Albert Ghiorso – Notes for Memoirs

Assembled from AG's computer

RWS notes on AG's notes Jan, 2010

I acquired these notes from AG's daughter, Kris Pixton, who had assembled them from AG's computer. Few of the entries were dated, and there was considerable repetition. Apparently some of these notes were written in the 1990's and some around 2005, and possibly other dates.

I have considerably re-organized these notes, and created new categories where it seems clear they are appropriate. The overall organization retains rough chronology of AG. The following outline is temporary, and certainly will evolve as the chronology becomes more clear.

AG Meta-notes
0. Childhood and formative years
0.1 Formative activities
1. Early days at the Manhattan Project
2. Move to Berkeley
4. Detection of the [First] Russian atomic bomb
3. The Mike explosion
5. Element 101
6. Development of the HILAC
7. Element 102
8. Element 103
9. Elements 104 and 105
10. The Omnitron
10.1 The Super-HILAC
12. Element 106
11. The Bevalac
13. GSI and Elements 107, 108, 109
15. Element 110
15.1. Element 118
15.2. Other scientific activities
16. Nonscientific activities
19. Personalities
20. Retirement
21. Legacy.

One aspect that has been dropped in this re-organization is the list by AG entitled "Pivotal Points in my Life." This list was highly redundant and uneven; it is questionable whether it was even accurate; more likely it was just items that AG remembered as he made the list. That is, I doubt that the list "pivotal points" was significant beyond his recent, admittedly fragmentary, memory.

I have emphasized comments by AG regarding the source of material; these are indicated by AG:_____. Comments by myself are indicated by [RWS:_____].

AG meta-notes

These memoirs were started

…because I had a lot of idle time suddenly made available when I began working on the element-110 data!

Pivotal points in my life

…when decisions could change the future for years to come. The principal one was clearly my decision to drop the Omnitron. If we had kept on that track and eventually persuaded Congress to fund it we would have changed the entire history of the heavy element discoveries; the BevaLac would never have been built; what else? It would have had a major impact on the Radiation Lab to say the least!

Incidents

[RWS: In AG's notes, this was called "Instruments", but I believe he meant "Incidents".]

0. Childhood

Parents

Father

My father as a bootlegger
A Registrar of Voters in the Wallace campaign of 1948

…in which I discovered that my father had never been naturalized, though he had voted for the previous 50 years.

How I quit smoking at age 12

My big sister caught me. A very simple story but it had very important consequences. A boy, younger than I, lived in a house nearby in Alameda who had had polio. I don't remember his name. His mother gave him cigarettes to smoke regularly, I guess because of the polio, and he shared them with me occasionally. I soon became addicted and because I did not have any other source of funds (we were very poor) I took to scrounging on the streets for discarded butts to supplement those I got from my friend. It was great, I thought, so much so, that I began to even smoke in bed! Fortunately, something happened. My big sister, Genevieve, four years older than I, smell smoke one evening and when she investigated found that I was smoking in bed! I don't remember what she said but it must have been powerful because that put a stop to my smoking habbit immediately! Unfortunately, she doesn't remember the incident at all. I admired her because she was an excellent student and was going to be a success in the world. Later, she was responsible for getting me a small scholarship to go to the University of California. I was really lucky because I was well on the way to becoming a permanent smoker; stopping at the age of 12 was a good time to stop this stupid habit.

The big Lincoln School fire.

I don't remember anything about it except that it was a spectacular because it was a large wooden structure, probably four stories high.

Tumbling

…with Eric Bergman

The small boy as an engineer

Shingles, tunnels, playhouses, the sawdust piles

How I lost my organized religion

…as a high school student in a world history class. Was it Roberta Monks?

Working in the shipyard

Rivet passer

…as a passer of rivets first and then as an electrician's helper. Lost-time accident--a keg of rivets dropped on my foot!

ACCIDENT! The near-fatal fall into the hold of a double-bottom tanker

In the intervals between sessions at the University of California I was able to get helper jobs at the shipyards along the estuary between Alameda

and Oakland. That came about only because my father was a good friend of the straw boss where he worked at General Engineering & Dry Dock. Through this influence I was introduced to the working class fraternity in 1933 in the depths of the Great Depression. For the first time I learned what it meant to get a paycheck. The check was not very large even for those times, 35 cents/hour, but it was something. And it was all mine because I lived at home and went to work with my father in his Model A Ford In those days we had the "shapeup" so we did not necessarily get to work every day. We never knew in advance whether there would be any work for us. After a few weeks even that stopped because of a big strike. A few months later after the successful strike, all the workers' wages went up but because mine was so low I gained more than most; my new pay scale was now 90 cents/hour, and I really felt that everything was going to be all right. I had a job and I was going to school. The very first day that I went to work I was assigned to be a Passer in a Riveting Gang with my father.

A Riveting Gang consisted of four persons, the Heater, the Passer, the Holder, and the Riveter, the most important being the Riveter, my father. The Heater would heat the rivets to a white heat and then toss them to the Passer. He would snag them on the fly in a small conical sheet metal catcher, grab them with a pair of tongs and put them in a hole in an iron plate that was to be riveted to another. The Holder would then press a heavy piece of metal against the head of the rivet to hold it in place while the Riveter pounded and shaped the projecting end of the rivet into a rounded head. The heavy riveting gun was run by compressed air so the whole operation was very noisy. It was also often done in a poisonous atmosphere in drydocks that were used to repair double-bottom tankers that had been in service for years. Sometimes, as a rivet glowing at about 8000C was inserted into its hole, the thick iron plate would heat up and any left over oil on it would burn producing clouds of choking smoke. Efforts were usually made to clean up the areas to be worked on but occasionally they were not very effective. The workers often had to rely principally on air blowers to supply them with air that they could breathe.

My very first job was a terrible ordeal. It was inside a large iron cylinder roughly ten feet in diameter and about thirty feet long as I remember and was being worked on **simultaneously** by four riveting gangs; the noise was literally deafening and almost continuous. The riveting was on the outside so the rivets had to be put in place on the inside. That meant that the Passer (me!) had to be on the inside to set the rivets in place. This was my very first experience in the shipyard and I was not prepared at all. No one had warned me to use ear protection! The heater for the rivets was on the outside of course and the very hot rivet was passed to me through a port hole by tongs. I then had to scramble with it and put it in the small hole where it was to be riveted in place. Between the noise and the fumes I was given a terrible baptism of fire into this primitive technology for four hours until lunch time. And then it was repeated for another four hours until quitting time. At home that night I noticed that the radio was not very loud; I had to put my ear against the loudspeaker to hear anything at all. Fortunately, the work on the tank was finished in just a few days and for that I earned about ten precious dollars!

As time went on I became experienced in the way of the workplace and I worked in many different places in the yard. Sometimes I would be high on a

scaffold and have to catch the hot rivet flung from a distance of as much as fifty feet. Since I was a baseball player I thought that this was exciting and enjoyed it! I escaped without any serious injury for a while on the job, until a hundred pound bucket of rivets was dropped on my foot in the supply area. That put me out of action for a couple of weeks even though I wore heavy shoes that were supposed to protect against such accidents. The straw boss, Jess Hansen, did watch over me because, occasionally when there was not enough work for me to be hired he would give me the job of washing his private automobile.

It was standard procedure while working with a riveting gang that when we had finished our scheduled job ahead of time our gang leader would tell us to "get lost" until quitting time. We were on a boat in drydock and I had been working on a dirty job in the double bottom below decks, so I decided that I would go up and get some air and explore around a bit. It was a bright sunny day outside and it was nice to get outside for a while. Up on the top deck I came to a place that looked interesting. It was a covered hatchway open on both sides and I could see through it to the other side of the boat. Without thinking , I crossed over into the dark place to get to the bright sunlight of the other side. There are usually three or four of these hatches, one above the other. Normally, the hatches were covered over with heavy planks that could be removed but the top one was not there now! I stepped into darkness and was horrified to find myself falling. It could not have taken more than a second to fall the approximate fifteen feet to the next hatch cover but it seemed like a lifetime! Fortunately, the next hatch cover was in place otherwise I would not be writing this some seventy years later! I was about eighteen years old and in pretty good shape. Furthermore, I was a tumbler and knew how to fall so I must have instinctively done the right thing for when I landed on the hatch planks below I was only momentarily stunned. I had not broken any bones as far as I could tell so I crawled over to the ladder and made my way out of the hatch to go and tell my work companions about my near escape from death. They were not in the least sympathetic. They laughed and told me what a stupid fool I had been! Perhaps that was the best medicine that I needed because I must have been in shock.

UC Berkeley

How my father tried to enroll me at Cal

…so that I could become the first honest lawyer!

I was very conservative at cal

…compared with the other engineering students. Wilma changed that very quickly.

ROTC at CAL

How I got dinged for wearing a blue sweater as a part of my ROTC uniform

When I was attending the University of California from 1933-1937 ROTC (Reserve Officer Training Corps) was compulsory and most of my class found it abhorrent and a complete waste of time. But it was mandatory and one had to make the best of it. The classes were ludicrous so it was easy to make fun of the playing-at –soldier routines. The classes were stupidly simple so it did not require any study. I use to read the required material on the bus and streetcar that took me from Alameda to the University on the morning of each class.

One of the absurdities of the course was that the class counted for grade points so missing even one class was not the thing to do. One morning for some reason that I don't remember I did not have on the white shirt that was required to go with the uniform. I thought that it was better to show up even that way rather than not show up at all. That was a mistake because when the drill seargent saw the blue sweater he saw red and shouted out "Ding that guy!" Sure enough, my record showed the only D that I received in my whole college career. I would have been better off if I had not showed up at all!

How I became acquainted with Wilma Belt and Helen Griggs.

The big intercom job at art jensen's

…which led to my finally getting my teeth fixed up and thence to marriage with Wilma, the most important thing in my life. Wilma ended up by paying the big dental bill after we got married!

Pearl Harbor Day 1941

Spent the day in Yosemite Valley with Wilma without knowing that WW2 had started!

Tuolumne Meadows

Tuolumne Meadows as a vacation retreat
The obsidian experiment at Tuolumne Meadows –
AG: Look up Yosemite Nature Notes

The Lee Vining Canyon flash flood of 1955. The frog!

About 1950 (?) we started to visit Yosemite National Park as a family and immediately became aware that this jewel was for us. We soon graduated from camping in the valley when we went up into the High Country and came under its spell. For some fifteen years we would spend our summer vacations at Tuolumne Meadows Campground and explore the area on foot. As we became more experienced we spent more and more time there, as much as a month every summer. We learned to appreciate the wonderful outdoors in the best way possible, by living there. The year 1955 was special, though, for I had been invited to attend the first Atoms For Peace Conference in Geneva, Switzerland. That was quite an honor and I planned to make the most of it by announcing for the first time publicly the names for elements 99 and 100. I would suggest that element 99 be named einsteinium after Albert Einstein and that element 100 be named fermium after Enrico Fermi, so I looked forward to attending the Conference. But I needed a passport.

After pondering the schedule and finding that I had no time to spare, I decided that it would be simplest to get my passport in Bridgeport which is not very far from Tuolumne Meadows. In 1955 we had a Volkswagen Beetle and I had equipped it with a trailer hitch and a supercharged engine so that we could pull a small trailer for camping; with that we got around the high altitudes very nicely. In fact, we could outrun (and did!) the Detroit monsters very easily. Early one morning we left the trailer at the campsite and the four of us, Wilma and I , Kristine (111/2) and Bill (almost 9), set off down the 13-mile Tioga Pass Road to go to Bridgeport. It was a pleasant drive on the relatively narrow road with lots of curves and the weather looked beautiful. Bridgeport is only about 50 miles from the bottom of Tioga Pass so it was not long before we arrived there and I got my passport.

