recommend me for graduate school at his alma mater, the California Institute of Technology, but going to California was too big a step for me to take at that time. Furthermore, by then a new 68-MeV synchrotron was being completed at Ames that opened many exciting research possibilities, and I elected to stay at Iowa State and continue to do research with Martin. During my first year in graduate school I met my husband-to-be, Marvin Hoffman, who had just come to Ames from the Cyclotron group at the University of Chicago, where he had been working for a year after returning from the Navy. He became a graduate student in nuclear physics of L. Jackson Laslett, then a Professor of Physics at Ames, who had been a student of E.O. Lawrence at Berkeley. Yes, indeed, it is a small world!

My father, although in accord with my decision to go to graduate school, suggested that I might want to get a teaching certificate in order to make sure I could get a job when I finished. This I absolutely refused to do, saying that the last thing in the world I ever wanted to do was teach!! Many times since I have thought how happy he would be to see that finally I recanted and eventually became a Professor of Chemistry at Berkeley, albeit via a most circuitous route. However, my decision to stay at Iowa State turned out to be a good one, as most unexpectedly my father died of a heart attack only two years later, at age 52. I was called at 4 a.m. by my mother, who told me the awful news and that the high school coach would drive to Ames that afternoon to pick up me and my brother, who was by then an undergraduate at Iowa State, and drive us home. I remember going in to ask if I could be excused from my classes and make up a midterm exam I was to have the next day in Quantum Chemistry when I returned. Instead, much to my dismay, my professor in Quantum Chemistry insisted on giving me the exam right then! Needless to say, I had a hard time concentrating on the test and hadn't really studied for it yet. I think he actually believed he was doing me a favor, and I managed to get a B in the test, but I never forgave him for his insensitivity. I went home to northeast Iowa to help my mother with funeral arrangements — she didn't drive, nor had she ever even written a check! My father's services were held in the high school gymnasium and some 1200 people attended. Although we were most appreciative of this indication of the community's respect for him, it was an ordeal I shall never forget as I strove to maintain my composure and never shed a tear. I later wrote a theme in a creative writing course vilifying all funeral services. I then became more or less responsible for my mother and my brother, who continued his studies as an undergraduate in Mathematics at Iowa State.

My mother and I had to clear out the house quickly, as it belonged to the school district and they needed it for the next superintendent, and so we kept what little we could and had an auction to get rid of the rest. She then came to live with me in Ames for awhile and soon after became house mother at one of the fraternities there. She was later nominated by her fraternity "boys" and named Ames "Mother of the Year." I continued my graduate studies (see Fig. 5) on photonuclear-induced Szilard–Chalmers reactions at the Synchrotron. My friendship with Marvin Hoffman was furthered by the fact that he was working at the Synchrotron and able to run it for me in the evenings so I could get irradiations almost anytime I needed them! I used a variety of complexes of cobalt and platinum which I synthesized to enable me to obtain very high specific activities and discover several new isotopes of cobalt, platinum, and iridium. For me, the discovery of new isotopes which nobody had ever seen before was an exhilarating experience, and to this day I still find the discovery of new isotopes — or, even better, new chemical elements — the most exciting part of nuclear chemistry research, although it is getting to be more and more difficult to do!

Fig. 5. Darleane Christian, Spring 1950 photo at Ames Laboratory, with remote apparatus for pipetting radioactive solutions.

I finished my Ph.D. in December 1951, in just over three years, and Marvin and I were married on December 26, 1951, in Waterloo, Iowa, the home of my grandmother Kuhlman. In January 1952, I left Marvin behind to finish his Ph.D. in Physics and I went to Oak Ridge to begin my new position there on the Aircraft Nuclear Propulsion Project, which involved uranium, but still not transuranium isotopes. I found out later that Prof. Laslett had told Marvin it was a terrible mistake for us to get married and that the marriage would never last under such unconventional circumstances! Some 20 years later, I had the pleasure of entertaining him in our home in Los Alamos, and again in Berkeley in the 1980s, which I think convinced him otherwise. (In spite of that erroneous prediction, we both thought he was one of the most intelligent men we ever met!) Marvin finished his Ph.D. near the end of 1952, and he decided to take a position in the Test Division at the Los Alamos Scientific Laboratory, where he had worked as a summer graduate student assistant in 1950. He was told that I would be offered a position in the Radiochemistry Group of the Test Division, so I quit my job at Oak Ridge and we went to Los Alamos after Christmas in 1952. Although I spent only a year at Oak Ridge, I made many close friends there. Once in Los Alamos, I immediately started calling the personnel department to ask about my job in the Radiochemistry Group of the Test Division and they told me, "There must be some misunderstanding, we don't hire women in that Division." Having never before run into such discrimination, I was totally taken aback and asked them to please try to circulate my application and find out where my job was supposed to be, but to no avail. Finally, in January 1953, Marvin and I went to a cocktail party for new hires and their spouses hosted by Director Norris Bradbury and I met Dr. Roderick Spence, group leader of the Radiochemistry Group. We talked and I told him my story and he said, "Where have you been — I've been looking for you. We need you for plutonium chemistry." Mike, the first thermonuclear test, had been fired in November 1952, in Eniwetok, and they were busy analyzing its unexpected and exciting results and badly needed more radiochemists, especially someone to devise new separation procedures for plutonium. So he hired me the next week, greatly relieved to know that I had a clearance and could start work immediately. Nuclear chemist Charles I. Browne, Ph.D., 1952, University of California, Berkeley, had joined the group as a military staff member in September 1952 and H. Louise Smith, M.S., 1952, University of Kansas, Lawrence, Kansas, had been in the group only about a week before the Mike test samples began to arrive! Rod Spence himself had been in the Pacific when the test was conducted and was there for the collection of air samples of the debris on filter papers held in special samplers attached to the wing tanks of airplanes which flew through the resulting cloud of debris. Little did we know that it would be mid-March 1953 before I would actually be allowed to get started. It seems my clearance was "lost" between Oak Ridge and Los Alamos. Since I originally got my clearance at Ames, it went back to the originating office rather than to the office which handled the Oak Ridge clearances. So they couldn't find it for three months — finally, after calls from everyone to Personnel, they asked the FBI to start a new clearance and they found it in the Chicago office in about three days. So that is how I missed being a discoverer of einsteinium (Z=99) and fermium (Z=100), which were identified in the debris from the Mike test while I was sitting in a small apartment in Los Alamos raging at the system. I will never again trust personnel offices, not just for saying "we don't hire women in that Division," which was untrue, but for their general insensitivity, incompetence, and bias — qualities which were not generally shared by the male scientists with whom I have worked!

Anyway, I finally did join the Radiochemistry Group at Los Alamos, on March 13, 1953. My first project was to find better and faster methods for the separation and analysis of plutonium in debris recovered from tests in the Pacific and later from above ground tests at the Nevada Test Site. Rod Spence was a wonderful mentor for me and taught me a great deal. I admired him greatly not only for his scientific ability but because he was one of the fairest and least egocentric individuals I have ever known. At last I had become a genuine "transuranium" person. The whole story of the discovery of elements 99 and 100 in the debris from the first thermonuclear device tested in November 1952, is given later (Chapter 6) in the book. The story of how I eventually became a "transplutonium" and even a "transactinide" person will unfold as the book progresses.

