Life With Seaborg

When Glenn Seaborg visited Kansas University in the Spring of 1957, it was a fruitful period in the quest to discover new synthetic elements. Elements 99-101 (einsteinium, fermium and mendelevium) had just been identified, subsequent to the discovery of elements 93-98 in the previous decade. Since Nobel Laureates were a rare attraction in Lawrence and the subject had gathered national interest, his distinguished lecture was met with considerable enthusiasm. For me, as a graduating senior who was trying to decide on graduate school, the lecture made a strong positive impression.

Among my graduate school offers, Harvard was high on the list, primarily to study inorganic chemistry under Eugene Rochow. However, when I had a chance to visit Cambridge to interview for a Danforth Fellowship (a futile effort, given my rare appearances in church), Rochow was out-of-town. Further, it was a cold, foggy, day in the Boston area. To top it off, when I went into a drug store and ordered a milkshake, they served a runny concoction that was highly deficient compared to a hearty Midwest milkshake. So Harvard was crossed off the list as too uncivilized. Wisconsin and Willard Libby were also under consideration, but Libby didn't inspire much enthusiasm among my faculty advisers. So given the northern cold, Wisconsin faded to the bottom of the list. That left UC Berkeley and Cal Tech.

After the lecture, some of my chemistry profs invited me to join them as they drove Seaborg back to the Kansas City airport. The initial part of the trip was decidedly awkward, as the faculty members attempted to discuss science. This was my first introduction to Seaborg's reluctance (at least in my presence) to discuss science -- unless it involved new elements or the periodic table. During one those frequent pregnant-pause intervals, I decided to interject and bring up the basketball game between KU and California during the previous season. Kansas, which wound up second in the NCAA tournament that year with Wilt Chamberlain at center, narrowly escaped an upset with a 55-54 win. The game was particularly notable in that Cal's 6'5" center Don McIntosh neutralized the 7'1" Chamberlain. Clearly, I had punched the right button with Seaborg. Thereafter followed a lively conversation about sports in general and especially golf, in which we shared a mutual interest at the time. On the return trip to Lawrence my faculty mentors remarked that this was the first time Seaborg had opened up during the entire visit.

The upshot of Seaborg's visit was that Cal became my future home for graduate studies. Not a very profound basis for making such a decision, but among the four alternatives, probably the best fit for me. After classes began in the Fall, the next step was to interview potential faculty advisers. The choices were a bit overwhelming -- everything looked exciting. When I came to see Seaborg, he was very direct. "I'd like to have you in our group", which was an offer that couldn't be refused by a kid from Kansas whose degree of sophistication was off the charts -- in the wrong direction. So on the basis of such deliberative thinking, I joined Seaborg's cadre of graduates students.

It didn't take long to discover the extent of Seaborg's empire. Students (there were four of us in my entering class) were parceled out among several staff members or postdoctoral fellows. Darrah Thomas, an Instructor at UC with a joint appointment at the Berkeley Radiation Laboratory (or Radlab, now Lawrence Berkeley National Laboratory) and currently

Distinguished Professor Emeritus at Oregon State University, inherited the unenviable task of trying to make a scientist out of me.

During the first semester of graduate studies, Seaborg and I had few interactions. In the second semester Seaborg taught the graduate nuclear chemistry course, so more opportunities for one-on-one conversations arose. However, since only a few months had passed in which to accomplish much in the laboratory, the major topic of discussion would usually be Cal sports. As for the course itself, its most memorable aspect was the final exam. Seaborg was carrying a heavy administrative load at the time -- a fact that I learned to deal with later -- so he assigned one of his senior students, Glen Gordon, to write and grade the final exam. Single-n Glen assumed that not only the lecture material was fair game, but also all the literature relevant to the field of nuclear chemistry. To put it mildly, the exam was a disaster. The entire class believed we had collectively flunked and would soon be looking for the quickest avenue of transport back to our respective points of origin.