We did not spend any more time there than necessary because we wanted to get back to Lee Vining so that we could explore the nearby Mono Craters, one of our favorite places. Whenever we were in the vicinity of Mono Lake we visited the Craters. Bill was particularly enamored of that geology and usually picked up samples of obsidian that attracted his attention. Sometimes he would put them in the campfire at our site in the Meadows to see what would happen to the black glass. Usually they would disappear in the coals but one time he had a clear case of their changing to the foam-like rock that one finds at the Craters and we knew that we had observed what must also take place in a volcano. Later I confirmed the experiment in the Laboratory with a blowtorch. We brought this fact to the attention of one of the rangers and he eventually published a short paper in Yosemite Nature Notes about Bill's experiments.

In the early afternoon we decided that we had better get back to the High Country because it looked as if we were in for a storm and we certainly did not want to get caught in it on the way up Tioga Pass. But the storm was much swifter than we were and soon the clouds had descended upon us and with it semi-darkness and heavy rain. We accelerated but it was much too late. The storm was already in the Pass and its torrential rain was very menacing. When we got up to the Ranger Station at the bottom of the Pass we encountered a number of cars, coming down from above, we assumed. But that was not the case; those cars were also going up into the Pass but had encountered a big landslide that blocked them from going any further and had to turn around. Not understanding what had happened, we kept on going and a little farther on we got to the big slide that had just blocked the road. Now we understood what had happened; obviously, it was time for us to get out, too! As quickly as possible, I turned the Beetle around but it was too late for a new slide containing big boulders was moving across the road as we watched helplessly. We were trapped! And for all we knew, a very large section above the road was about to come down on us, too. We had to get out of there! We had stopped just above the western end of the campground and there was a house trailer parked next to the creek. But that little creek was now a big river and had overflowed its banks. I waded down to it to see if any assistance for us from its occupants was possible. Its hysterical occupants were in worst shape than we were and I quickly saw that our best course of action was to walk around the slide and go towards the Ranger Station in the half-light. There was heavy rain and it was hard going but we did not have any other choice so we slogged ahead the half mile or so that it would take for us to get to safety.

At the Station we were received with surprise but got a warm welcome. We soon found out that the Ranger also had two children of about the same age and also the same names!

The next morning we reconnoitered the slide area to see how the Beetle had passed the night because we half expected that it had perished in a big slide. But it was in fine condition and in the same place where we had left it. I had come to a halt between two large slides because there was a huge solid wall of rock perhaps 100 feet long or more on the cliff side that projected out to the road; the slides that trapped us came from around our savior. So far, so good. Could I get the Beetle out? I opened the car door and out jumped a large frog! It must have jumped in when we got out the night before; there was no other possibility !

I decided that I might be able to rescue the Beetle by driving down towards the creek to where I could skirt the slide. That worked thanks to the supercharger and we could now continue. The Ranger told us that he had learned that there were something like thirteen major slides in the Tioga Pass area and it would be many months before it would be passable by automobile. The only way that we could get back to our camp at the Meadows was by driving north to Parker(?) Pass and circling around back to Tuolumne. That would be something like 300(?) miles to travel the 15 miles back to the Campground!

We had a lot of groceries that we had purchased for camp so we left them with the Ranger so that he would be able to help any other wayward travelers and started on our long journey. All went well until we got to Kennedy(?) Meadows. (There we had car trouble and I have not remembered yet what it entailed or how it was fixed but we must have solved the problem somehow for we did get back to camp! I think that it had something to do with the supercharger, probably the belts that drove it)

The McCarthy era

Almost disastrous. I would have been fired except that GTS went to bat for me and was able to make it stick.

AG: I should look up his diaries and see if there is any mention of it.

[RWS: From conversations with BG, I believe this refers to the investigation of Wilma, and by association AG, for ultra-liberal activities. This is referred to in my notes from the phone conversation with BG 1/18/10.]

O.1. Formative activities

[RWS: I have no clear chronology of these events, although they are probably traceable via other sources.]

My first scientific discovery

The flat tire incident

…the heating of air caused by a tire pump noted when a young lad on my uncle's ranch in St. Helena. A budding scientist?

Learning science in high school

The Joel Hildebrand method was taught at Alameda High School.

Model airplanes

I almost became an aeronautical engineer!

"Letters from a Radio Engineer to his Son"

By John Mills

A very important book to me because it determined that I would enter Cal as an engineer instead of a lawyer.

AG: I wonder whether I could find the book in a library; it has a very unusual title.

[RWS: The book is available online, and I downloaded a copy of it. See the file John_Mills_book].

My first exposure to negative feedback at Cal in 1936-7

Auditing a course by Prof. L. Reukema in which he spent many weeks on the theory of the recent discovery by Black at the Bell Telephone Labs. How one time he spent the whole hour developing a complicated equation and came up with the wrong answer! I don't think that it was negative feedback, probably antennae design.

Attendance at a few of the Journal Club Meetings

…in the late 30:s. My first taste of the excitement of scientific meetings.

Radio bootlegging

First contact with Ohio on the 5-Meter band. Used a xtal-controlled xmitter and a superhet receiver, all home-built, of course, with a W8JK directional antennae. About 1937, I think. Talk to Gilbert.

Working for nothing

Working for nothing to gain experience with D.R. Tibbets

…and his Communications Supply Co.

A device to measure radio frequencies accurately

My first research job that led to a published paper, though I was gypped out of authorship by my boss.

This was a very interesting interval in my developing career. It was the first time that I demonstrated a talent for original creativity and the publication of it in a journal should have borne my name on it. My boss, D. Reginald Tibbetts, was a very selfish man and deserves a few paragraphs someplace in the book. He had sent in the article without listing me at least as a coauthor. He told me that he had included me and the publishers had

deleted me. I did not believe him. He gave me the $35 fee that he had received. I never trusted him after that. I was very proud of my little invention, even though it was very simple. It was an audio oscillator that I had constructed to measure the beat frequency between an incoming radiofrequency signal and a 10 kHz harmonic oscillator that provided accurate intervals over the whole band.

Consultant for Tracerlab

Consultant

Shasta dam

Designing, building, and operating radio communication equipment for the construction of the Shasta Dam. Police radios.

Building the world's first commercial GM counter circuits

The jackrabbit pulse height analyzer

My near venture into commercial electronics

0.2 Professional activities

[RWS: The chronology of these events is not clear. Probably most of these will ultimately be moved to other sections.]

Heavy ion experiments at the 60" cyclotron

…with Bernie Rossi which led to the design of the HILAC (suggested by Alvarez). This is the time when I came to know Chuck Corum as a valuable designer and I stole him away for the HILAC a few years later.

The famous meeting in Cincinnati in October,1957

…which lead directly to the huge Actinide Program. Bill Crane. Get a copy of the memo that I wrote to GTS on the return flight and then his resultant memo to AEC Chairman Strauss.

Here was a case where the long range insight of Seaborg played a very important role. An important meeting had been called in 1957 in Cincinati to discuss the future uses of the big nuclear reactors that were being planned and Glenn said that I should attend it and stake out our claim. I did not want to go because I was busy with the element-102 problem but he insisted because he could not go at that particular time and he knew that the way to make gram quantities of the heaviest elements lay in the tremendous neutron fluxes that were going to become available. He was absolutely right. That was where the future lay but I was trapped by the question of how could one ever use gram amounts of americium or curium in an accelerator experiment. The simple answer to that is that heavier isotopes can be made with neutrons that cannot be made with accelerators and these atoms in turn can be used in accelerator experiments. I was supposed to go to the meeting and present a talk on what the needs of Berkeley were for large quantities of actinides. (Look up the GTS Journals)

I went to the big meeting, not expecting too much, but I made my presentation, almost a pep talk, in which I explained what could be done with the multigam quantities of the heavy curium isotopes that could be made with the high flux reactors that were being projected. I happened to sit down next to Dale Babcock of the Savannah River Project where Du Pont had built and was running several huge reactors in N. Carolina that were devoted to making **tons** of plutonium for the military program. He whispered to me that they had the neutrons that I had projected that we needed for the future and all that had to be done was to have the MTR fuel assemblies refabricated to the one-inch diameter required by his reactors and irradiate them further. That was exciting news to me and I sought out Bill Crane to tell him what Babcock had told me. Crane, representing the Livermore now, had just gotten his PhD at Berkeley in our lab under Seaborg. During a break Bill and I got together with Dale to discuss a possible scenario and timetable for his suggestion to make macro quantities of americium and curium at the SRP. We could see that it would be the ideal target material for the projected High Flux Isotope Reactor (HFIR) at ORN; with a planned neutron flux of 5x1015 it would be able to make large quantities of the heaviest elements, ultimately as much as gram amounts of californium in a few years! It was a very stimulating meeting and I was so impressed with the idea that I did something that I had never done before. On the plane that took me back to

Berkeley I composed a memo to GTS that suggested the long range program needed to make our dream come true. The next step, Glenn decided was a letter to the Atomic Energy Commission Chairman, Lewis Straus to start the ball rolling. In his letter Glenn outlined also the need for a very high flux reactor (the HFIR which ORNL had proposed) and a two-fold program to; (1) irradiate 239Pu in a high flux production-type reactor (the SRR) to produce 244Cm, and (2) irradiate that curium in the very high flux reactor to produce berkelium, californium, and einsteinium in substantial quantities.

Thus began the huge program that was to be so important to future developments in the heavy element field; it was going to take many years and cost many millions of dollars. Fortunately, Seaborg himself was to become the next AEC Chairman and thus he was in the perfect position to oversee the program and make sure that it stayed on course. And that is what happened.

The HFIR was built and operating by 1965 and the processing plant (TRU) needed to process the transplutonium products from HFIR was operating by 1966.

AG: I should be able to get a lot of material by digging into the archives. [RWS: What archives? LBL?]

"Science can only seek the truth" (1959)

--Moscow State U.

AG: See if it is possible to get a picture of the blackboard where I dreamed up the above quotation.

In 1959 Prof. Ivanenko invited me to talk at MoscowUniversity on my research in the heavy element region. It was quite an honor and the hall was full. I thought that the talk, which was about the element-102 work world wide, went very well. I started out by detailing the work at the Stockholm cyclotron that was published in 1957. After about a year's difficult research at Berkeley we had shown it to be completely erroneous and had then gone on to find some real isotopes of that element. All of this I laid out in some detail and I felt that I had done a competent job.

At the end of the talk Ivanenko invited me to a nearby room and asked me to autograph a special blackboard that he had reserved for guest speakers. The guest was supposed to write a short sentence on the board and then sign it. Afterwards they would spray something on the board to preserve what had been written. I had not known about this "requirement" ahead of time and I had only a few seconds to think about what to write. Unfortunately, I did not have a camera with me so I have only a dim memory of what was already on the board. I do remember one quotation which struck me as being quite profound. Paul Dirac had written "Mathematics must not only make sense, it must have beauty!" So, on the spur of the moment I wrote "Science can only seek the truth!" and signed it. Stanley Thompson, who was with me, also signed it. Unfortunately, I did not have my wits about me for I should have asked the Russians to send me a picture of the board. I never saw it again and I was never able to trace it when I tried several years later.

The big experiment at Argonne on ^{239}U

…to measure the fissionability of the 23-min ^{239}U.

Look up the report; it used to be secret but it has probably been declassified by now.

Counter development at Chicago and at Berkeley

Golf, GTS, and Jack Rivers

…the Chicago area and then Berkeley

The 1004 Restaurant

A good place to talk about falls and the tricycle
That's where a lot of the planning and ideas originated

Working with Burris Cunningham

…learning how to do careful science

Volatility "chemistry"

My supercharged VW

Its use as a fast "chem lab" for the 101 experiment.

The first recoil experiments

…a rebuff to the experts

Playing golf with Luis [Alvarez?]

…caused his invention of the golf trainer

Giant haloes.

Gentry, McMillan, Florida State and the "superheavy elements".
[RWS: See separate essay]

The 1963 GTS-led delegation to the USSR

of which I was a member. Getting Goldanski onto the President's plane.