I should not fail to mention that during the days in Los Alamos Marvin and I produced two children. Our daughter, Maureane, was born on Easter Sunday, 1957, and our son, Daryl, was born on September 2, 1959. Los Alamos was a wonderful place to raise children. In both cases, I was able to continue my work until a day or two before each of my children was born and then quickly go back to work afterwards. I was privileged to have a wonderful woman to take care of them during the day. Then in 1964 my mother came to live near us after her mother passed away and she was instrumental in making it possible for me not only to pursue my career, but to travel as necessary. However, both Marvin and I spent as much time with our children as possible. A 1974 photo of one of our leisure activities is shown in Fig. 6. Although Marvin was not musical, he was a great listener and critic! Both of our children went to the Los Alamos schools and graduated from high school there.

Fig. 6. Darleane, Marvin, Daryl, and Maureane Hoffman around the piano in their home in Pajarito Acres, Los Alamos, NM, 1974.

Maureane received her B.S. from New Mexico State University in 1976 and an M.D. in pathology and a Ph.D. in Toxicology from the University of Iowa in 1981. She is now a tenured professor in the medical school at Duke University, in Durham, North Carolina. Daryl received his B.S. in 1981 from the University of California, Los Angeles, an M.D. from the University of New Mexico in 1984, completed a six-year residency in plastic surgery at Stanford University in 1990, and is now in private practice in the Palo Alto, California, area.

Two singular events which proved to be career-shaping should also be pointed out. The first is that in 1964 I was awarded an NSF Senior Postdoctoral Fellowship and Marvin was awarded a Fulbright Fellowship to Norway, so we took our two children and my mother and went to live in Oslo for a year. I found this a personally very liberating experience, as in Norway women were treated equally, but they were also expected to take their share of the responsibility as well. It was quite safe to go places by oneself and also not unusual for women to go out alone, while at that time in the U.S. women usually didn't dine out at restaurants alone or go on business trips with male colleagues. I performed research on short-lived fission products at the reactor at the Institute for Atomic Research at Kjeller, near Oslo, and learned new rapid separation techniques. We made many long-time friends there and were able to visit the farm at Havaas on Hardangerfjiord, where my grandmother Christian's parents came from, and the area in Gubrandsdalen, where my grandfather's parents came from. Our daughter attended second grade in the neighborhood school and became fluent in Norwegian. We returned to Los Alamos a year later — all feeling greatly enriched by the experience.

Much later, I was awarded a Guggenheim Fellowship for the year 1978–79 for the study of the mechanisms of nuclear fission, and I spent this sabbatical year with Glenn Seaborg's group at Berkeley and Marvin had a research position at SRI International in Menlo Park, California. I had already become well acquainted with Glenn Seaborg after my successful search for ^{244}Pu in nature in 1971, and my appointment in 1974 to the first IUPAC/IUPAP *ad hoc* committee to consider claims of priority of discovery of elements 104 and 105. I also worked closely with him (mostly by telephone, since I was still in Los Alamos) in the final honing and careful wording of the 1976 Science "Criteria" article [9-3]. During this time I had the opportunity to work with Al Ghiorso and Diana Lee, both so important to my future career. During this time also, Al Ghiorso designed and we built and tested the Merry-Go-Around (MGA) rotating wheel system (Fig. 7) which we used to study the spontaneous fission (SF) properties of the isotopes, fermium-246 and -248, which had half-lives of only a second and 36 seconds, respectively. The MG was later upgraded to study α-decay and used in a special "mother–daughter" stepping mode devised by Ken Gregorich for the 1993 seaborgium confirmation studies (Chapter 10). During the year 1978–79 I also became involved in some of the searches for superheavy elements (SHE) and attended the brown bag lunches of the "Super Heavy Element Isotope Kjemikers" (or SHEIKS) held in Seaborg's office every Wednesday noon. When I left Berkeley in July 1979 to return to Los Alamos to become Division Leader of the Chemistry–Nuclear Chemistry Division, the SHEIKS group had a wonderful party for

Fig. 7. Darleane Hoffman and Diana Lee with the MGA at the LBL 88-Inch Cyclotron, 1979.

Fig. 8. Darleane Hoffman cutting a cake at her going-away party, July 1979. Matti Nurmia is at the right.

me with a cake on which was written, "It was SHEer pleasure knowing you, Darleane". My picture with this lovely cake is shown in Fig. 8. I think that this pun was probably the brainchild of either Matti Nurmia or Al Ghiorso, who were our resident experts at devising clever acronyms, puns, etc.

I reluctantly cut my sabbatical year a few months short in order to return to Los Alamos in the late summer of 1979 to become the Division Leader of the Chemistry–Nuclear Chemistry Division — the first woman to fill such a position at LASL. This was a great challenge and honor for me and although I hated to leave Berkeley, I was eager to take up my new position. I had many ideas about things I wished to implement in chemistry and nuclear chemistry — not just in heavy elements, although certainly I had been seriously inoculated with that virus. When I could, I took time off from my administrative duties at Los Alamos to go to Berkeley (and even GSI) to participate in fission studies of the fermium isotopes and searches for SHE. It was a very busy time for me, but also very productive.

My Los Alamos colleagues and others nominated me for the 1983 ACS Award for Nuclear Chemistry "for her contributions to the understanding of the forces that govern nuclear behavior through studies of the fission process and of the production and characterization of heavy elements, both man-made and in nature." And, indeed, I was chosen for this award, the first woman selected for an ACS scientific award, other than the Garvan Medal, which is specifically designated for a woman chemist. (I was also very pleased to receive the Garvan Medal in 1991, but I was especially pleased that I was first honored by my colleagues for my ability as a nuclear chemist rather than because I was a "woman" chemist, prestigious though that award is.) As fate would have it, much to my pleasure, I learned that Glenn T. Seaborg was to present this award on March 21, 1983, in Seattle, Washington, during the National ACS meeting there. The picture of us taken on the occasion of this Banquet and Award ceremony is shown in Fig. 9. A color version of this hung in Glenn's office at LBL for several years until, much to my regret, he replaced me with a picture of himself and the movie star Ann-Margret!

Fig. 9. Glenn T. Seaborg presenting the ACS Award in Nuclear Chemistry to Darleane Hoffman, March 21, 1983, at the ACS National Meeting in Seattle, Washington.

Probably largely as a consequence of my 1978–79 sabbatical year at Berkeley and my subsequent close association with the heavy element group, in 1984 I was invited to return as a Professor in the Department of Chemistry of the University of California, Berkeley, 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 àsked 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 α-particle backscattering that I did under Burris Cunningham was deduced by careful integral counting of the activity emitted by uranium samples in a 2π α-counter. This effect was important, because it meant that the counting geometry for a weightless sample of plutonium in our 2π 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 α-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}Pu in the final sample. I soon decided that one possibility that offered hope for discrimination from the plutonium α-activity was a range measurement, almost the equivalent of measuring energy. Since we were pretty confident that the range of the α-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 2π 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 α-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}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 α-decaying series. The information enabled us to study in great detail the systematics of α-radioactivity under the leadership of Prof. Isadore Perlman. In this work we took advantage of α-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 α-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}N and ^{16}O bombardment of ^{238}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}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}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 μA of 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}Am or the ^{239}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}Pu in a high flux production-type reactor (the SRR) to produce ^{244}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}Cf (amounting to several hundred micrograms) that would be milked from the ^{249}Bk "cow" which had been purified and set aside for the purpose of growing ^{249}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 $^{263}106$. 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 α-particle activities by their α-energies and by the genetic relationships that they had to other α-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 α-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 α-particle was worth at least ten thousand fissions! However, we did much research in this area under the tireless leadership of Matti Nurmia 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,

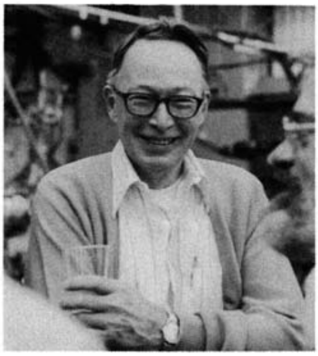

Fig. 3. Robert Main, in charge of the operation and development of the HILAC and the SuperHILAC. Coinventor of the Omnitron.