As it turned out, Seaborg's immediate preoccupation was in his capacity as Cal's Faculty Representative to the Pacific Coast Athletic Conference. On the day grades were to be assigned, the Conference voted to expel the University of Southern California for illegal athletic practices (so what else is new?). Glenn was ecstatic and as a consequence, he assigned everyone in the class an A. The only time I ever saw him happier was at the San Diego American Chemical Society Meeting when he learned that element 106 had been named after him. So we unpacked our bags and went back to work. The episode signaled Seaborg's rising status among the administrative ranks, which led to his appointment as Chancellor of the Berkeley campus shortly thereafter.

Darrah Thomas' interests were in the area of nuclear reaction-mechanism studies, not the new-element-synthesis program, which proved fortunate for me. Berkeley's new heavy-ion accelerator, the HILAC, provided a unique opportunity for nuclear fission investigations and we embarked upon a program to study the angular distributions of heavy-ion-induced fission reactions. The HILAC accelerated unique beams of ions as heavy as neon to energies well above the Coulomb repulsion barrier of any target element. It was the forerunner of many such machines that would follow. While new element production dominated the HILAC program, Seaborg's right-hand man, Albert Ghiorso, couldn't utilize the accelerator 24/7 (although on occasion he tried), so in the leftover time it was possible to be among the first graduate students to conduct studies of heavy-ion-induced nuclear-fission reactions.

However, it seemed to me that from Glenn Seaborg's point of view, heavy-ion reaction studies were primarily an avenue for completing a credible thesis. His focus was on heavy-element synthesis. Early on I was assigned a project called "The Masses", which I considered to be a secondary project. The work involved analyzing the systematic behavior of heavy-element alpha- and beta-decay energetics in order to predict the nuclear masses of unknown heavy elements and isotopes. It seemed to be a mundane project and it was hard to develop much enthusiasm for the work. As I was later to learn, Glenn Seaborg did not view it that way.

During the summer of 1958, Seaborg would occasionally drop by my office. His first question was usually, " How are 'The Masses' coming?" Having given little thought to the project, my usual bailout position was that I was trying to computerize the calculations. This was a stretch because the only accessible computer was an IBM rotating-drum machine with a memory capacity of 2000. With several hundred pieces of input data and the need to apply fitting routines, even small components of the project could not be handled without some imaginative programming -- which was beyond my skills. Later, when a more advanced computer became available, I was too wrapped up in nuclear reaction studies to divert my attention to the mass-calculation program. .

Thus, most conversations with Seaborg quickly evolved to Cal sports and our mutual interest in golf. On one occasion he invited me to play nine holes with him at his Lafayette club. As an added attraction, Lafayette was east of the Berkeley Hills, and hence much warmer than the often summertime cold fog in Berkeley. Whenever I longed for the hot Kansas summers, an eastward trip through the Caldecott Tunnel would provide instant relief.

These were the good old days for golf, a time when those abominable carts were used only by the infirm, so we were touring the course on foot -- the way golf should be played. After eight holes we were tied. As we were about to tee off for the ninth hole, he remarked that he must have left one of his irons back on the seventh green. As an accommodating graduate student and gym rat, I volunteered to run back and fetch it. So I ran the quarter of a mile to the seventh green, but no seven iron. The golfers on the seventh green said that they thought they saw it on the sixth green, so I set off on another quarter-mile journey and rescued the wayward golf club. Returning to the ninth green after running a mile, I butchered my tee shot and wound up losing the match by one stroke. I've often wondered if Glenn had been reading Stephen Potter's treatise on Gamesmanship and was applying the lessons garnered from Potter's perverse insights.

During the Fall semester of 1958, Darrah Thomas moved to Brookhaven National Laboratory although he maintained regular contact with me throughout my graduate career. In addition, at this point interactions with Seaborg became more infrequent as he assumed his duties as Chancellor of the Berkeley campus, splitting his base of operations between the Rad Lab in the Berkeley Hills and the campus below. Over the next two years, fission-fragment angular distributions from gold and bismuth targets were measured with a differential-range recoil technique for every available beam and energy available at the HILAC. I would submit regular research reports on the status of my nuclear reaction studies. Some progress was also appended relevant to "The Masses", mostly in the way of literature research, but little on predicting the properties of unknown elements and isotopes. When we met, the conversation would inevitably turn to "How are 'The Masses' coming"? There was rarely much discussion of my reaction studies, so I would deflect the conversation to subjects such as to Cal's 1959 Rose Bowl team and NCAA basketball championship in 1959 and runnerup finish the following year.