Student tours of the HILAC

…started by Bob Berges of Alameda High School

Science in Action program with GTS

1. Early days at the Manhattan Project (1942-1946)

How I came to join the Manhattan Project

The Jones Laboratory

My briefing by Glenn

…went right over my head! I did not understand that the chief goal was to make Pu. I kept wondering why they were throwing away all of the power that was generated in a nuclear reactor!

Getting adjusted.

The beginning of my education into the mysteries of nuclear energy and there were lots of them.

Confrontation with a Chicago cop

…on 63rd St on a Sunday afternoon **How I almost went to jail**

Development of new kinds of counters

…under the guidance of Spofford English.
Library work.
Learning, learning, learning.

I remember my amusing goof when I thought that I had made a real breakthrough when I found that I could "improve" the signal/noise ratio from an amplifier by passing the signals through a bias amplifier. I had the job of making the alpha counters more reliable At the time air ionization chambers were used and the s/n ratio was poor because of slow rise time and their sensitivity to microphonics. At the time air ionization chambers were used and the s/n ratio was poor because of slow rise time and their sensitivity to microphonics.

Development of a portable counter for finding lost specks of ^{239}Pu

Search for trans-Pu elements with Ralph James

At St. Louis U cyclotron under the direct supervision of GTS
Crude equipment

High radiation levels Unknown chemistry and radiations.

Using a mica absorber

… to absorb the alphas from a 1-mgm/cm2 target bombarded with D's.

Use of mica to make a range curve for trans-Pu

The final discovery of element 96 and then 95

The disclosure at the U of Illinois.

My first scientific thrill when Glenn told me that I was going to be listed as a co-discoverer. I was officially a scientist!

Amusing "discovery" of a very high SF rate from a sample containing ^{242}Pu

…until I found that it was due to charging up of the sample by the intense alpha radiation from the ^{239}Pu.

Thermal neutron fission measurements

…of actinide elements at the first Argonne reactor

The discovery that ^{239}Pu emitted x-rays

…which lead to my very first scientific talk

Chicago 1945? It could have been disastrous because I had mistakenly deconvoluted the absorption curves backwards. No one noticed.

Cutting of Fermi's cable

I used to make routine measurements of the fissionability of all of the heavy element isotopes that I could lay my hands on, both long and short-lived. I had designed a special chamber that would fit into the graphite thermal column and I used to go out to the reactor about once a week so it became a routine. One time when I went out I discovered that there was a cable that went into the four inch square hole that prevented me from sliding my unit into place. I was very impatient of this interloper and cut the cable. A little later I found that it was Enrico Fermi's cable! Very embarrassed, I sought him out to apologize. He was as nice as could be and told me not to worry about it. From then on he would consult with me as to the results that I was getting. He was a real gentleman.

My original measurement of the 1945 Alamogordo ^{239}Pu explosion

A small group from Los Alamos had come to Chicago to measure the amount of ^{238}Pu made by the n,2n reaction on ^{239}Pu in the first nuclear explosion. They were going to measure the fission/alpha ratio. When they found out that I could measure it directly they asked me to make the measurement to check what they had found. The 48 was about 10% of the 49 by activity. That was why in 1950 at Berkeley when I was asked to measure the alphas from a tiny 4 count/min sample I was able to identify it as ^{239}Pu from an explosion with a low efficiency, the first nuclear explosion in the Soviet Union! I went on to add some more information by examining the other radiations from it. I was a long range nuclear detective!

The ^{233}U decay chain –

How we almost made a major goof

Atomic scientists of Chicago

Lobbying for the MacMahon Bill in opposition to the May-Johnson Bill.

2. Move to Berkeley (1946)

The long process of setting up a new laboratory

Elements 97 and 98

The big tank explosion at Bldg. 10

…next to the lunch room. If it had happened during the lunch hour it would have wiped out a good part of the scientific staff!

The "Ghiorsomeister" curves

The exciting discoveries of the artificial collateral alpha series

4. Detection of the first [Russian] Atomic Bomb (1949)

Detection of the first Atomic Bomb

The Joe-1 sample in December,1949--"The Cold War started because I proved that the Russians had expoded an atomic bomb!"

AG: Maybe I can find the original data when I clean out my office.

AG: I looked up the incident in GTS's Journal. It is mentioned in Vol. 4 on P.145.

On 8 May 1950 Bob Penneman asked me to pulse analyze the Pu sample. The plate arrived by courier on 17 May. On 29 May I reported that it had 4.8±0.3% Pu-238 by activity.

3. The Mike explosion! (1952)

The telegram from Washington

My brash proposal rejected by GTS.

But just a few days later we had found 99!!!

And then 100, a couple of months later!

AG: Look up data in GTS log of the incidents

The MTR experiments

AG: Somewhere else I have written a detailed account of the beginning of this episode. Find it.

5. Element 101

Crazy idea on a plane flight to the MTR

The experiments

My narrow escape from electrocution

…when I was frozen onto the high voltage PS that reset the 48-channel pulse height analyzer recorders.

My supercharged VW

Its use as a fast "chem lab" for the 101 experiment.

How element 101 came to be named Mendelevium

How we finessed it by the establishment.
The reaction at the first Atoms for Peace meeting in Geneva.

The first recoil experiments

…a rebuff to the experts

6. Development of the HILAC (1955-57)

Heavy ion experiments at the 60" cyclotron

… with Bernie Rossi which led to the design of the HILAC (suggested by Alvarez). This is the time when I came to know Chuck Corum as a valuable designer and I stole him away for the HILAC a few years later.

How the HILAC almost became a cyclotron

… at the last moment because of my encounter with EOL at a Sunday Open House.

AG: See if GTS has any notes of the meeting on the following Monday.

As I remember, it the cyclotron was quashed when I kept pointing out that beam extraction was a problem for the cyclotron, whereas it was automatic for the linac. I thought that my name would be mud with EOL from then on, but that was not the case. I would often encounter him at the HILAC at night during the testing phase (he tended to prowl the Hill to see what was going on in his Lab); he was always very friendly and I would fill him in on how the machine was progressing. Unfortunately, he died before our big triumph with element 102.

Construction of the HILAC (1955-1957)

The subsequent regular improvement of the accelerator kept it viable with other machines, whereas its Yale twin was shut down after about 10 years. Our machine came on-line at the same time as the first Sputnik began to orbit the earth on 4 Oct.1957.

The beginning experiments were the search for "nobelievium"

We found "lopsided" fission in the very first experiment.

Discovery that we could vary the HILAC energy

Very important

7. Element 102

Transfermium Working Group's survey.

This is a good place to discuss in greater detail all of the facets of the work by all parties that got involved. Maybe include a critique of the For instance, raise the question of why the TFWG neglected entirely a discussion of the 0.3-s, 80-ms, 20 ms SF activity in evaluating the DUBNA work.

The AG/GTS letter to Wapstra letter of 26 Nov 1991;

AG: It has a lot of good quotable sentences in it.

The other day (Mar 1, 1992) Glenn suggested that I should get going on a rebuttal paper to the TFWG report because we may need it soon if they go ahead and publish their thing. I said that I would do it in connection with the "Adventures". Yesterday I asked Pat S if he would calculate the abundances of the nobelium isotopes that we made in the old experiments and suggested that he should be a coauthor of a rebuttal white paper on the controversial elements. He would be very good at researching the subtleties to make it a very definitive paper. It would also be very good for his scientific career.

The big spill of 1959.

The grid chamber which led to the big spill.

AG: Use Garden's report.

HADES and LIMBO

The chapter on element 102 in my 1990 Texas paper

I guess the first thing to do is to call up and here it is. I expect that I will leave out most of it, but it's a start. (Notice that it does not address the 250m Fm problem; that can be taken from the reports made for the TFWG.

---

5. Nobelium, No (Element 102) The First of the "Controversial " Elements

With the panoply of modern devices available to scientists today it is easy to forget that at one time they did not exist; thus, in 1957 the detection methods were still fairly primitive. Our slow 48-channel alpha analyzer was one of the larger instruments at that time and our Frisch-grid chamber one of the better ones. No one in the world had yet faced up to the new problems that would be encountered when it would be necessary to detect short-lived activities at the accelerators. Passive detectors had been used but these were highly limited in their resolution and performance; it would be four more years before the invention of solid-state devices would revolutionize detection systems. As a consequence, it should not be surprising that mistakes of interpretation would happen so that re-interpretations would have to be made. A corollary of this changing period is that the scientific standards would also undergo a profound change as the proficiencies of each experimental group improved. Experiments and their conclusions that were acceptable then, often would not be acceptable by today's standards. As will be shown, errors of various sorts were made by all groups during this period with very confusing consequences, but eventually almost all of the discrepancies were resolved.

Temporarily we had come to the end of the road as far as new elements were concerned. To make element 102 we needed to have a projectile with at least three protons because the heaviest target that was available was element 99. Some years earlier Rossi and I had developed a small internal beam of heavy ions at the 60" cyclotron but it had a continuous energy distribution. We experimented with beryllium, carbon, and nitrogen ions (GHI54A) and made tiny amounts of Cf, Es, and Fm so that it did appear that the heavy ion approach was a viable one; clearly we needed a better accelerator, but it would be late in 1957 before usable heavy ion beams would begin to make their appearance at Berkeley and, coincidentally, at Dubna.

Luis Alvarez, having just invented the linear accelerator (ALV55) and put it into successful operation with protons, proposed that a linac be constructed for heavy ions. Instead of using grids in each drift tube to overcome the natural defocusing caused by the electrostatic acceleration, he suggested that we take advantage of the new strong-focusing principle that had been enunciated at Brookhaven National Laboratory by Courant, Livingston, and Snyder (COU52) and use magnetic quadrupole lenses inside each drift tube (MAI66,MAI68). With no further ado a design study team was set up at Berkeley with members from both Yale University and the Radiation Laboratory with the objective of making twin machines, one at Berkeley and the other at Yale (HUB61). The goals were limited-- a fixed energy of 10 MeV/A, with argon the maximum Z that could be accelerated. The HILAC, for Heavy Ion Linear Accelerator (See Fig. 10), an acronym that I suggested and began using almost immediately, was the first accelerator in the world to use magnetic strong focusing. The maximum average beam current was only a few electrical microamperes, a limit set principally by the duty cycle and the output of the ion sources available at that time.

The design and construction went swiftly so that by early 1957 the Berkeley HILAC was completed and ready for debugging. It was not anticipated that the bottleneck would be the 90-ft-long poststripper tank. For many months efforts at pumping and baking seemed to make very little difference and we almost despaired of achieving an operating vacuum sufficient to allow the high gradients needed for the radio frequency accelerating voltages. The outgassing of the large areas inside the tank caused ion locks; the 2-Hz pulse rate and the narrow pulse width put very little power into the tank so that it took a very long time to release the gas on the walls. Finally, in October of 1957 after this long bakeout period we broke through the last of the ion locks and we had a working accelerator. Now we could look forward to searching for an isotope of element 102 by bombarding curium with carbon ions. Before this could be carried out, however, we were startled by news from Sweden.

A group of scientists from Argonne National Laboratory, the Atomic Energy Establishment at Harwell in England, and the Nobel Institute for Physics in Stockholm were claiming that they had just discovered an isotope of element 102 in research at the Nobel Institute (FIE57). Using the cyclotron at the Institute, they reported that the bombardment of ^{244}Cm by 90-100 MeV ^{13}C ions had produced an alpha emitter with an 8.5-MeV energy and a 10-min T1/2 which they believed to be due to that element. They said that the activity eluted early from a cation adsorption-elution column in the approximate position that they expected. They proposed that element 102 be

named nobelium in honor of Alfred Nobel, whose name was given to the Nobel Institute where this work was done and who made such a significant contribution to the advancement of science through his Nobel Prizes. The IUPAC commission accepted that name and the symbol, No, very soon thereafter.