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.

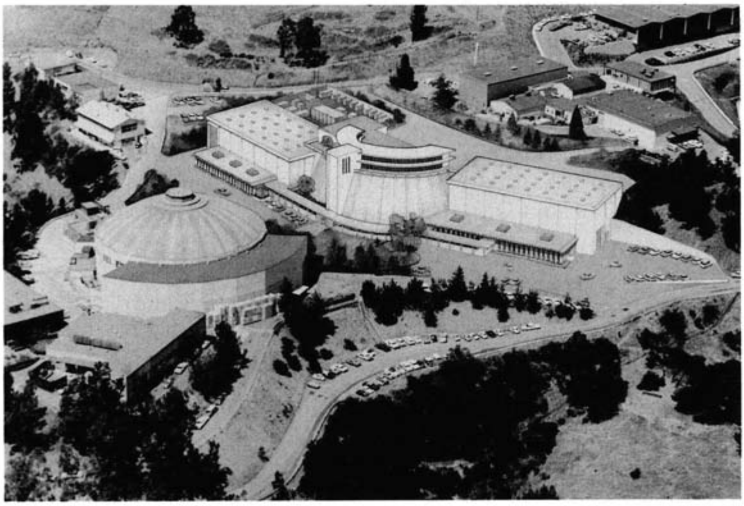

Fig. 4. Architect's drawing of the Omnitron model superimposed on the hill area near the 184-Inch Cyclotron.

Fig. 5. Ghiorso, approximately 1969.

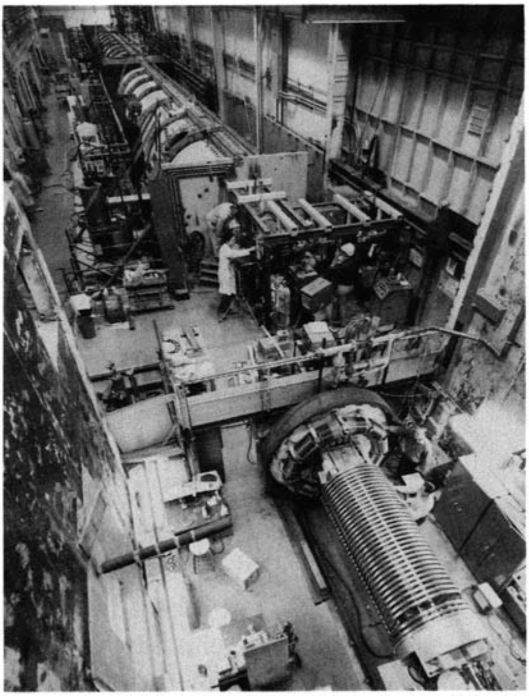

Fig. 6. The SuperHILAC in the final stages of construction, 1971. Some of the iron shielding is not yet in place. The ADAM injector is at the lower right.

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

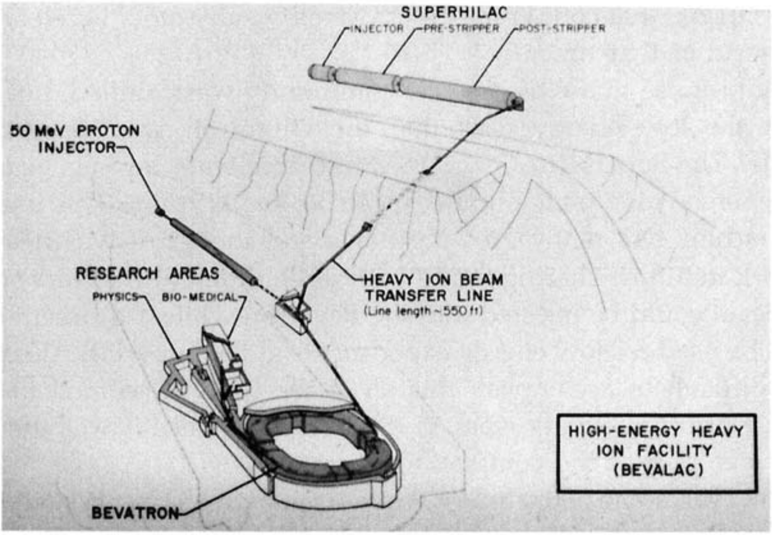

Fig. 7. Schematic diagram of the BevaLAC arrangement.

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}Cf with ^{18}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}Ho at the entrance to the steering magnet at the end of the accelerator and bombarded it with a beam of ^{40}Ar ions. An aluminum catcher foil was mounted downstream at the end of the magnet so that an α-radioautograph could monitor how much the 40 MeV α-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}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}Bi with ^{59}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}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 mother, Charlotta Wilhelmina Johnson (whose family name was changed to Farrell), came to Ishpeming in 1869 at the age of 19 from Örebro with her parents and brothers and sisters. His father, Johan Erik Sjöberg (whose name was anglicized to Seaborg), came to Ishpeming in 1867 from Hällefors at the age of 23. Johan's father, Erik Sjöberg, changed his name from Olsson in 1835. As I recall my father telling me, Johan crossed the Atlantic Ocean as a steerage passenger in a cargo ship. Johan had a friend at the Hällefors Iron Workers, the grandfather of the Swedish Nobel Prize winner, The Swedberg, so I suspect that the name The and my middle name Theodore have a common origin.

The Seaborg home in Ispheming at 639 East Division Street was occupied by members of the Seaborg family until 1914, then by members of the Kurin family until 1980, when I acquired it, and I still retain it for sentimental reasons.

My mother, Selma Olivia Eriksson (changed to Erickson), was born in Grängesberg in the southern Dalarna region of Sweden, and came to the United States (Ishpeming) in 1904, when she was 17 years old. She and my father met at a picnic on Swedish Midsummer's Day, June 24, 1908, and were married three years later on Swedish Midsummer's Day, June 24, 1911. I was born in Ishpeming on April 19, 1912 (during the presidency of William Howard Taft).

My mother's ancestors had lived in the southern Dalarna–northeastern Västmanland region of Sweden for many generations. A home in which her ancestors, Michael Hindersson and his wife, whose maiden name was Maria van Gent, lived in 1673 (as shown by an inscription on the living room wall) was moved in 1895 from Kopparberg to Skansen, where it stands with the name "Laxbrostugan" as part of the representative houses from "Bergslagen." This house has served as the rallying point for hundreds of my relatives, and has given me contact points to enable me to trace my ancestry back to the 14th century in Holland (Maria van Gent's birthplace). The Hindersson's daughter Britta married Nikolas Pemer, whose family came from Augsberg, Germany. The Pemer clan has formed a "Pemer Society" in Sweden in which Helen and I and our children are among the hundreds of members. Here my ancestry has been traced back to the 15th century.

Ishpeming had typical sections that were nearly all Swedish and it was in one of these that we lived. Since my father was fluent in Swedish and this was my mother's native tongue, the Swedish language was spoken in our home as it was throughout the community. I learned to speak and understand Swedish before I did English, but I am afraid that in the intervening years my facility with the language has declined. My younger sister Jeanette also spoke Swedish as her first language.