While I sometimes felt concerned about the lack of direct contact with my research adviser, and how it might impact completion of my thesis, this lack was more than compensated by the freedom to chart my own course and access to the superb technical support available to those in his group -- excellent drafting and machine-shop personnel and forefront electronics developments, advantages few graduate students could claim in those days. Regularly-scheduled beam time on the HILAC accelerator allowed me to pursue my experiments systematically. In addition, senior staff member Earl Hyde and new postdoctoral fellow John Alexander stepped in to provide invaluable guidance. And secretarial support was readily available, a big help in those pre-computer days. So Seaborg's hands-off approach had many compensations.

In the summer of 1959 research at the HILAC ground to a halt due to a major radiation accident that shut the facility down for six months. Albert Ghiorso had failed to install fail-safe safety precautions on a highly radioactive ^{244}Cm (element 96) target, which had the audacity to rupture and produce a veneer of radioactivity throughout the lab. Since I was working in the adjacent experimental area at the time, Ghiorso and I received the largest radiation doses. The local health chemists seized on this unique opportunity for the study of the radiobiological effects of Curium. So for the next six months my graduate-student status relegated me be the subject of daily measurements, while Ghiorso's rank exempted him from such intrusions on his daily excretory routine. The accident was also a major setback for completion of my thesis research on the schedule I had envisioned.

Once the HILAC was up and running again, I focused acquiring the data for my thesis, with the hope of finishing by the end of summer in 1961. Seaborg's heavy administrative load at the time absented him from much direct guidance, but John Alexander and Earl Hyde were there to back him up.

In the winter of 1960-61 newly elected President John Kennedy threw a curve ball at my thesis plans when he tapped Seaborg to be the new head the Atomic Energy Commission, the forerunner of the present Department of Energy. His date of departure for Washington, DC was set for April 1, 1961, several months before the anticipated completion of my thesis. It took many long days to complete the document before April 1st, during which time I also took up distance running to preserve my sanity (assuming running 30-40 miles per week can pass for sanity). The departure date also meant that there would not be enough time to finish 'The Masses'. By late March the document based on my HILAC studies was in Seaborg's hands and he signed it on his last day in the laboratory before departing for Washington, no questions asked. Not sure he ever read it, since it contained no mention of "The Masses". However, they were not forgotten.

In order to account for the time lost due to the HILAC shutdown of 1959 and his premature departure from Berkeley, Seaborg made arrangements for me to continue work on "The Masses" as a postdoc at the Radiation Laboratory. And since he knew of (and encouraged) my interest in teaching, he arranged a position as an Instructor in the Chemistry Department at Cal.

The next two years turned out to be among the most profitable of my scientific career. It was an exciting time for science at the HILAC. In addition to the new element program, Dick Diamond and Frank Stephens were initiating their studies of high-spin rotational bands, Ron Macfarlane was discovering numerous neutron-deficient alpha-decay isotopes, John Alexander was investigating the effect of high angular momentum on neuron evaporation rates and many graduate student colleagues -- among them Bruce Wilkins and Marshall Blann -- were pursuing their theses.

In the latter stages of my thesis studies, Torbjorn Sikkeland, a staff member in Ghiorso's group at the HILAC, and I had been discussing discrepancies between the fission angular distributions on uranium measured by Glen Gordon in his thesis work and those I had reported on gold and bismuth targets in my thesis. Although my intentions about 'The Masses' were honorable, my research interests continued to be in heavy-ion-induced fission reactions, while the mass-calculation project remained a project I needed to get to -- one of these days.

Since Sikkeland was working for Ghiorso (which was roughly equivalent to two full-time jobs), he asked me to join him in a program to study heavy-ion-induced fission reactions. A particularly appealing aspect of this collaboration was a recent development by the detector laboratory where they had fabricated some of the first silicon semiconductor nuclear-particle detectors. This development placed us in the unique position of having some of the first such detectors and working at the world's most advanced heavy-ion accelerator, which permitted us to pioneer the use of semiconductor detectors for investigations of heavy-ion-induced fission reactions. Over the next two years, we utilized these new devices to study fission reactions on targets between terbium ($Z = 65$) and uranium, measuring cross sections, kinetic-energy release, angular distributions and demonstrating the effects of linear-momentum transfer, which resolved the differences between my thesis data and those of Glen Gordon. As is often the case in science, being in the right place at the right time can be a counterweight to one's limited intellectual capabilities.