We immediately set to work to try to confirm their findings for we assumed that they were correct and we saw nothing unreasonable about their published paper. We devoted many months trying to reproduce the Swedish experiments but without success (GHI58). The first method that we used was to catch recoils from a ^{244}Cm target in a thin Pd foil; this was quickly dissolved, an actinide fraction extracted, and the final plate alpha pulse analyzed. This technique took about five minutes but produced only ^{245}Cf, ^{246}Cf, and ^{250}Fm. The amounts that we observed were hundreds of times larger than those produced in the Stockholm experiments and can be considered as internal tracers in comparing the two sets of experiments.

We worried that element 102 metal might be so volatile that it would volatilize out of the warm metal catcher foil so we tried a second method. The recoils were caught in a Mylar foil which was cooled by helium gas; presumably, the element-102 recoils would oxidize in the plastic and thus not be volatile. After the bombardment the Mylar foil was placed upon a Pt foil, ignited to burn off the plastic as was done in Sweden, and then alpha analyzed. Again we saw large amounts of the lower Z actinides but no 10-min 8.5-MeV alpha activity.

A third method was tried which we felt confident would not discriminate in any way against the ephemeral activity. This involved the electrostatic collection of the recoils and their direct counting without any kind of chemistry. A negatively-charged aluminum plate was mounted near the target in a helium atmosphere and, after a short bombardment, was analyzed in our grid chamber. Although the actinide activities were easily seen, the Swedish activity was not.

A final method, which was developed later for another purpose, was also tried. This used a metallized tape to conduct electrostatically-collected recoil ions into an on-line grid chamber. Although this was successful in finding what later turned out to be ^{252}102, we did not see the 10-min alphas.

With the HILAC we had at least an order of magnitude more usable 13C beam than that available at the Nobel Institute; yet the identical experiments did not bring forth the phantom 10-min 8.5-MeV alpha activity. We used ^{12}C, ^{13}C, ^{14}N, and ^{16}O ions with a wide range of energies without success. We did see an unusual reaction, which we called "lopsided fission", wherein neutron-deficient isotopes of Th, Ac, Ra, and their descendents were observed at high bombarding energies. It is possible that this may have been the source of the mysterious alpha activity at Stockholm for one of the activities that we detected was 8-min ^{225}Th, an alpha emitter which gives rise to a chain of other shorter-lived alpha particles culminating in 8.377-MeV ^{213}Po. It should be noted that thorium does elute early from a cation ion-exchange column and this characteristic is compatible with the Nobel Institute experiments (FIE57).

The cyclotron experiment was a difficult one to perform since they used a weak internal beam of carbon ions with a broad energy distribution. The amount of activity varied erratically and was always very small, not more than a few events being observed in any one experiment. In most

experiments none were seen with only 12 out of the 50 bombardments being successful. Our fruitless search went on for about four months; it did prepare us to look for our own element-102 isotopes, however, and finally we began that search.

It was clear that chemical methods were not fast or efficient enough to do the job; from now on purely physical means were to be used to identify new elements for the first time. Eventually, however, it would be possible to do meaningful chemistry with isotopes that had half lives that were only fractions of a minute.

For this work a radically new method was tried (GHI58A). As already mentioned, a negatively-charged plate can attract nuclear transmutation recoils in an atmosphere of helium (GHI58A, VAL59). Once the atoms are on the plate it means that one has a thin sample from which it is possible to collect daughter products by recoil after alpha decay. This means, in principle, that it is possible to isolate daughter fermium atoms that are ejected by alpha decay of element 102.

A double-recoil geometry was arranged to accomplish this seemingly magic feat (See Fig. 11). A negatively-biased metallic conveyor belt was located beneath the target to collect the element-102 primary recoils; downstream from that area a second, more negatively-biased collector was placed close to the belt to collect the element-100 recoils after alpha decay of the element-102 atoms. This collector was divided into five sections which could be analyzed simultaneously by five grid chambers after each bombardment. The system was shown to work properly by bombarding a target of ^{240}Pu to make a new isotope, ^{248}Fm (GHI58A). This decayed with a T1/2 of 0.6 min to 20-min ^{244}Cf as shown by the analysis of the relative amounts of it that were transferred to the multiple catchers.

After we had demonstrated that this unusual "milking machine" worked, experiments were started to find a short-lived isotope of element 102 using a curium target that was 95% ^{244}Cm and 4.5% ^{246}Cm. The 500-microgram/cm2 target was bombarded by monoenergetic 12C ions with energies that were varied from 60 to 100 MeV. We had decided that the most likely isotope of element 102 that we might see with this system would have mass number 254 with a predicted T1/2 of seconds. After alpha decay it would recoil the known 30-min 7.43-MeV alpha emitter, ^{250}Fm, onto the second collector. A series of runs showed that ^{250}Fm did indeed appear on the catchers with a distribution that was in accordance with a 3-s T1/2 apparently made by the reaction, ^{246}Cm(12C,4n). The excitation function to transfer 250Fm in this manner was found to peak at 705 MeV. This corresponded closely to the value calculated for this reaction by the Jackson-Sikkeland method (SIK58), which had been developed recently by our group. That the atoms collected were ejected by recoil was proved by the fact that neither ^{245}Cf nor ^{246}Cf were found on the second recoil catcher, though they were in great abundance on the conveyor belt. Changing the belt speed changed the distribution of 250Fm on the second collector in a manner that conformed to the presumption of a 3-s parent. The number of ^{250}Fm counts corresponded to a cross section of a few microbarns for the reaction with ^{246}Cm. A chemical identification of these atoms as fermium was made twice by means of a cation elution column; in one case, two atoms, and in the other, nine were identified in the element-100 position. This work was published by T. Sikkeland, J. R. Walton, G. T. Seaborg, and me in July, 1958 (GHI58A).

After our milking experiments were completed we started the development of an apparatus that would enable us to determine the alpha energy of the element-102 isotope that we had found (GHI59). This was not an easy task since this was in the days before solid-state detectors became available. The only way open to us was the use of a gridded ion chamber. Normally these chambers were used at an elevated pressure of 90% argon/10% methane; this presented a difficulty since the electrostatic collection of charged nuclear recoils was accomplished in helium. After some experimentation we found that a very large chamber run at atmospheric pressure Ar/CH4 would work satisfactorily. A metallized Mylar tape was used to carry the collected recoil atoms from the target chamber to the inverted ion chamber directly below, gravity and small slits being the means to keep the two gasses from mixing. The system was fast and efficient and gave fair resolution but it was fraught with danger, as we were to find out in a matter of months.

With this equipment we were able to measure 7.8-MeV alpha particles from ^{248}Fm, the 30-s emitter found the previous year by the continuous milking of daughter ^{244}Cf in the conveyor belt experiments. We then set to work to try and measure the alpha energy of the 3-s parent that gave rise to ^{250}Fm. Immediately we ran into what turns out to be a perennial problem, the production of light element alpha emitters in the polonium region from tiny Pb impurities in the target. Careful purification of the target and the use of purified Ni as a target substrate reduced this background sufficiently for these experiments and we were able to proceed with the curium bombardments. We soon found a 3-second alpha emitter with an energy of 8.3 MeV and, to our great surprise, spontaneous fissions (SF) with an abundance of about 30%, if we assumed that the radiations all came from the same nuclide. Since the fissions were more abundant than we had expected by several orders of magnitude, we went to some trouble to assure ourselves that the big pulses were indeed caused by fissions and that the fission to alpha ratio did not change as we changed the bombarding energy. Since the half life was the same as that observed in the milking experiments, we quite naturally assumed that we were dealing with the same isotope and assigned the alpha particles and the SF to ^{254}No. Further runs undoubtedly would have brought to light a mass 252/254 discrepancy but, unfortunately, this work was abruptly halted by a terrifying incident in 1959.

In the beam line just in front of the curium targets there was interposed a thin double window of Ni foil cooled by the passage of helium. We had always assumed that if a window were to break it would be the one on the vacuum-isolating side because there was already one atmosphere of pressure on it; to guard against that eventuality we had installed a fast-closing valve to protect the HILAC. But we completely misjudged the situation. We did not realize that the window on the atmospheric side would become badly corroded by passage of the beam through it, possibly because traces of methane from the grid chamber had reached it over a period of time while the beam was present. The weakened foil was not able to stand an accidental overpressure and it explosively ruptured. The gas impinged on the fragile targets, which were only a centimeter away, and broke them up into micron-sized particles. A huge amount of helium poured through the target chamber carrying roughly 1012 alpha dis/min with it into the cave area!

Our second mistake was that there was no secondary containment to halt the spread of the radioactivity. The helium continued to flow for a period of a minute or two since it was not immediately obvious that a serious disaster had occurred. I was on the scene at the time and noticed that the counting rate of the activity in the grid chamber had risen enormously so I went out to the cave to see what had happened. I quickly found out! Horrified, I called the Health Chemistry monitor and told her that we had better evacuate the building. This was done very quickly and, fortunately, no one became contaminated. But we were very lucky. I doubt that even Madame Curie had to contend with spreading a hundreds of milligrams of radium all over her laboratory, for that is what the equivalent would be.

Starting the next day, every available person in the Health Chemistry Department went to work to clean up the "spill". The first step was to monitor where the activity had come to rest and cover it with sticky tape. After a few days the very large ceiling was covered almost completely with tape marked with the amount of activity under each piece. As I remember it now, the largest amount was of the order of 106 dis/min. The helium, being much lighter than air, had carried the tiny particles upward to the ceiling. The particles were tiny because that is the way they were deposited in the molecular deposition process by which they were made. By another miracle we were back in operation with the HILAC in three weeks; however, from then on the monitors were always on the lookout for hidden curium activity in obscure out-of-the-way places that were not easily available for checking. For the next ten years we continued to find traces of the residue.

This fearful accident disposed of all of the precious curium that we had up to that time. It also made us "gun-shy". Only gradually did we return to our routines in dealing with very alpha-active targets and then, only using elaborate precautions. We could have turned our attention to plutonium targets and oxygen projectiles to make element-102 isotopes but we knew that our beams would be weaker and the cross sections would be smaller. We decided instead to look for element 103 by using boron ions on the small amounts of californium that we now had. They were to be detected by a new tool, semiconductor alpha detectors.

In 1961 one of the results of the cross bombardments that we had to make in order to identify element 103 was that we discovered another isotope of element 102 (GHI61). This nuclide was an alpha emitter with an energy of 8.2 MeV and a T1/2 of approximately 15 seconds made by (11B,pxn) reactions with a mixture of californium isotopes (^{250}Cf, ^{251}Cf, and ^{252}Cf). At that time we ascribed it to ^{255}102 on the basis of comparative yields; however, there was no way that we could make an exact mass number assignment because of the several possibilities and eventually we found that it was actually 26-sec ^{257}102.

Work in the Soviet Union on Element 102

In the autumn of 1957 at the USSR Academy of Sciences I. V. Kurchatov Institute of Atomic Energy experiments to identify element 102 were started by G. N. Flerov et al using ^{241}Pu and ^{239}Pu targets with ^{16}O ions (FLE58). The internal beam of the 150-cm cyclotron was used with an aluminum catcher shuttled about 2 meters in 4-5 seconds to place the recoil activity next to a nuclear emulsion for alpha range measurements. Alpha

particles were observed with ranges corresponding to energies of more than 8.5 MeV. Later experiments (FLE58A) in 1958 reported 8.8 MeV alphas in ^{16}O on ^{241}Pu bombardments and these were attributed to possibly $^{253}102$ or $^{254}102$; however, it was necessary to make very large corrections for background due to Pb-produced alpha particles (mostly 25-s 8.87-Mev 211mPo). The poor resolution of the nuclear emulsion method and the high background from lead made it impossible to come to a conclusion as to whether element 102 had been produced or not.