I was born in the second story rented home of my parents at 231 New York Street, in a section of town called the "Old Location," named after a nearby abandoned iron mine. (This old house is still there in 1998.) The congested "Tangletown" neighborhood, so called because of the mixed-up labyrinthine nature of the streets, contained the homes of our Swedish relatives and friends — the Swansons, Swanbergs, Hedstroms, Bjorks, Petersons, Greens, Quaals, Samuelsons, Olsons, and Dahls. When I was three and sister Jeanette one year old (Fig. 1), my parents purchased and moved into a two-

Fig. 1. Childhood picture of Glenn T. Seaborg with his sister Jeanette, ages four and two, Fall 1916.

story house about a quarter-mile north of the Old Location at 802 East Wabash Street, at the corner of 7th Street. (This old house is also still standing in 1998.) Ishpeming had severe winters with high levels of snow. I recall climbing out of my second-story bedroom window with my skis strapped on for an adventurous cruise on the top of the snow pack at near the house top level.

Here my main playmate was Clarence Larson, a classmate who lived diagonally across the street. Clarence had a younger brother Raymond who was about Jeanette's age and her (and my) close friend. My other playmates included Laurel "Dirt" Williams, Clarence "Cuckoo" Vinge, Ralph Haugland and his younger brothers "Winky" and "Coonjigger," Carl "Issy" Carlson and his sisters Anna, Esther, and Margaret (about my age and a classmate), Toive Dahl, and Eric Dahl. Most of us had nicknames. My nickname was "Lanky," an obvious appellation (a third-grade picture shows that the tallest of my classmates only came to my shoulders) (Fig. 2). On a visit to Ishpeming in 1994, I called on Dirt Williams and found him living in the same house that his family occupied when I lived there more than 70 years ago. I have kept in touch over the years with Clarence "Gom" Larson, who has lived in the neighborhood of Ishpeming throughout his life.

Fig. 2. Third Grade Class, High Street School, Ishpeming, Michigan, June 13, 1921, showing Glenn T. Seaborg as the tallest student in the back row.

Swedish customs of all kinds prevailed in our home. I remember particularly well the Swedish food that we enjoyed at our dinner on Julaften, or Christmas Eve. The fare usually included smorgasbord, which featured sil, or pickled herring. One of the mainstays was lutfisk, which was always served with boiled potatoes and a white sauce. Another feature was saffron buns and bread, usually served hot and made with glaceed fruits. This was part of a large spread of buns and cakes, including gingersnaps made in the form of goblins, piglets, stars, and other patterns. Another component which was almost always present was the Swedish lingonberries, which I still like so much. The meal was usually rounded off with risgryn, or rice pudding, which was topped with cinnamon with cream and sugar. Even in later years my mother carried on these traditions and my wife, Helen, has done her best to continue such activities for the benefit of our children.

Ishpeming during my first ten years was an isolated world of its own. I never saw, or even heard, the word "radio." I don't recall speaking into a telephone. We were served by a weekly newspaper, *The Ishpeming Iron Ore*. The dirt streets had a fringe of red color due to the iron ore "hematite," which topped underground deposits laced with miles of tunnels for the mining operations. We did have some access to nearby Marquette's daily *Mining Journal* and to the Sunday edition of the *Chicago Tribune*. I recall reading the Sunday comics featuring classics such as "The Katzenjammer Kids," "Slim Jim," and "Bringing up Father" (featuring the irrepressible Jiggs).

I started kindergarten in the High Street School in September 1917 and continued there through the first three grades. For the fourth grade I moved in 1921 to the Old Grammar School a couple of blocks west (corner of First Street and North Street). In the third, fourth, and fifth grades I was an ardent admirer of a girl named Dorice Gray. During the summers of 1920, 1921, and 1922, when I was eight, nine, and ten years old, I worked as a caddie at the Ishpeming–Negaunee nine-hole golf course (the Wawonowin Golf Club), where caddie rates were 20¢ for nine holes, paid by one category of player, and a 25¢ charge for the more affluent players. We sometimes maintained the Ishpeming or Negaunee gate (opening the gate for an incoming automobile). A small coin (a penny, nickel, or sometimes a dime) was thrown on the ground for us. Dorice's father was the manager of the golf club, and thus I was able to admire her from a distance during my summer visits to the Club House area.

Although I had become sufficiently fluent in English to cope by the time I started kindergarten in the fall of 1917, I was so shy as to cause problems. My mother had to negotiate a special arrangement with the teacher, Mary Earle, to allow me to go directly to the restroom during class time, without having to raise my hand and speak up to ask permission (an act that was beyond my capabilities).

The flu epidemic struck Ishpeming, as it did throughout the country, in the fall of 1918. My parents and many of my neighbors were afflicted and I recall that a number of our friends succumbed. I also remember vividly being surprised that I was so sick when my turn came. My sister Jeanette's flu turned into pneumonia, which gave us great cause for concern until her recovery.

In 1922, when I was ten years old, my family, which included my younger sister Jeanette, moved to Home Gardens, now a part of South Gate, California (near Los Angeles). At this time I changed the spelling of my name from "Glen" to "Glenn." This move was made largely at the urging of my mother, who wanted to extend the horizon for her children beyond the limited opportunities available in Ishpeming. However, unlike in Ishpeming, where he would have had guaranteed employment for life, my father never found permanent employment at his trade in California, and our family found itself in continuing poor circumstances. Since the new subdivision of Home Gardens had no schools, my sister and I during the first year traveled by bus to attend the Wilmington Avenue Grammar School in the Watts district of Los Angeles. I completed my grammar school education through the eighth grade in the newly constructed Victoria Avenue Grammar School in Home Gardens, skipping a couple of semesters on the way to my eighth-grade diploma.

When I entered David Starr Jordan High School in the Watts district of Los Angeles, again traveling to school by bus, I had to choose between a commercial and a college preparatory curriculum. My mother pressed for the commercial course; to her this was the road to a respectable white-collar job. But I started down a different road and chose the college preparatory program, with literature as my major subject. During my freshman and sophomore years, I studied the usual college preparatory subjects, such as English Literature, Oral English, and World History, as well as Algebra, Geometry, and a foreign language.

In my junior year I was required to take a laboratory science in order to be eligible for admission to the "tuition-free" University of California at Los Angeles (UCLA). Because my high school was small, Chemistry and Physics were offered in alternate years, and Chemistry was the offering in my junior year. It was fortunate for me that my first science course was taught by Dwight Logan Reid, an outstanding teacher who exerted a strong formative influence on me. Mr. Reid not only taught Chemistry, he preached it. He related some fascinating experiences he had had as a Chemistry student in college, and, when he lectured, his eyes would light up. His irrepressible enthusiasm, obvious love for the subject, and ability to inspire interest captured my imagination almost immediately. Early in his course I decided I wanted to become a scientist. As a senior I took Physics, also from Mr. Reid, and since then my interests in Physics and Chemistry have been inseparable.

Immediately after starting at Jordan High School, at the age of 13, I fell madly in love with Vivian Dawson, a slim brunette of greater than average height with flashing eyes that radiated intelligence, a fellow freshman, and a resident of Watts. We had adjoining seats in the Oral English class, which gave us a chance to communicate and get well acquainted. I believe she was also attracted to me. She shared her chocolate bars with me and was very friendly. I was too shy and inexperienced to take advantage of the opportunities to walk her home after school and after football games — stupidities that I have often, in retrospect, regretted. She left Jordan High School in the middle of the semester, when her family moved from Watts. I don't know where she went and I never saw her again. I have always hoped that we would meet again to bring each other up to date on our subsequent activities.