However, while the science was highly productive, 'The Masses' fell by the wayside.

At the beginning of 1963 I began an NSF postdoctoral-fellowship appointment at the CERN laboratory in Geneva, Switzerland, where I quickly learned, "Toto, we're not in Berkeley anymore". My goal was to exploit the new semiconductor detector technology for the study of a new reaction process called fragmentation that could only be observed at high-energy accelerators, such as those at CERN. While the project was the first study of nuclear fragmentation with silicon detectors, it was also the first unsuccessful such endeavor. I had failed to appreciate that the technical support that was available in Berkeley was not a universal characteristic of all nuclear-accelerator laboratories. Whereas at Berkeley, Seaborg insured access to necessary personnel and resources, at CERN the high-energy physicists perched atop the pecking order and nuclear chemists were near the bottom. Further, the project was technologically far too primitive to succeed. Major advancement in computers, electronics and detector technology were required to address this problem -- and it was another 30 years before I could return to it properly.

Once at CERN, my conscience directed me to bite the bullet on "The Masses" and let Seaborg know that my two-year extension at Berkeley did not serve to advance his favored project. I had not heard from him since his departure to the AEC, other than to exchange drafts of my thesis paper for Physical Review, on which he had primarily editorial corrections. So I wrote with an apology for my inattention to my designated task, assuming his new duties had erased the project from his priority list.

His return letter was prompt and emphatic. Somehow in five years I hadn't gotten the message. He wanted the project completed! It was probably fortunate that email did not exist in those days, as his response had to be filtered through his secretary. From that point forward, I devoted a significant fraction of my time at CERN to the tedious business of correlating nuclear data in order to predict the properties of unknown nuclei.

Over the next two years we carried on an active exchange by mail regarding the mass-energy calculations. I would send regular reviews of the status of the project, to which he would respond promptly, with detailed comments on my approach to the problem and new data that his contacts at Berkeley had provided him. He also suggested that we expand the program to predict the half-lives of the unknown nuclei in our tables, especially those for spontaneous fission. In the end, I came to appreciate that if I had wanted a more rigorous interaction with my thesis adviser, I should have concentrated on the masses project. However, in retrospect, the project profited from the delay, as the expanded data base that had been accumulated in the intervening years was much richer, and major changes had occurred in the field, for example, the shift from ^{16}O to ^{12}C as the reference point for calculating atomic masses. In this regard, I also benefited greatly from interactions with Aaldert Wapstra, who gave me access to his latest (yet unpublished) tabulations of known nuclear masses, and Wladek Swiatecki, who provided valuable inspiration on the theoretical side.

In 1966 two papers detailing the predicted behavior of heavy-element nuclear energetics and half-lives were finally published, bringing to an end the long gestation period of "The Masses". While the project had never received my enthusiastic embrace, I was amazed by the number of reprint requests and citations "The Masses" generated. It was a subject to which I would never return, other that to check occasionally to gauge the success of the work as new discoveries were reported. Such investigations were better left to those who could refine the calculations by taking into account the rapid advances in computer technology. Nonetheless, although the computer can survey the atomic mass landscape broadly, it can also distort predictions that can be made more realistically by empirical extrapolations that consider localized nuclear irregularities such as those created by nuclear structure effects and isotopic spin.

One personal dividend of the "The Masses" project occurred when Seaborg referred Cal Tech's Willy Fowler to me concerning a question about stopping points in heavy-element nucleosynthesis. Fowler was concerned with the nuclear decay mode of the then unknown nucleus ^{259}Md. My analysis concluded that this nucleus would decay by spontaneous fission and thus block any additional mass build up by neutron capture. The prediction later proved to be correct, which relieved my conscience. Fowler's enthusiasm in this exchange sparked my interest in nuclear astrophysics, and some years later prompted Willy and his colleague Don Burnett to direct me to the study of lithium, beryllium and boron nucleosynthesis, which I was in a unique position to investigate at the Maryland Cyclotron.