The work was transferred to the relatively new Dubna laboratory and a new series of experiments was started. A successful culmination came in 1963 with the production and identification of a different isotope of element 102, the internal beam of the new 3-meter cyclotron being used to bombard ^{238}U with ^{22}Ne ions to produce $^{256}102$ (DON64). With a technique (See Fig. 12) based on the Berkeley double-recoil method they chemically identified ^{252}Fm, the alpha-emitting daughter, and deduced that the half life of the element-102 mother was about 8 s (A better value was measured later by our group to be 3.2 s (GHI67)). Their work was confirmed later by the production of the same nuclide by the $^{242}Pu(18O,4n)$ reaction (FLE67) using their version of the helium-jet method (ZAG66). Much later, after the Berkeley discovery that ^{250}Fm could be transferred by 250mFm (see below), experiments were conducted which excluded the remote possibility that there was also an isomeric transition in ^{252}Fm that might have caused its transfer in those experiments.

In December, 1965 Donets et al (DON65, DON66) identified $^{254}102$ by the double-recoil method and found, much to their surprise and later to ours, that the half life of the element-102 parent was not three but fifty seconds! By using other reactions they confirmed this result and went on to examine the characteristics of other isotopes of element 102 using the gas-jet technique that had just been pioneered by R. Macfarlane and myself. [The technique was discovered in unpublished experiments designed to make an isotope separator (GHI59) with a device that I called RAMA; Macfarlane was the first to publish its use in experiments (MAC63)].

The gas-jet technique was a method to transport recoil atoms from the target chamber to a place where they could be analyzed. In the Soviet version a stream of helium flowing rapidly through a capillary tube deposited the atoms onto a Maltese cross; this rotated periodically to place them next to a solid-state detector to measure half lives and energies accurately. Using this modern method and a new 1.5-meter cyclotron the Dubna experimenters identified the alpha particles from their $^{254}102$ isotope and found that they had an energy of 8.10 MeV. Measured half lives ranged from 20-50 seconds (ZAG66) to 7515 seconds (MIK67), values far different than [*RWS: "from"*] the three seconds claimed by our group.

They also looked for spontaneous fissions and did not find any (FLE67A, FLE68C), in complete contradiction with our work of 1959. Actually, they did see a 3-s SF activity in their experiment, but assumed that it was caused by some low-Z isomer because they also observed 3-s SF activity in a control experiment, $18O+ ^{233}U$, with about the same yield. I think that it is quite possible that what they measured in the "control" experiment was ^{233}U fission induced by its own delayed neutrons, a phenomenon that we discovered in our early work with the SF shape isomers. Another Dubna group identified $^{252}102$ as having an alpha energy of 8.41 MeV with a T1/2 of 4.51.5 s

(MIK67). Two other isotopes of element 102 were characterized so that by October, 1966 seemingly they were able to announce the complete identification of isotopes of element 102 from mass numbers 252 to 256 (AKA66, AKA67, DRU67, DUB67).

Our Response to the Soviet Work on Element 102

These results seemed to be at variance with all of the earlier Berkeley results so in November, 1966 we responded to their challenge by repeating our old experiments to clear up the discrepancies. This time we used a gas-jet system with a small wheel to convey atoms to positions next to a small array of solid-state detectors. In a very brief time we were able to produce and identify isotopes of element 102 from mass numbers 251 to 258 (GHI67, SIK68, NUR69), and a few years later mass number 259 (SIL73). We bombarded monoisotopic targets of ^{244}Cm, ^{246}Cm, and ^{248}Cm with projectiles of 12C and 13C. With the advantage of larger cross sections for our fusion reactions, we were able to make more definitive measurements than at Dubna.

The first observation that we made was that the half life of ^{254}102 was definitely not three seconds as we had claimed in our 1958 and 1959 papers; in the reaction, ^{246}Cm(12C,4n)^{254}102, we found 555 s in agreement with Dubna. Its production cross section was about a microbarn.

The next step in understanding the data was finding that ^{252}102 had a T1/2 of 2.30.3 s and an alpha energy of 8.410.02 MeV (Dubna: 4.51.5 sec, 8.410.03 MeV). We observed an SF-branching ratio of 30%, in excellent agreement with our grid chamber result of 1959. The grid chamber measurement of 8.3 MeV can be corrected to 8.4 MeV by using the modern value for the alpha energy of ^{248}Fm, which is about 100 keV higher than that measured in the grid chamber experiments.

The last piece of the jig-saw puzzle was uncovered during our work on element 104 a few years later. This was the discovery that there exists a 1.8-s isomer in ^{250}Fm, a low-lying isomeric transition (GHI73). It was made evident in those experiments by the transfer of the ground state, 30-min ^{250}Fm, from the wheel to the detectors, by the feeble recoil of the isomeric transition. It is likely that this effect played some role in our original element-102 experiments but it is not at all clear how prominent it was; the recoil energy from ^{250}Fm decaying to the ground state is very low so that the efficiency to transfer ^{250}Fm to the second collector would be small and highly variable.

It is not possible to be certain as to the reason why our original experiments gave the wrong half life for ^{254}102. One possible scenario is that the confusion was caused by ^{252}102. This would have been made by a (12C, 4n) reaction on the 20-times more abundant ^{244}Cm component of the target. It is quite possible that the granddaughter of ^{252}102, 20-m 7.22-MeV ^{244}Cf, was mistaken for 30-min 7.43-MeV ^{250}Fm in the conveyor belt experiments because we were plagued by problems of drift in the multi-chamber grid chamber system used to analyze the small amounts of activity in each experiment. The chemical milking experiments were not bothered because fermium was separated from californium.

The isotope, ^{257}102, was identified in our later experiments (GHI67) and found to have prominent groups at 8.22, 8.27, and 8.32 MeV with a T1/2 of 232 s. This was the ca 15-s 8.2-MeV alpha activity seen in 1961 in

connection with the element-103 experiments, correctly assigned to element 102 (GHI61), but mis-assigned to mass 255.

Beginning in 1967, the Dubna-discovered isotope, 3-min ^{255}No, was used in chemical experiments at Berkeley to show that in aqueous solutions nobelium only exhibits divalent characteristics (MAL67, SIL69, SIL74). At the time, this was an important and unexpected result; among other things, it shows that nobelium could not have eluted early from a cation exchange column as claimed in the Stockholm experiments (FIE57). Our 1967 experiments provided the first chemical evidence that element 102 was different from any other element. Using the same isotope, Dittner et al (DIT71) at Oak Ridge National Laboratory made a confirming atomic number identification by the elegant method of measuring alpha particles from ^{255}No in coincidence with ^{251}Fm K x-rays (See Fig. 13). Later still, the isotope, 1-h ^{259}No, was discovered (SIL73) at Berkeley so that it has been possible to conduct more extensive studies of nobelium chemistry (DOU83, DAV89).

SUMMARY

We believe that the following conclusions can be drawn:

a) The Berkeley group showed that the 1957 work at the Nobel Institute was completely in error, not only from a physics standpoint but also from a chemical standpoint.

b) The 1957 work at the Kurchatov Institute was inconclusive at best because of resolution and background problems.The results are also incompatible with presently-known data on element-102 isotopes (BRO86).

c) In 1958 at Berkeley, the isotope, 250mFm, was shown by chemical means to exist in a recoil-milked fraction. At that time we claimed that this proved the atomic number of the element-102 parent; in part this is true, but we now know that some of the ^{250}Fm was probably transferred by the tiny recoil from 1.8-s ^{250}Fm. From later work it is now known that granddaughter-^{244}Cf, coming from the alpha decay of ^{252}102, had to be present, also, in this fraction. Thus, there were two isotopes of element 102 present at the time, 55-s ^{254}102 and 2.3-s ^{252}102, a circumstance not recognized by us until the publication of the Dubna results caused us to re-examine our findings and correlate them with our later data.

d) In 1959 at Berkeley, the isotope, ^{252}102, was discovered as shown by its alpha energy and half life, and by its spontaneous fission branching decay. At that time the mass number 254 was assigned to this nuclide erroneously for understandable reasons.

e) In 1961 at Berkeley, during the element-103 experiments, the isotope, ^{257}102, was discovered as shown by its alpha energy and half life, although the mass number was mis-assigned to 255 at that time.

f) In 1963 at Dubna for the first time in their laboratory, the isotope, ^{256}102, was positively identified by the chemical milking of its recoil daughter, ^{252}Fm (DON64).

g) In 1966 at Berkeley, isotopes of element 102 from mass numbers 251 through 258 were produced with good statistics to lay to rest most of the uncertainties that had developed in energies, half lives, and mass assignments.

h) In 1967 the first chemical identification of element 102 was made in Berkeley using the 3-min ^{255}No isotope.

By the time this definitive work had been completed, ten years had passed since the original experiments at Stockholm and the name nobelium had come into common usage. In a review paper (GHI67A) on the element-102 research published in 1967, T. Sikkeland and I suggested that the name be retained; but the symbol, No, will always be a reminder of its somewhat painful history!

---

8. Element 103

False discovery of element 103 by Argonne

AG: Preface this with the incident wherein I was called back from TM to check on a …

9. Elements 104 and 105

Element 104--the bad Dubna experiments

Flerov claim

In Log Book #9 pp.14, 18 there is an account of a telephone call from Paris from Asaro about the disclosure by Flerov of their "discovery" of $^{260}104$

Our attempts to find their activity--futile

The beginning of the new era of the alphas

Element 104 finally discovered

Pat Somerville and his thesis re the element-104 Dubna work.

The search for the 0.3-s and 80-ms SF's

…which eventually led to our discovery of the 20-ms $^{260}104$ activity.

And then element 105

10. The Omnitron

The Omnitron--a great machine that never made it—

The first machine for which we proposed making radioactive beams

Seaborg

As Chairman of the AEC Glenn had a profound impact on the direction of nuclear science in the world. I remember how he caused us to invent the Omnitron by sending me a letter from Washington telling me of a conversation with John Huizenga wherein John mentioned that ANL was thinking of building a large HI cyclotron. After I invented the Bevalac with the help of Frank Self, Glenn was very instrumental in getting funds for it. He is still active in looking ahead as witness his fervent support for Mike Nitschke's radioactive-beam accelerator, ISL.

Original papers

AG: Unfortunately, I do not have any original papers which describe the genesis of the project; at least I can't remember that I ever wrote down anything. I will just have to reconstruct something using the big Omnitron book that was made for the budget people.

McMillan

What I do remember is who torpedoed the project. It was none other than Ed McMillan. He refused to go to bat for it, something that I was not aware of until long afterwards. It was ironical later when I invented the BevaLac and went to MacMillan and told him about the idea. His reaction was to clap his head and say that he should have thought of it. He could have made the difference in building the Omnitron, Glenn told me later. Without the support of the Lab Director there was only so much that Glenn could do even though he was the AEC boss. Unfortunately, I was never skilled at working behind the scenes to get something done and I was not even aware that MacMillan opposed the project. I was a real novice at this game. Even though I had invented the BevaLac I allowed Ed to decide that Ed Lofgren should manage the project entirely. He promptly froze Bob Main out of any role in the construction by sidelining him to a trivial job. Bob died within a few years, probably of a broken heart. What I should have done was to go to Glenn and ask him to intercede. That might have worked because Glenn controlled the purse strings entirely in getting the BevaLac started.

Consequences of not building it

The reason that the Omnitron was so crucial to the future was that this *was* the wave of the future. The Omnitron would have given us access to the heavier elements before any other laboratory by several years. Most importantly, our superheavy element program would have been supported with good funding and we would have done what GSI did more than ten years later, namely discover elements 107-112.