Early in 1927 I saw at a basketball game Bonita Edwards (an eighth-grader) and was struck by her vivacity and brunette beauty, and her resemblance to Vivian Dawson. Although shorter than Vivian, she also had sparkling eyes that advertised intelligence. (She graduated from high school at the age of 16.) She lived in Watts, across the street from Jordan High School. To my delight in the fall of 1927 as a freshman, she was a member of Charles Hicks' Latin I class, in which I was also enrolled as a junior. (Her attractive sister, Claire, a sophomore, was also a member of this class.) During the following two years of Latin class, I had an opportunity to get well acquainted with Bonnie and Claire. I became enamored with Bonnie but was too shy to advance my cause. I was ecstatic when, in the spring of 1929, she asked permission to wear my senior class ring. I was devastated when she returned to me my ring at the end of the semester, as she had, unfortunately, promised to do. Again, in retrospect, I have often ruminated on how clumsily I handled this situation. Bonnie married a football-playing friend of mine, Bud Coffin, and we have been lifelong friends.

After I finished high school in June 1929, I was very fortunate to find employment during the summer as a laboratory assistant, working as the lone control chemist on the graveyard shift (11 p.m. to 8 a.m.) at the Firestone Tire and Rubber Company in their South Gate plant. This provided the money that made it just possible for me to enroll at the University of California at Los Angeles (UCLA) in the fall. I knew I wanted to major in either Physics or Chemistry. I believed that a physicist could make a living only by teaching in a university, and at that time university faculties had few openings. On the other hand, a chemist unable to find a university teaching position could go into industry. So I chose Chemistry, hoping to become a university teacher, but, knowing that if I did not, other career opportunities would be available.

I lived at home and commuted by car with Jordan High School friends a distance of some 20 miles to UCLA. I have continued lifelong friendships with many of my classmates at Jordan High School in Watts. Stanley G. Thompson, who lived in Watts with his grandmother, was a sort of boisterous roughneck when I first encountered him in the ninth grade at the age of 13. He became a serious student when he reached the Chemistry class in his junior year and received the top grade in the second semester. We attended UCLA as Chemistry majors, traveling on occasion in his new Ford sedan, purchased for him by his "aunt" Bessie Brigance (who, I learned later, was actually Stanley's mother). We retained a close relationship before and during our wartime service on the Plutonium Project at the Metallurgical Laboratory at the University of Chicago, and then at the Lawrence Berkeley Laboratory (LBL) at the University of California (UC), until his death in 1976. (I had the pleasure of serving as best man at his marriage to the delightful Alice Smith, a San Diego girl, on Sunday, November 27, 1938, at Northbrae Church in Berkeley.) Stan was an extraordinarily able chemist with the best intuitive sense, *Chemisches gefuhl*, for solving chemical problems of anyone I have ever known. As I shall recount later, he solved the problem of chemical separation of plutonium on the Plutonium Project. We were collaborators on the synthesis and identification (i.e., the discovery) of a number of transuranium elements.

One sunny morning in September 1929, I walked across the ravine on the bridge which served as the entrance to UCLA from the Hilgard Avenue (east) side in Westwood. This was the opening year on this site of this young campus, only ten years old. Los Angeles State Normal School, on North Vermont Avenue in Los Angeles, became the Southern Branch of the University of California in 1919 and then the University of California at Los Angeles in 1927. There were some 5000 students and the total facilities at the opening of the Westwood campus in 1929 consisted of four buildings and a couple of temporary structures, including a student bookstore and gymnasium facilities. The four buildings, situated in a quadrangle, were the Chemistry Building (now Haines Hall, housing Geology), the Physics–Biology Building (now Kinsey Hall for Physics), the

Library Building (now Powell Library), and Josiah Royce Hall (named for the famous American philosopher and accommodating the other departments, including Mathematics) (Fig. 3). We found these not-quite-completed buildings rising starkly from the bare earth. Raw lumber and sacks of cement lay stacked for use. Lawns and shrubbery were nonexistent and the dusty walks turned to lanes of gooey mud when it rained.

Fig. 3. UCLA campus and environs showing Library, Royce Hall, Chemistry Building, and Physics Building, 1929.

I recall vividly the first meeting of my freshman chemistry class in the fall of 1929 with Prof. William Conger Morgan, a formidable man, some six-and-a-half feet tall. When Prof. Morgan strode into the auditorium of the Chemistry Building he glowered at the 300 students filling the room. He finally broke his silence to announce in a stentorian voice, "Look at the student on your right." After we had all done this, he commanded, "Look at the student on your left." After we had all done this, he bellowed, "One of you three will not be here at Thanksgiving time." I resolved to survive past his deadline.

In 1929, UCLA offered unique opportunities for undergraduates in Chemistry. In addition to Prof. William Conger Morgan, chairman of the Chemistry Department, Profs. William R. Crowell, G. Ross

Robertson, J. Blaine Ramsey, Hosmer W. Stone, Max S. Dunn, and a year or two later, William G. Young and Francis E. Blacet offered an extraordinary curriculum. The absence of graduate work in those years was probably the reason our able professors gave us a taste of graduate-type research by the time we had reached our sophomore or junior years.

At the end of the fall semester, in January 1931, I received a grade of 99% in the final examination for Prof. William R. Crowell's class in Quantitative Analysis (Chemistry 6A). This so impressed Prof. Crowell that he gave me the job to serve one afternoon a week as laboratory assistant in his Quantitative Analysis course for premed students (Chemistry 5). He also set me up in a job one afternoon a week in the stockroom for the freshman chemistry laboratory (Chemistry 2A and 2B for nonmajors), checking out to students chemicals, equipment, etc. These jobs, a total of six hours a week at the magnificent pay of 50 cents per hour, restored me to financial solvency. To add to my security, I received in May a letter from UCLA Recorder H.M. Showman, advising me that the UCLA Committee on Scholarships had granted me a $150 scholarship for the academic year 1931–1932.

One day in the summer of 1932, Stan Thompson and I paid a nostalgic visit to the Long Beach home of our high school chemistry teacher, Dwight Logan Reid. Here I met his attractive daughter Beth, about a year younger than I, with whom I was very much impressed. She had attended two years of junior college in Long Beach. I was delighted when I met her about a month later, when she came to register at UCLA as a junior with a major in Physical Education. Although much interested, I didn't have my first date with Beth until the following spring, when I escorted her to a party of my professional chemistry fraternity, Kappa Gamma Epsilon, which I was serving as president (and which I later led into membership as the Beta Gamma chapter of the national professional chemistry fraternity, Alpha Chi Sigma). Over the next year or so I had dozens of dates with her — escorting her to parties, playing tennis, accompanying me to football games at the Los Angeles

Coliseum, attending noontime assemblies in the auditorium of Royce Hall, etc. We attended a "pajamarino" celebration at a bonfire on the lower UCLA campus the night before the first-ever football game between UCLA and Cal-Berkeley. Beth and I were in the Los Angeles Coliseum the following afternoon to witness the historic 0–0 tie and I witnessed the ceremony just before the kickoff when UC President Robert Gordon Sproul, student body presidents Wakefield Taylor (Berkeley) and Porter Hendricks (UCLA), and graduate managers Bill Monahan (Berkeley) and Bill Ackerman (UCLA) met on the Coliseum turf. My dating of Beth ended when I left for Berkeley in the fall of 1934, but we have remained friends and seen each other on many occasions in the intervening years.