With "The Masses" completed and my acceptance of a faculty position at the University of Maryland in 1966, interactions with Glenn assumed a more collegial role. He was certainly very supportive of my faculty career and since we were both in the Washington, D.C. at the time, there were occasional opportunities to resume our mutual interest in sports, as well as to enjoy an occasional social function, such as a reception and dinner at the Cosmos Club, which my wife Nancy particularly enjoyed.

By 1973 Seaborg had divested himself of service at the AEC in the Nixon administration and returned to Berkeley. I was due for a sabbatical leave from Maryland that year and he arranged for a visiting appointment to complete my salary needs. The goal of the Berkeley year was relevant to his interests, as I wanted to study reactions between very heavy nuclei and the possibility that they might fuse together to form new exotic heavy elements. The laboratory had just completed construction of the SuperHILAC, the world's most advanced machine for accelerating heavy nuclei such as krypton and xenon, so it was ideal for this purpose.

Research at the SuperHILAC was painfully slow in the beginning phases of operation. Repeatedly, we would prepare our experiment, only to have the machine malfunction after hours of waiting. Groups of hopeful outside users would come and go, only to return home dataless. Then in December of 1973, just before the laboratory was to be shut down for Christmas break, the machine began to spit out its first sustained beam of krypton ions. Kevin Wolf, John Huizenga and I were fortunate to be the scheduled users of the accelerator and after a day of preparation on our equipment, were able to pursue our experiment non-stop for 48 hours. It would be several months before the SuperHILAC would deliver a repeat performance. So we were lucky to have been present at the right time.

When the accelerator was turned off at 8:00 a.m. for the Christmas shutdown, Kevin and I believed we had seen evidence that the collisions between krypton ions and bismuth nuclei had fused temporarily. In our defense, neither of us had slept for more than a few hours out of the last 72. Once the experimental area was secured, my misplaced euphoria directed me to Seaborg's office to give him the 'good' news that the reactions we had been studying might open the door to a new avenue for heavy element synthesis. As it turned out, our premature conclusions were off base. Instead, we had observed a new reaction mechanism that would complicate synthetic element research in the future.

Several months later, I made a presentation to Seaborg's group and a visiting contingent from the Soviet Union, headed by Georgi Flerov, explaining the ramifications of this new reaction mechanism (which actually had been observed in Flerov's laboratory by Volkov and Wilczynski somewhat earlier, but the results gained little traction in Flerov's group and were not readily available at the time). Our results left no doubt as to the magnitude of this new process. However, the impact of my presentation was minimal, as both the Ghiorso and the Russian group continued to pursue their long-standing A + B = C approach to heavy element synthesis. It fell to Peter Armbruster's group at the GSI laboratory in Germany to appreciate the nuclear physics implications of forming new elements with very heavy projectile nuclei. As a consequence, within a few years, the Germans became the dominant players in the field.

For a graduate student the downside of working for an internationally-acclaimed scientist is that it is sometimes difficult to put everything in perspective. There is a tendency for the scientific community to overlook the critical contributions of those responsible for an experiment's success, when all the acclaim is focused on the senior investigator. It always left me somewhat uneasy that Seaborg would attach his name to many publications for which he had little or no direct involvement. The flip side of that concern is that I enjoyed state-of-the-art physical resources and exceptional scientific colleagues that enabled the research to be performed in the first place -- for which Seaborg deserved major credit. And while he rarely provided insights on non-heavy-element nuclear physics (at least in my case), he was highly dedicated to education in the broader sense, especially at the high school and undergraduate levels.

After I returned to Maryland at the end of my sabbatical leave, and later at Indiana University, my principal contacts with Glenn were during frequent visits to the SuperHILAC for experiments, and at American Chemical Society meetings. On one occasion he invited me to join him at a San Francisco 49ers football game. But my lasting memory of Glenn Seaborg was when I encountered him at the ACS meeting in San Diego on the day that he learned that element 106 had been officially named Seaborgium. His elation at the news was comparable to a pitcher who has just thrown a perfect no-hitter. Perhaps that's the best way for me to remember him.