We would have gotten into the Ca-48 bombardments of the heavy nuclides very early on and that means that we would have discovered the SHE years before the Dubna people; however, at this point, early in 2005, it is not yet settled that they actually have found them. Their evidence looks to be impregnable except for one very important set of experiments that Ken

Gregorich et al have recently performed looking for the kingpin, 112-283. The 88-Inch group has not been able to reproduce this nuclide at all in what look like good experiments. On the other hand the Dubna/Livermore collaboration claims to have found 32 nuclides in Ca-48 bombardments of a lot of actinide targets, all with the same low picobarn cross sections. To me that in itself is suspicious. As a result as of March, 2005 I have come up with a novel proposal to build a much better separator using a superconducting magnet that would increase the yield at the cyclotron by as much as a factor of ten. I have not surfaced the idea yet to anyone except George Kalnins. He is going to look into it and soon I will bring it up to the others.

Because of the Omnitron I invented the BevaLac.

That machine led to the proposition that there was a quark-gluon plasma which had only been present at the creation of our universe. In turn, that led to the building of RHIC at Brookhaven, where in 2004, Berkeley scientists leading a big team with Star now believe that they have good evidence for the QGP.

10.1 The super-HILAC (1970-71)

…the fallback position in case we lost the Omnitron

12. Element 106

Element 106

A classical experiment, reproduced in 1993

The TransPlutonium Program Committee was set up a few years before this experiment was started and it was vital in getting all of the ^{249}Cf available at that time for us.

The story of "Seaborgium"

Last week Tues. 30 Nov. 1993 seven of the co-discoverers of the element team got together and officially decided to name it after Glenn!

About a month before I had gotten the idea, waking up during the night when the thought struck me very strongly that now was the perfect time to break the 50 year precedent that we ourselves had reinforced that governed the selection of names for newly-discovered elements. I felt that in the great panoply of names that had already been picked for the heavy element region, the name seaborgium would be of equal worth to curium, einsteinium, fermium, mendelevium, lawrencium, rutherfordium, hahnium, nielsbohrium, and meitnerium. Nobelium is an anomaly for unfortunate reasons that I will not explore at this moment. I was brought to this realization after our discovery was confirmed by the Gregorich/Hoffman Group's in a very nice experiment at the 88" cyclotron in October. Subsequent to this, I had been called by science reporters wanting to know what we were going to name it. At least two of them, George Petit and Malcolm Browne, suggested that we should call it ghiorsium! It was this sort of needling that led me to the idea of naming it seaborgium.

I decided to try the idea out on the other co-discoverers. A few days later I talked to Jose Alonso and was pleased to find that he also thought that it would be a great name. He had the same hesitation that I had, namely would Glenn accept it ? We both felt that the overriding consideration should be that we should not do anything that would embarrass Glenn. The next day Jose told me that Carol was very enthusiastic and I knew that we were on the right track. I talked to Wilma about it and she had no hesitation in saying that it would be a great name so encouraged by this initial reception I went ahead with gradually canvassing the other seven members of the team (excluding Glenn, of course).

Mike was not sure at first since he was involved in an aborted scheme hatched at GSI when he was there a couple of years ago. He and Armbruster were playing with names for all of the elements and Glenn's was in one of the permutations. He had talked with Glenn and found that he was against it at that time so he felt that it would not be acceptable now. I pointed out that it was a completely different situation than what we had at this time. He agreed that it was worth a try so I continued to try and build a consensus.

Matti Nurmia was my next contact, having just returned from Finland. He also agreed that it would be very appropriate and was all for it. I think at about this time I started to consult with Darleane Hoffman about the matter. There was no hesitation, she was all for it, too. I was surprised to hear from Sam Markowitz in a voicemail message who had independently come up with the same idea. He wrote up his thoughts and reasons and I received them in the mail a few days later.

Now that we had progressed this far I turned my thoughts toward when and how we should make the announcement. I knew that Ken Hulet was going to be honored at a San Diego ACS meeting on the occasion of receiving one of the ACS awards in March of 1994 and decided that this would be an excellent time since he was one of the co-discoverers. He could announce the name during his speech.

The next person to be contacted was Ron Lougheed. Again there was agreement and I felt that I had achieved a consensus, with the possible exception of Ken. I was pleasantly surprised; he was not opposed to the idea but he thought that Glenn would not accept it. He suggested that I ask him before we go any further in our search for a name. I said that I was willing to do so but that I thought that we should have a meeting of the seven of us and make sure that we had a unanimous agreement. I said that if I could report to Glenn that this was the case he would be much more willing to go along with it.

The meeting took place on Tuesday, 30 November at the HILAC. For the meeting I had prepared a loose-leaf notebook for each of us which was mostly copies of excerpts from the two log books concerned with the element-106 discovery in 1974 and the logbook in which we actually observed element 106 in 1970. I also included other pertinent documents along with the Physical Review Letters article that has been submitted on the confirmation work at the 88". Everyone was pleasantly surprised; it is a great memento.

Unfortunately, there was one mistake in the PRL article; in the list of references Ken G. had inadvertently left off the names of Hulet and Lougheed in the reference to the original discovery of 106. Ken H. almost immediately spotted this error and was very upset. I had not noticed the boo-boo since I don't often look at references until they are needed. It was an easy error to make since the Livermore contingent names were spatially separated from the Berkeley ones. It was an awkward moment for Ken was clearly distressed. Apparently, he was pissed off because Science had only referred to the Berkeley part of the original element-106 collaboration and did not mention LLNL. I smoothed the matter over as best I could but it was an unfortunate error.

We discussed the ground rules for picking a new name in case that Glenn would not accept our proposal. We decided not to pick an alternate because everyone agreed that Glenn should be involved. We then went around the circle of seven to make sure that we were unanimous in the big decision. Again the only dragging of feet was based on whether GTS would accept. Both Carol and I felt that he would do so because he took the long view of history. The name would last for a long, long time and it was right that his should be on the roster. There was no room for false modesty here. Jose's view was that I should not accept no for an answer.

The next step was now for me to approach Glenn. I had an extra day to think about it since the next day Wilma and I went out birding so I set up an appointment to see Glenn on Thursday after lunch. I decided to let him know of our decision in an unusual way. I made up a special cover page by hand for the "Element-106 Story" folder. As I remember it (I forgot to make a copy) the page said simply:

Dear Glenn, the members of the element-106 team had decided unanimously that the name seaborgium should join the distinguished roster of the heavy elements and then I named all of them from plutonium to meitnerium..

It looked great. Explaining how I came to put it together, I presented the folder to him and watched as he opened it to the first page.

He was clearly astonished! And pleased, too. I now felt confident that he would accept our decision. We talked for a little while about the big step and I told him how it had come about. He told me that now he understood why I had been evasive in my answers to his queries about how the naming procedure was progressing, especially when my normal attitude was very forthcoming. After the 88" confirmation we had discussed it and I had said that I was looking into the matter of the name; every now and then he would ask me about it and, unusual for me, I would make some non-committal remark. I told him that I had asked the opinions of Darleane and Paul Karol and I told him of the Markowitz independent suggestion. After about a half hour he said that he would like to think about the idea overnight and talk to Helen and he would let me know tomorrow about his decision.

The next morning I called him and asked him about Helen's reaction. He was jubilant and said that she was ecstatic. He said that he, too, was so excited that he had sat down last night and read the whole folder that I had put together. We talked a little in general terms and then he told me that this was the greatest honor that he had ever received in his life! Coming from him, this statement makes me feel great. We did the right thing.

I told him about how I had "sold" our collaborators on the idea, one-by-one until I had gotten to Ken Hulet. To clinch him that we should do it I had suggested that he should announce the name at the ceremony of his being awarded the Nuclear Applications Award in San Diego. He got a big kick out of my strategy and said that what I had done was "Machiavellian". Hulet did a beautiful job and I was very proud that I had picked him.

Add this somewhere: Some weeks after I told Glenn about the seaborgium incident he told me in a private meeting that "he thought that my naming element-106 after him was the greatest thing that I had ever done!" I could have kicked him but maybe he was right.

The name was not accepted at first by the official powers that be but the vast majority of scientists thought that it was a great idea and after some months of quiet campaigning by Glenn and others seaborgium was finally accepted by the establishment. I don't remember the details but I can probably get them in Glenn's writings so that I can use the complete story.

11. The BevaLac

How the BevaLac came to be invented

Proposal for the Bevalac

From the 1971 Nuclear Chemistry Annual Report P. 315

The "BevaLac-a Versatile Accelerator Concept"

A. Ghiorso

For many years the biomedical groups of the Lawrence Radiation Laboratory have been interested in a high-energy medium-heavy-ion accelerator which could be used for extremely important extensions of the field of nuclear medicine. Unfortunately, new high energy machines with the necessary capability are, of necessity, very costly, so that there has been a natural reluctance in the past to fund the projects that have been proposed for this purpose. The latest of these studies is for a synchrotron ring to accept particles from the SuperHILAC and accelerate them up to energies of several hundred MeV per nucleon. This project is feasible but suffers from the difficulties of cost (about six million dollars) and marginal utility. We have just recently come to the realization that the inexorable advance of accelerator technology now makes it possible to use the existing facilities at Berkeley in a novel manner to furnish the biomedical groups with ample beams of fast heavy particles at a very moderate cost without disturbing the normal operation of these facilities.

By combining the new SuperHILAC, which will be operating in 1972, with the pioneering high-energy accelerator, the Bevatron, we obtain at minimal cost a very powerful team that can undertake a number of important tasks. The missing ingredient for the combination to be successful is a booster to raise the velocity of the SuperHILAC ions to the point where they will match the velocity of the protons that are normally injected into the Bevatron. The other requirement, because of the relatively "poor" vacuum in the Bevatron, is that the ions be completely stripped of electrons; the booster makes this possible for the heavier atoms that we desire to inject into the Bevatron. It turns out that such a booster, another linear accelerator, can be very valuable in its own right for both biomedical purposes and nuclear chemistry-physics. The booster would be able to accelerate all of the ions emitted from the SuperHILAC to energies in the region of 50 MeV/nucleon, depending on the atomic number. Its duty cycle would be comparable to that of the SuperHILAC and it would have its own modest target area for those experiments requiring the intermediate energies.

The most logically site for the booster is adjacent to the SuperHILAC exit but ant an elevation about 30 ft lower. The linac would point toward the south to take advantage of the terrain and to get its exit beam closer to the Bevatron. A beam line for inflection of fully stripped 13-Mev/nucleon heavy ions into the Bevatron would be about 400 ft long; such a length is easily handled by about 20 quadrupole magnets similar to those used in the SuperHILAC itself.

The BevaLac will be able to accelerate heavy ions to energies as high as 2700 MeV/nucleon. The upper limit in Z that can be accelerated depends upon the ion velocity out of the booster. Detailed information as to the velocity for complete stripping is not yet known for atoms beyond argon, but it is expected that an appreciable current of 180-BeV Kr ions can be obtained. The beam current of ions such as neon could be as high as 1014 ions per second. With further modifications of the Bevatron and the booster it should be possible to accelerate U ions to 460 BeV.

The Bevatron can readily be adapted at modest cost to a time-share mode of operation so that the therapeutic use of the machine can be performed constantly. The SuperHILAC, by the addition of a third 800-KV injector, is already designed with this time-sharing capability. Since the time required to fill the Bevatron accelerating ring is only a few hundred microseconds there will be no problem furnishing a beam pulse whenever desired by the medical facility. After exploratory "tuning" work has been accomplished with the "BevaLac" it should be routine procedure that can be handled by computers.

The tremendous range of capabilities of the proposed combination uncovers possibilities far beyond its use for cancer research and therapy and has thus aroused the excitement of many in the nuclear chemistry-physics community. Clearly, a whole new field of theoretical and experimental science would open up rather quickly at minimal cost. The collective effects of the nucleons in a complex ion traveling at high velocity should be a rather interesting phenomenon; many scientists feel that these effects will not be simply those of individual nucleons traveling separately at the same speed. For the first time an exploratory effort will be possible.