I have an especially vivid memory of the Friday, March 10, 1933, widespread earthquake centered in the Long Beach area. Stan Thompson and I were driving home from UCLA in his Ford along Slauson Avenue when the earthquake struck at 5:55 p.m. The sensation was so severe we had to stop driving. The Huntington Park High School was on fire when we drove past and we saw many demolished buildings as we approached home. When we arrived home in South Gate my mother and father and sister Jeanette were out in the front yard, having evacuated the house. I went in our house, found our dinner (pork chops) all over the floor in the kitchen, and furniture throughout the house overturned (i.e., the radio in the living room; the typewriter which normally sat on a table in my front bedroom had been thrown over onto the bed). I slept in my Chevrolet coupe that night, and my parents and sister slept in the family Star car. The next day I drove around to view the earthquake ruins. Compton and Long Beach were hit worst, with Watts, Huntington Park, Huntington Beach, and Santa Ana also hit hard. The morning newspaper estimated the death toll at 127, with 3000 injured and $30 million worth of property damage. Tremors continued throughout the day and the next day. However, when I returned to UCLA on Monday I found very little earthquake damage there.

While majoring in Chemistry, I took the maximum number of courses in Physics. In my senior year at UCLA I had a course in Modern Physics given by Prof. John Mead Adams (a lineal descendant of the second president of the United States), who talked to us of the exciting discoveries in nuclear science. And these lectures fixed my sights on this new frontier.

I stayed on a fifth year at UCLA, 1933–1934, taking a number of courses in Physics, which that year were started at the graduate (Master's degree) level. I hoped that graduate work would also be instituted in the Department of Chemistry. Just before the beginning of the fall semester I went to see Provost Ernest Carroll Moore to urge on him the initiation of graduate work in Chemistry, but he indicated that such a decision must be made at the level of President Robert Gordon Sproul. Therefore, I immediately visited Berkeley, brashly called on him (without an appointment) to press on him the initiation of graduate work in Chemistry in UCLA. His secretary, Miss Agnes Robb, let me go in to see Sproul. He treated me very well but was noncommittal on the graduate work questions. (On this visit I met chemistry professor Wendell Latimer, which may have helped in my admission later to graduate work at Berkeley.)

It soon became apparent that I should instead go on to graduate work at Berkeley. Ramsey urged me to go on to Berkeley, an additional incentive to that furnished by physics professor Adams in his course on Atomic Physics, in which he described the pioneering nuclear research underway at Berkeley.

For my graduate work there could be no place but the University of California at Berkeley. The very name, Berkeley, was magic; it was a distant and almost unattainable mecca. The chemistry staff at Berkeley was legendary, having written the textbooks from which we took our courses at UCLA. There were names such as Joel H. Hildebrand, Wendell M. Latimer, William C. Bray, C. Walter Porter, Gerald E.K. Branch, and, of course, the great Gilbert Newton Lewis, dean of the College of Chemistry. I had become acquainted with his 1923 book *Valence and Structure of Atoms and Molecules,* and was fascinated by it. I wanted to meet and become acquainted with this remarkable man. The name of the rising young nuclear physicist Ernest O. Lawrence was beginning to ring through the world of science. I wanted to work as near as possible to Lewis (the great "G.N." — "The Chief") and to Lawrence. And again the absence of a tuition fee was consistent with the state of my finances.

Reaching this mecca was not necessarily simple. Not everyone was admitted, and so the custom was to apply to a number of graduate schools. Moreover, I had not only to be accepted but to be granted a teaching assistantship (at a salary of $50 per month) to support me through graduate study. UCLA chemistry professor James B. Ramsey, who had done his graduate work at Berkeley, assured me that there was no need to apply to alternative institutions, that Berkeley would grant me both my wishes. And so it did, to a lingering disbelief on my part, despite Prof. Ramsey's reassurances.

It is difficult to describe the exciting, glamorous atmosphere that existed at the University of California at Berkeley when I entered as a graduate student in August 1934. I took formal courses in Chemistry from such eminent men as Profs. Axel R. Olson and William F. Giauque, and in Physics from Raymond T. Birge and Robert B. Brode. As a teaching assistant in freshman chemistry my instructor colleagues in the laboratory sections included such men as Joel H. Hildebrand (who always gave the main lectures as well), Wendell M. Latimer, William C. Bray, Giauque and Ermon D. Eastman. Probably the high point of each week was the Tuesday afternoon Research Conference held in Gilman Hall, at which graduate students presented a research paper on a current topic from the literature, which was followed by a faculty member, postdoctoral scientist, or advanced graduate student describing his own recent research. The latter was always in the forefront of scientific research in an interesting area. Here we saw G.N. at his best, sitting at the head of the table which dominated the center of the room, chain-smoking his huge black cigars. He asked questions and stimulated discussion over the whole wide range of Chemistry and Physics in a manner which I have never seen equaled.

Another high point was the weekly evening Nuclear Seminar, covering recent articles from the scientific literature and the current work in the College of Chemistry in the area of Nuclear Science; this seminar was run by Willard F. Libby and Robert D. Fowler, who guided my research until he left. G.N. also always attended these seminars, which added considerably to the excitement. In Le Conte Hall on Monday evenings, there was the Physics Journal Club, presided over by Lawrence, including the brilliant galaxy of J. Robert Oppenheimer, Edwin M. McMillan, Luis W. Alvarez, Philip H. Abelson, Martin D. Kamen, and John J. Livingood, just to mention a few. It was in this atmosphere that I was privileged to carry out my doctoral research in the company of such fellow students as David C. Grahame (who worked with me as my research partner), Kenneth S. Pitzer, Samuel Ruben, and many others.

I made a good start toward realizing my ambition to become a nuclear scientist when I completed my graduate thesis on a nuclear physics project, regarding the inelastic scattering of fast neutrons. After starting this project with Fowler, who moved to Johns Hopkins University, I completed the work with Chemistry Professor George Ernest Gibson. Grahame and I carried out this research in the cavernous auditorium of the abandoned East Hall, an ancient building which had been moved from its original site at the present location of Le Conte Hall to a then vacant spot just to the south of Faculty Glade at about the present location of Morrison Hall. We were forced to perform our experiments during the graveyard shift, because Lewis required the use of the Chemistry Department's sole radium–beryllium source of neutrons during the daytime and evening hours. Our experiments provided what was probably the first unequivocal evidence for the phenomenon of inelastic scattering of fast neutrons. We established a minimum probability (cross section) for this type of reaction in the region of lead and bismuth, an observation that was beyond theoretical understanding at that time but was explained years later as due to the closed nucleon shells of 82 protons and 126 neutrons.

When I obtained my Ph.D. degree in May 1937, I stayed on, continuing my research even though I had no immediate prospect of a job. This was a Depression year and satisfactory positions were very difficult to obtain. Yet such was the atmosphere at Berkeley and my preoccupation with my research that I was only vaguely worried about my future. Then one day in the middle of the summer, G.N. called me in and asked whether I would like to serve as his personal research assistant. Because of his reputation and standing, he was almost unique in having such an assistant, and the position at $1,800 per year happened to be open at that time. I was overwhelmed at this opportunity and immediately accepted, after first expressing some genuine doubts as to my adequacy. In this role, I published several papers with "The Chief" in the area of generalized acids and bases, which was his current interest and rather far from my own area and aptitude.

Some time before I began my work with G.N. I entered almost by accident the mainstream of my career as a nuclear scientist. One day in 1936 I was suddenly confronted by Jack Livingood, a physicist who was favored by ready access to that nuclear horn of plenty, the 27-inch cyclotron. He literally handed me a "hot" target, just bombarded by the machine, and asked me to process it chemically to identify the radioisotopes that had been produced. Naturally, I jumped at the chance. The facility he offered in Le Conte Hall was hardly luxurious. My best recollection is that it was the custodian's closet and that the resources consisted of tap water, a sink, and a small workbench. With some essential materials bootlegged from the Department of Chemistry, I performed the chemical separation to Jack's satisfaction. In the course of my collaboration with Livingood, covering a period of five years, we discovered a number of radioisotopes which proved useful for biological explorations and medical applications. Among the isotopes that we discovered were iodine-131 and iron-59, and among the useful isotopes that we characterized was cobalt-60.