The availability of high energy complex ions in the laboratory is very important for biomedical studies of the hazards that they may present in long spacecraft flights, and this is planned as a part of the biomedical research program.

This facility would also be very valuable to the research workers in the space sciences, since for the first time they would be able to use a versatile machine for calibration purposes. The exciting discovery of high energy uranium ions in the cosmic flux opens a new window looking out into the universe.

The concept that has been outlined briefly can be accomplished with great rapidity and at modest cost. It will interfere with either the construction or operation of the SuperHILAC-in fact, by judicious design we believe it may be possible to incorporate certain qualities in the proposed booster that will enable future changes in the present post-stripper so that even more efficient acceleration of the heaviest ions will become possible.

The joint additional operating costs of the new concept will be comparable to that expected for the originally-proposed synchrotron.

Footnote

This memorandum was sent by Albert Ghiorso to Edwin McMillan on 22 March 1971 to outline an inexpensive concept of combining the SuperHILAC with the Bevatron to enable the acceleration of heavy ions to high energies and thus achieve one of the goals of the ill-fated Omnitron project. As it turned out, the Bevatron is able to accept ions from the

SuperHILAC even without the booster that was suggested in the memo, and thus it will be possible to make the BevaLac operational late in 1973 if the funds for the transfer line between the two facilities become available as expected. To prove that the Bevatron could accelerate heavy ions efficiently, a crash program was undertaken in the summer of 1971 and was successful in accelerating to very high energies a small beam of nitrogen ions from its own injector.

Impact of the Bevalac

The most important impact of the BevaLac turned out to be in the field of high energy physics. Early on, probably after the successful acceleration of U238 ions to high energies, someone suggested the possibility of creating the quark/gluon plasma that must have existed when the universe was created by the "Big Bang". Though the physicists at the BevaLac tried hard their research was unsuccessful in making a quark/gluon plasma it did excite the curiosity of the scientific world and did lead directly to the building of RHIC in the conveniently empty trenches left by Isabel. And just recently it now appears that they have been successful in gold-gold collisions in observing this fundamental phenomenon. If I had not invented the Bevalac it would have been delayed at least 25 years. Explain.

The Bevelac upgrade

BevaLac upgrade 25 years after the Omnitron was very similar and also did not fly.

The BevaLac: was it a mistake?

In retrospect, I think that we and everybody else would have been better off if we had kept trying for the Omnitron, because eventually we would have gotten it. We might have lost a couple of years but we would have had a much better machine in that we would have been able to accelerate uranium ions years before anybody else as well as the medium weight elements. We would have been able to generate much more support both from inside and from outside. We would have probably built a fancy velocity selector like SHIP and found elements 107, 108, and 109!. The people who eventually joined the BevaLac would have migrated into low energy physics and would still be there today.

13. GSI and Elements 107, 108, and 109

The rise of GSI to prominence with the discoveries of elements 107, 109, and then 108

15. Element 110

The gas-filled spectrometers, SASSY1 and SASSY2.

I invented the gas-filled principle independently in 1959 at the HILAC. I found out later that a Russian scientist had proposed it a couple of years earlier but had never really made it work properly.

Building SASSY2

How Bill Ghiorso came to join the Lab

Bill's role in SASSY2.

AutoCad, computers, machine work, designs such as the mouse trap valve, the fast wheel and its driver, pulsed valve for blocking unwanted beam pulses, etc.

Saburo as a graduate student and Post-Doc--development of SASSY2

The element-110 experiment

15.1 Element 118

The Element 118 fiasco

It would have been named Ghiorsium according to Darleane Hoffman!

15.2 Other scientific activities

Development of a new procedure for making thin polyproplene films

The famous bottle of "Ghiorsium"

Given to GTS as a surprise Xmas? Gift.
Earl Hyde's citation.
AG: A good place to give Earl a nice tribute.

Search for double-beta decay with Charles Levine

Migma--a revolutionary way of producing power?

Contribution from Kalnins and Ghiorso.
Maybe include my early ramblings?
[RWS: I think this was a temporary interest of AG. It involved colliding ion beams, which he knew something about.]

Notes for the Explorer's Club talk of 28 Oct. 1994

[RWS: Albert was invited to talk to the Northern California Chapter of the Explorers Club by myself (Bob Schmieder). I picked him up at his house and drove us over to SF. The meeting was attended by about 30 members and guests. He was addressed as Dr. Ghiorso.

In the Notes for Memoirs, there is an entry "Notes for the Explorer's Club talk…" followed by what appears to be an outline. But it is not clear where the outline ends, or even whether it was an outline for a talk or merely another list of items AG wanted to organize.]

Student tours of the HILAC

…started by Bob Berges of Alameda High School

The honorary ScD degree at Gustavus Adolphus College

Spent the day with Charlie Townes, who was also getting an honorary degree; we had a lot of time to talk and we became good friends. Later Wilma and I often saw him and his wife at the Art Museum on the Campus at lunch. They were also bird watchers.

16. Nonscientific activities

Birding

Birding as a hobby

Photography.
First use of flash in the field?
Photographing the skylark

Art collecting

How Wilma and I got into the art collecting mood.

The Brenner-Parkinson collection.
Jazz
The Dawn Club

Doodles

19. Personalities

Glenn T. Seaborg

Notes for a short talk on the occasion of his 80th Birthday celebration at the lab on 16 April 1992.

I have been working with GTS for 50 years

That research centered around [RWS: "on"] the Trans-Pu elements. He was one of the charter members of the group that used chemistry as the tool for nuclear physics. He must be the most famous nuclear chemist in the world

As well as being an excellent scientist Glenn was an unusually good scientific director. He kept his eye on the ball but never tried to micro-manage. He picked his people carefully and then backed them to the limit. He knew where he wanted to go. He kept in contact with the workers and knew what was going on. As Joe Katz has said, if someone was not performing there soon would be some one at an adjacent bench who would be doing the same job.

He has been very successful as a teacher and has turned out about 65 PhD's. A high percentage of them have become leaders in nuclear science.

His memory is exceptional still and serves him well.

He works just as hard now as he has in the past. He is a master at working one-on-one with others and in a small group he can dominate it when he feels it necessary.

As Chairman of the AEC he had a profound impact on the direction of nuclear science in the world. I remember how he caused us to invent the Omnitron by sending me a letter from Washington telling me of a conversation with John Huizenga wherein John mentioned that ANL was thinking of building a large HI cyclotron. After I invented the Bevalac with the help of Frank Self, Glenn was very instrumental in getting funds for it. He is still active in looking ahead as witness his fervent support for Mike Nitschke's radioactive-beam accelerator, ISL.

He is interested in a wide variety of things, from science of all kinds to education to sports.

He has had a profound impact on the Lab and continues to be an important "mover and shaker". I suspect that he will continue to play a prominent role during the next decade.

Ken Silverstein's book "The Radioactive Boy Scout" is a sensationalized but fascinating account of a boy with a vivid imagination, one who at an early age decided that he wanted to earn an Eagle Scout badge by following in the footsteps of the great scientists who ushered in the nuclear age. Silverstein brings to life quite vividly the amazing story of David Hahn's untutored ramblings into the field of chemistry.

The boy was certainly an impressive character in terms of his determination and ingenuity but why was he so averse to learning? He was not a good student and that was the eventual cause of his downfall. A very simple understanding of whether his idea of making a "model breeder reactor" was even remotely possible would have shown him that with the materials available to him he would be short by a factor that I estimate would be much more than a billion billion. He seemed to have the notion that he

had a new idea that no one had thought of but that notion could only come about through ignorance and a lack of common sense. He apparently only read popular writings and his teachers never exposed him to scientific reasoning, otherwise he would have realized that his ideas were naïve in the extreme. But he did have some knowledge gained from reading and he was able to perform some chemical operations successfully without understanding the science of chemistry at all. He had the spark of curiosity that all good budding scientists have but, unfortunately, he had no one who could point the way to his getting the necessary fundamental education.

His story reminds me of my first foray into science in 1945 when I was a part of the team at the University of Chicago's Metallurgical Laboratory Project searching for unknown element 95. This is the same element that is used in millions of smoke detectors throughout the world and also the same one that was used by David Hahn in his futile attempt to make his fanciful "model breeder reactor".

The techniques that we had to use were also primitive because we were venturing into completely unknown territory only 25 years or so after the discovery of radium. I shared David's admiration for Marie Curie and her exploration into the new field of radioactivity and I read her scientific works with great interest. In those early days I hoped that I, too, could contribute to the new science by making a better measurement or interpretation of the data that she had uncovered. After all, though still primitive, our instrumentation was substantially more sensitive than that used by her. But that never happened and in the end I concluded that she was very smart and overcame her handicaps by careful reasoning and interpretation of her results.

At that time we did not yet have a simple way of measuring the energy of the alpha particles from the unknown activity that we had found in deuteron bombardments of plutonium. Ralph James and Tom Morgan, led by the great Glenn T. Seaborg, only knew that they had found an alpha emitter that had a different chemistry than any of the known elements. To really prove that its alpha particles were different than those emitted by plutonium we needed to measure their energy. But the amount of activity that we had made was miniscule in our first experiments, only tens of counts per minute, so that the known techniques were not sensitive enough to help us. I was the instrument man of the team so I had to invent some way of doing the job.

First, I went to the library and pored over what other physicists had done. They had used elegant methods for determining the range or energy of alpha particles at low geometry but always had the big advantage of having samples that were thousands or millions of times stronger than any available to us. After studying the literature for a while I finally came up with a very simple method to measure the range of the alpha particles with an efficiency that was roughly 5%. I simply covered the sample to be measured with thin mica sheets that had been weighed accurately. This combination was placed in an ionization chamber and any alpha particles that made it through the mica absorbers were counted. When enough mica absorber had been placed over the sample to completely absorb the alpha particles emitted by Pu^{239} only those from element-95 would come through in our highly purified sample. Tedious counting gave us a range curve and that is equivalent to energy.

This new method was radically simple and effective but it had to pass the scrutiny of my colleagues; David had nobody to whom he could turn who

could challenge his ideas. That was unfortunate because in the right environment the boy could have been steered into a more realistic scientific career.

It is difficult to know how serious the radiological hazard was that was generated by David's foray into science. It is clear that the monitors who arrived on the scene could not have observed any nuclear reaction involving neutrons because there could not have been any. What they did observe was probably the "fallout" from thoron the radioactive gaseous daughter in the long thorium decay chain, the thorium coming from the many mantles that had been combined to make his "reactor". Being a gas, thoron escapes quite readily and it could have been detected at a distance for when it decays, isotopes of lead, and bismuth are formed and these are easy to detect with a Geiger counter. This would explain why David's "reactor" was detected an alarming distance away from the potters shed although I suspect that the distance, "five doors down the block", was highly exaggerated. In any event David did realize that something was badly amiss and that prompted him to take corrective action. But he misinterpreted what had happened and thought that he had made a big breakthrough, that his dream of a new kind of reactor had come true. In reality, all that he had done was to create a "spill", a mild radioactive hazard.

20. Retirement

"Forced retirement" in 1981

The deal with Joe Cerny to keep him from assigning Mike Nitschke to run the 88" Cyclotron.

21. Legacy

The use of ^{241}Am in fire detectors

…more than paid for the cost of the heavy element research program, possibly. Contact the makers and try to get some figures.

[RWS: See separate file on Smoke Detectors obtained from web sources.]

Dear Eddie Sanchez: [1995]

[RWS: This appears to be a personal letter by AG, in which he refers to a previous letter. There is considerable personal information in this letter.]

Here is some more information, this time of a more personal nature. This is the first time that I have ever put together this sort of biography so it may not be very well organized but I think that it is reasonably accurate. I should tell you that your requests are impelling me to do something that perhaps I should have done years ago and, to tell you the truth, I enjoy it! It is a start on my autobiography which I will call "Adventures of an Alchemist".