The discovery of iodine-131 has given me special satisfaction. On one occasion during this period, in 1938, the late Dr. Joseph G.

Hamilton, one of the outstanding nuclear medical pioneers, mentioned to me the limitations on his studies of thyroid metabolism imposed by the short lifetime of the radioactive iodine tracer that was available. He was working with iodine-128, which has a half-life of only 25 minutes. When he inquired about the possibility of finding another iodine isotope with a longer half-life, I asked him what value would be best for his work. He replied, "Oh, about a week." Soon after that, using the 37-inch cyclotron, Jack Livingood and I synthesized and identified iodine-131, with a half-life, luckily enough, of eight days. This isotope is widely used for the diagnosis and treatment of thyroid disease and the diagnosis of other disorders. I have the added satisfaction that my own mother had her life extended by many years as a result of treatment with iodine-131.

Also in 1938, in a collaboration with Emilio Segrè, I was a discoverer of technetium-99m, which has become the most widely used radioisotope for diagnosis in nuclear medicine.

My experience as a radioisotope hunter led eventually to the transuranium elements, a nuclear field that was to become my lifework. My interest in the subject had been aroused soon after I arrived at Berkeley. In the fall of 1934, at the evening Nuclear Seminar presided over by Libby and Fowler, we learned of the experiments by Fermi and his group in Italy. They reported that they had bombarded uranium with neutrons and produced what they thought were radioactive isotopes of transuranium elements, i.e., elements in the periodic table that were heavier than and beyond the heaviest natural element, uranium. Somewhat later, this work was taken up in Germany by Otto Hahn, Lise Meitner, and Fritz Strassmann. I read avidly all the reports on these so-called transuranium elements. I even chose this as my topic for the Tuesday Research Conference, using one of the papers by Hahn and his associates as the basis for a complete description of the chemical properties of these transuranium elements, a nonsubject on which I considered myself already a minor expert.

Then, at the Journal Club meeting in the Department of Physics on a Monday night in January 1939, my mastery of the "field" vanished in a moment. The information had come through by word of mouth that Hahn and Strassmann in Germany had identified some of the radioactivities as isotopes of barium and lanthanum, and that what actually happened upon the bombardment of uranium with neutrons was the splitting of the uranium nucleus into two approximately equal-sized fragments, with the release of a large amount of nuclear energy. Nuclear scientists had been looking at fission products, not transuranium elements.

I cannot possibly describe either the excitement that this produced in me or the chagrin I felt in realizing that I had failed to interpret correctly the wealth of information I had studied so assiduously for a number of years. After the seminar was over I walked the streets of Berkeley for hours, in turn exhilarated by the beauty of the discovery, despairing over my lack of insight and intrigued by the import of this exciting new fission reaction.

Fig. 4. Helen L. Seaborg and Glenn T. Seaborg, Christmas 1941 in San Francisco.

I have often said that my greatest discovery was Helen Griggs, the girl I married. I first met Helen in September 1938, when she was serving as Ernest Lawrence's secretary. I found her very winsome and felt that I wanted to get to know her better. She is five years younger than I. However, although I saw her many times during the intervening three years, I didn't succeed in dating her until the fall of 1941. I had competition from Donald Cooksey (assistant director of the Radiation Laboratory), who was dating her on a regular basis. Helen and I continued dating during the fall of 1941, and it was clear by Christmas time that I was madly in love with her (Fig. 4). Finally here was a girl that I was dating exclusively. For the first time since I came to Berkeley in 1934, I did not go home to my parents for Christmas, but had Christmas dinner with Helen in San Francisco.

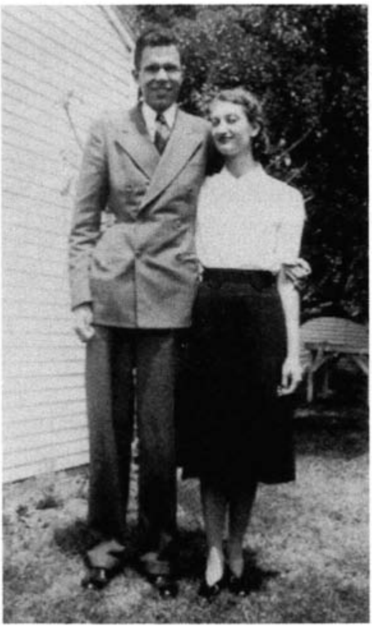

Fig. 5. Helen L. Seaborg and Glenn T. Seaborg at Seaborg's parents' home, South Gate, June 5, 1942, the day before they were married.

When the decision was made that I should move to Chicago for work on the Plutonium Project, I immediately proposed to Helen, on March 23, 1942, and she accepted. The understanding was that I would make a visit back to Berkeley soon to join her for the wedding. After our reunion in Berkeley we visited my parents in South Gate, then boarded a train headed for Chicago, planning to get married en route (Fig. 5). Later Helen wrote the following account of our misadventures in getting to "the altar":

> "We had quite a time getting married but it was also very amusing; in fact, we began to feel like a movie scenario before we got through. We got off the train in Caliente, Nevada, on Saturday morning, June 6, about ten o'clock, with a great deal of confidence, without a care in the world, and a feeling that we merely had to take care of a few details and we would then have been quietly married. Little did we realize what was in store for us! We first decided to check our bags, but Caliente had no checkroom. The telegraph operator finally told Glenn that he could leave our things in his place. We deposited our junk and proceeded out to look over the town and find the place to get a marriage license. Being a little coy, we strolled around looking for the place instead of boldly asking anyone. Since we couldn't locate the city hall or anything that remotely resembled same, we finally went in to the town telephone and asked the woman who operated it. She acted as though she had never heard of such a thing as a 'city hall.' We then asked where one could get a marriage license, to which she replied, 'Why, from Evans Edwards' in tones that clearly indicated she thought us terribly stupid not to know that. Glenn asked her where Ev was, and she said, 'Why, down around the corner next to the drug store.' Here her tones indicated that it was the same place it had always been, what was the matter with us anyway. So we proceeded to Ev's to find him leaning back precariously in his swivel chair looking into space. We told him we wanted to get a marriage license. From Ev we learned that one could not

obtain a marriage license in Caliente and that the closest place to get one was Pioche, which was some 25 miles north. Ev was most uncommunicative and didn't seem to have anything to offer in the way of a suggestion for us to get over there. At this point a woman came out. Like most women, she had romance in her soul, and she told us we could probably get a ride in the mail truck, told us where to find it, and said it had not yet left. We thanked her gratefully and left.

"Glenn went to find the driver of the truck while I went to buy a pair of tennis shoes. The town was nothing but dust, and, of course, I had nothing but toeless shoes. When I came back I found him talking to a young man. The latter, it turned out, was a deputy sheriff and son of the telegraph operator, in whose place we had left our bags. Apparently the local inhabitants were somewhat suspicious of us and had asked the young fellow to check up on us. So he had come up to Glenn and asked, 'Are you a teacher?' Poor Glenn, this was a horrible shock; he thought, 'Surely it doesn't stick out all over me already,' but he admitted he was. This was followed with, 'Do you teach Chemistry?' 'Yes.' After this cautious approach the fellow admitted that he had just graduated from Cal majoring in Chemistry [*Glenn learned much later that the deputy's name was Frank White Anders*], that Caliente was his home, and that he was there for the summer before taking a job in research laboratories in Washington this fall. Then he was very nice and offered to help us in any way he could; so he and Glenn went over to the town telephone to call Pioche to make sure we could get in the county courthouse if we got there after noon (since it was Saturday). A voice at the other end of the wire said of course someone would be there — didn't he know the county commissioners were meeting that afternoon? Then we piled into the mail truck and were off, via Panaca, for Pioche. We arrived about 12:40 p.m. to find the assistant county clerk waiting for us. She told us that she would have been gone if Glenn hadn't called; so we felt very lucky. She made out our license and was more nervous than we were. By that time we were very calm anyway — prepared for whatever might come. I am sure we were beyond the point of surprise. Then she asked us if we wanted her to call the judge, and we asked her about a minister. It turned out there was only one in town; so she tried to locate him for us, but he was nowhere to be found. We decided we had better not take further chances and asked her to get the judge for us then. Well, nobody knew where the judge was; so she told us to get in her car and she would drive us to town to find the minister or the judge. (The courthouse was about a mile from the town itself.) When we got there, she told us to go have lunch while she hunted. We invited her to have lunch with us, but she insisted on our going ahead and that she would hunt. She told us which was the better restaurant in town and went off. When we were half through lunch, she came in to tell us that she had located the judge and to come to her car when we were finished eating. A few minutes later the judge [*Glenn learned later that his name was Edgar L. Nores*] came in and told us they were right across the street waiting for us. I shall never forgive Glenn (as I tell him as frequently as I have an opportunity) for practically telling the judge that we would be there as soon as he finished his apple pie, and he sat there and calmly finished. The rest of the day was comparatively calm after that. The judge was just like the country judge one reads about in books. He was very proud of his country and told us all about the mining there. He was rather cute. When he gave me the marriage license, he told Glenn to keep his hands off — that it was mine — and told me not to let him have it. We got quite a kick out of it. Then we both forgot to pay him — after our having some discussion as to what one should give him.

"Glenn and I were both convinced that had we not the good fortune to run into the two young women with romantic souls we would never have succeeded in getting married. The assistant clerk was a roly-poly good-natured person who told us that Pioche was not her hometown — Panaca was. (Panaca was 13 miles south.) It turned out she was married to a sailor and had been going where he did whenever she could, but he went to Ireland last summer, and she had felt lonely and had come back to Pioche to work. She had been married eight years and apparently thought it was a very good idea.

"We had to wait until 4:30 for the mail truck to take us back to Caliente; so we took a walk and proceeded up a hill and saw some beautiful cedars and got some sun. We then decided that the city folk may make suckers of the country people who come to town but that this is nothing to what the country folk do to the city guys. We met a man with a bunch of pictures of the Pioche High School band, and it seemed the band had just won some kind of an honor and that they would be able to do something else wonderful if only we bought one of the pictures. He told us that this was the only honor that had ever come to Pioche and put up a wonderful sales talk — what could we do but purchase a picture of the Pioche High School band. On the way back to town we met a very palsy-walsy fellow (slightly tipsy), but he wanted us to stop and have a cigarette with him. We gravely thanked him, told him we were very sorry we didn't smoke. This left him feeling very sad, and he assured us he wasn't mad and that we didn't have to have a cigarette with him. We loafed around and drank cokes until the mail truck started back, and we arrived in Caliente about 5:30. So we say it took us all day to get married. Confidentially, I was beginning to wonder if we were really going to get married, and Glenn doesn't deny that he conjured up these obstacles himself in the hope that he would get out of it at the last moment. The funny part is that we got married in Nevada to save time."

Helen and I visited Caliente and Pioche again at the time of our 50th wedding anniversary, June 1992. We found some changes, but the Caliente railroad station building was still there, changed to a museum and city offices, and the Lincoln County Courthouse in Pioche was still there, even the room where Justice of the Peace Edgar L. Nores performed our marriage ceremony. Helen and I have succeeded in locating Frank White Anders so we could tender him our belated thanks for his crucial help on June 6, 1942.

My entry into the transuranium field is described in the introductory passages of Chapter 1.

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Glossary

Acronyms

ACS	American Chemical Society
ADAM	3 MeV injector for SuperHILAC
ADC	Analog-to-Digital Converter
AEC	Atomic Energy Commission
AFOAT	Armed Forces Office of Atomic Energy
ARCA	Automated Rapid Chemistry Apparatus
BART	Bay Area Rapid Transit
BevaLAC	Combination of Bevatron and SuperHILAC working in tandem, LBL
BGS	Berkeley Gas-filled Separator
CERN	Centre Européenne pour la Recherche Nucléaire, Geneva, Switzerland
CNIC	Committee on the Nomenclature of Inorganic Chemistry
Dees	Accelerating electrodes inside the cyclotron shaped in the letter "D"
DESY	Deutsche Elektronen Synchrotron (German Electron Synchrotron)
DQD	Dipole–Quadrupole–Dipole
DSDV	Dirac–Slater Discrete Variational
ECR	Electron Cyclotron Resonance ion source
ETR	Engineering Test Reactor
FAKE	Fast Automatic Khemistry Experiment
GSI	Gesellschaft für Schwerionenforschung mbH
HADES	Heavy Atom Detection Equipment Studio
HDEHP	di-2-ethylhexylorthophosphoric acid

HEVI	Heavy Element Volatility Instrument
HFIR	High Flux Isotope Reactor
HILAC	Heavy Ion Linear Accelerator
IDCNS	Interdivisional Committee on Nomenclature and Symbols
IUPAC	International Union of Pure and Applied Chemistry
IUPAP	International Union of Pure and Applied Physics
JINR	Joint Institutes for Nuclear Research at Dubna, USSR
KAH	Kernphysikalische Arbeitsgemeinschaft Hessen
LASL	Los Alamos Scientific Laboratory
LANL	Los Alamos National Laboratory (effective 1981)
LBL	Lawrence Berkeley Laboratory
LBNL	Lawrence Berkeley National Laboratory (effective June 16, 1995)
LEAP	Large Einsteinium Accelerator Program
linac	linear accelerator
LN	liquid nitrogen
MGA	Merry-Go-Around, rotating wheel system at LBNL
MIBK	Methylisobutyl ketone
MIT	Massachusetts Institute of Technology
MTR	Materials Testing Reactor
NAO	National Adhering Organizations
NSD	Nuclear Science Division, Lawrence Berkeley National Laboratory
OLGA	On-Line Gas Chemistry Apparatus
ORNL	Oak Ridge National Laboratory
PAC	Pure and Applied Chemistry
RFQ	Radio Frequency Quadrupole
RHIC	Relativistic Heavy Ion Collider
SASSY	Small Angle Separator System
SHE	Super-Heavy Elements
SHIP	Separator for Heavy Ion Reaction Products
SIS	Storage ring Ion Source at GSI
SLAC	Stanford Linear Accelerator Center
SRP	Savannah River Project
SuperHILAC	Improved HILAC (1971–1993)

TBP	Tributylphosphate
TIOA	Triisooctylamine
TOF	Time-of-Flight
TRU	TRansUranium Processing Facility at ORNL
TTA	Thenoyltrifluoracetone
TWG	Transfermium Working Group
UNILAC	UNIversal Linear Accelerator
VW	Vertical Wheel
Z	Atomic number = number of protons
Z>89	Transactinium Elements
Z>103	Transactinide Elements

Decay Modes

α	alpha
β	beta
ec	electron capture
SF	Spontaneous Fission

Units

s = second
min = minute
h = hour
d = day

A = ampere
b = barn = 10^{-24} cm^2
eV = electron volts
g = gram
m = meter

Prefixes

M = mega = 10^6
k = kilo = 10^3
m = milli = 10^{-3}
micro = m = 10^{-6}
nano = n = 10^{-9}
pico = p = 10^{-12}