I don't know very much about my father's personal history. One of his four brothers, Albert, for whom I was named, is still living at the age of about 90 but I have never quizzed him about the early days. I guess that I should do so. I do know that Papa was born in Genoa, Italy in the early 1880's and when his family emigrated to St. Helena, California he was about two years old. He grew up in that area until as a young adult he went to San Francisco where he worked as a cook and a taxi driver and a jack-of-all-trades as was common in those days. I do know that he was in the famous earthquake and fire of 1906 that devastated the City and lost all of his possessions. Included was the proof that he had been born there he was to tell people in the years that followed.

This was a common stratagem that many people used who lived in S. F. but forty two years later he told me that actually he had been born in Italy and had not gone through the technicalities of the naturalization process! He disclosed this to me when I, as a voter registration volunteer during the presidential campaign of 1948, tried to re-register him; I had to officially report that fact to the local government. It was a serious though technical violation of the laws because he had voted illegally in every election after he reached the age of 21. During my early years I had always written down in any documents that I had to fill out that my father had been born in S. F. He was never prosecuted for this technical violation of the law because he died a short time after I tried to register him.

After he left St. Helena he learned the trade of a riveter and worked in shipyards repairing vessels. My mother was born in Watsonville, California and her folks came from Spain. She married my father when quite young and bore seven children, two of which died in infancy. The third of the five children that survived, I was born on 15 July, 1915, in Vallejo, California. I had three sisters, one being younger than I, and all still survive. My brother, the last born in the family, died a few years ago.

When I was about two years old we moved to Alameda, California and this is where I spent the first 26 years of my life in the same old two-story house that was constantly being repaired or altered by my father. Like many men of the time he was able to do anything and had acquired many skills by necessity. By and large our family was quite poor and suffered greatly during the boom times of the early 20's and the depression times of the 30's. My father was often out of work and had a hard time providing for the family. He never went beyond the fourth grade because he had to go to work to help support the large family of his father but he became self-educated,

nonetheless, and strove to better the lot of his own family later on. Because he had never received an education he was very sensitive to the enormous value of obtaining one; he was always pressing us children on the necessity and virtues of acquiring learning especially when times were hard and life was difficult.

There was a time in the early 30's when he became a bootlegger to keep the family afloat, even serving a short term in the county jail because of it! In this era of prohibition it was not at all unusual for people to indulge in this small-time occupation to support themselves. I was baptized a Catholic (because my mother was very religious) and went to Sunday school, so I did not like the fact that my father violated the law of the land. The very unpopular Prohibition Law promoted much lawlessness leading to gangster-ridden illicit liquor enterprises; it was finally repealed in 1933. Papa was very outspoken about the economic injustices that were (and still are) all too common in our social system. He wanted me to become a lawyer, an honest one, and help right the wrongs that he saw all around him.

I failed him in that regard, I guess, by becoming a scientist instead of a lawyer. I certainly don't regret that because I would not have made a great lawyer although I still share his criticisms of our society. I feel a great revulsion toward the present attempt by the Republicans and, unfortunately, a large number of the Democrats (Republicrats) to push our country back to the "good old days" when the greedy capitalist robber barons ruled the land. I feel very strongly that Newt Gingrich's "Contract With America" should be more properly labeled "Contract **On** America." The present process in Congress is a thinly-veiled operation to transfer ever more of the wealth of the nation from those who have little to those who already have more than they need or deserve. They are attempting to overturn all of the gains made by Franklin D. Roosevelt's New Deal and to do this they are using the old tactic of divide and conquer to fool enough of the people to go along with their schemes. The age-old trick of promoting scapegoats by bashing the poor and those who are different is being sold to the country and seems to be working once again. When will we ever learn and get our priorities straight?

As far as I am concerned, the most important goal of our society should be that of caring for the children. They are our future and we must be careful that all of them, rich or poor, should have adequate opportunities to become productive citizens. That means that if our so-called "free" enterprise system cannot provide employment for their fathers and mothers then our government has the obligation to do so. It is the employer of last resort. It also means that our educational system must be effective. To do that we must attract dedicated teachers by giving them adequate compensation. When I was a child the teacher was near the top of the relative pay scale rather than near the bottom as it is now in some places. Living through this era as a young man was very instructive to me and I will never forget the lessons learned in this bitter period. Our country was very close to a violent revolution during this period.

The other important priority that I am concerned with is that of our earth. We have to stop trashing it since it is the only world that we will ever have. Ecology should be the first and most important word that a young person learns. Understanding the relationships of one thing to another in an unbroken chain is critical to our learning how to live in a world with limited assets. It is extremely foolish to run a society to maximize only short-term

profits; we must take the long view and understand how what we do now affects our future. Our fragile universe is a marvelous thing to behold. It is a much better turn-on than any drug could possibly be.

As a child I was an experimentalist from the start. Our neighborhood was very sparse with new houses being built all around us so I had odds and ends of building materials such as wooden shingles to play with. I remember putting together toy houses, living a fanciful life. Later when I learned to use the local library I discovered another treasured storehouse of ancient lore and consumed enormous numbers of books of all sorts. This lust for reading was to help me immensely in the future and enable me to escape from the poverty in which I lived in the present.

When my grandfather and his large family emigrated to the United States they settled on a small ranch in the wine country in the Napa area. It was a fairly primitive operation, barely a subsistence farming venture. As a child I used to spend my summer vacations up there in the hills of St. Helena. My grandparents spoke nothing but Italian so here was a great opportunity to learn another language. Unfortunately for me, I "blew it" and refused to do so. I wanted only to be an American! This was a very stupid thing to do, of course, but it was a common attitude of the times. It is very clear now that a multi-lingual capability is a great asset in this ever-changing world.

I did fairly well in school and liked all of my subjects. I was particularly interested in history and marveled at the great world around us. When I realized that most people were not of the Christian faith I lost my blind acceptance of the dogmas of the Catholic Church and became an agnostic, i.e., one who has not decided whether there is an all-knowing God or not. Most importantly however, when I rid myself of organized religion I did not throw out the Christian ethics that are an integral part of it. That was a very vital part of the religious lessons that I retained from my earliest days and I appreciate it very dearly. I now consider myself to be a "christian atheist" (with a small "c" and a small "a") since I see no need for a personal God, but I am completely tolerant of other people's beliefs and I deeply appreciate the great art and music of the Church.

I learned as a child what it was like to be a member of a minority group, being variously pigeon-holed as a "dago" or a "wop". I did not appreciate this classification, of course, and reacted in the usual way of finding my own scapegoat in the pecking order. This turned out to be a Negro boy of my age, the only one in the neighborhood. I looked down on him but never dreamed of doing anything overtly to make him unhappy. It was not until a decade later that I realized that my attitude was racist and thus not much better than that of people in the southern states where racism flourished and which I condemned. It is an excellent example of how the insidious hatred that pervades this country and probably all countries is generated. Any insecurity produces the reaction that one has to ascend the pecking order by stepping on others. It is tragic, especially when one considers that we are all members of the same family. Modern science has come to a consensus that life started in Africa, probably from only a few cells, so it seems clear to me that we are all brothers and sisters under the skin!

Since I lived in Alameda right across San Leandro Bay from the fledgling Oakland Airport, I became interested in aviation. Very often I used to ride my bike over to the place and marvel at the flying machines. I became very proficient at making model gliders and rubber-powered airplanes and so

learned something about the art of flying. It became the consuming passion of my life and early on I decided that my future should be that of an aeronautical engineer. I remember joining the hundreds of thousand of people who greeted Col. Charles A. Lindbergh in his silver high-wing monoplane, the Spirit of St. Louis, when he arrived on his barnstorming tour of the US. following his pioneering flight across the Atlantic Ocean in 1927. He was everybody's hero for a while; a dozen years later when he became an "America First" puppet his star had declined to the point where he was considered just another member of the wealthy upper class.

When I was about 12 years old I encountered the "art" of smoking. A neighborhood boy (slightly younger than I as I remember) who had poliomyelitis was given cigarettes regularly by his mother for some reason or other. Occasionally he would give me one to smoke. In a very short order I became addicted, too. To supplement the few that I could cadge from my friend I would pick up cigarette butts wherever I could find them and very soon I was completely hooked by this insidious drug. If this had been allowed to continue I would long ago have died of lung cancer I am sure. My luck held out though and in less than six months from the time that I started to smoke my big sister caught me in the act. I don't remember what she told me (nor does she) but whatever it was that she said, it was effective for I never smoked again in the 68 years since that time. I was very fortunate to say the least to have a big sister whom I admired and respected!

I became interested in athletics at an early age, in particular, baseball and tumbling. These two sports were very good for me because I learned how to coordinate my body movements. And of course it was fun to follow the teams on the radio and even occasionally to actually go to a game. I remember my father taking me to see the great Babe Ruth and Lou Gehrig when they came through Oakland as part of a tour throughout the country. It was a thrill to see the Babe hit four balls out of the park, one after the other! When I played I usually played catcher and for a little kid I did fairly well.

I was not drawn to science in general until I entered Alameda High School. There I was lucky in having two excellent teachers, one in chemistry and the other in physics, who taught their courses in a fashion similar to that used at the nearby University of California. When I entered Cal in 1933 and took freshman chemistry and physics I was completely at home with the courses and did reasonably well. These early science courses were seminal and aroused in me the creative instincts to marvel at the world around us. I feel strongly that the function of early schooling is to awaken the innate curiosity that everyone has.

I graduated from high school in 1932 second in my class; but, in the midst of the Depression, there was no money to send me to the University. Through the intercession of my big sister (four years older than I) I was given a small scholarship that took care of a few of the costs of commuting daily from Alameda to Berkeley by public transit. I enrolled in courses that were necessary for an engineering education rather than my original intention of majoring in history or some such academic subject and thereby hangs a tale.

In the interval between high school and college I could not find a job so I decided to explore the new field of radio. My father had been a tinkerer of the medium; although he did not really understand any of the underlying principles that made radio possible he was curious and learned how to make

some equipment work. He had accumulated a fair number of parts and it is these that I concentrated on. It soon became clear to me that my knowledge was woefully inadequate to do anything meaningful with the materials at hand and I set about rectifying that deficiency. I went down to the local library in Alameda and looked through their selection of appropriate books that might be helpful. I was very lucky in finding a book called "Letters from a Radio Engineer to his Son". The Engineer had outlined some 15 experiments that could be performed with simple pieces of equipment that would demonstrate fundamental principles of electricity and radio and I proceeded to do them, one by one.

I was completely captivated by what one could do and it opened up a whole new world to me. For instance, I found that I could make a carbon microphone that was sensitive enough to pick up the sound of a fly walking. There were a lot of different types of experiments that I performed and from each I learned a lot of new things that challenged me to learn more. I had entered the field of the experimenter and I was never to leave it. I wish that I could remember the name of the author of the book. The book would be old fashioned by present day standards and it is undoubtedly out of print but it played a very important role in my career. The net result of this encounter was that I changed my major from Liberal Arts to Electrical Engineering when I entered Cal as a freshman in 1933.

I did reasonably well, getting A's and B's with a few C's in unimportant subjects, but had to work very hard to do so. In between semesters I worked, when possible, in the shipyards to earn enough money to continue my schooling. That was not without hazard, however. One year I had a keg of rivets fall on my foot, a very painful injury that kept me off the job for two weeks. When I first started working in the shipyards I was assigned to be part of a riveting gang. My job was that of the "passer" the one who put the rivets in place. The rivets were heated to a white heat in an air-blown coke fire and then thrown to me. I was supposed to catch them in an iron cone that I held in my outstretched hand and then use long tongs to place them quickly in previously-drilled holes. The rivets would then be flattened with a riveting gun on one side and a "holder" held by a second person on the other side. Once I got the hang of it that was not too hard a task to perform. The big problem was that on this first job I was inside a 10-foot diameter tank that was perhaps 50 feet long and there were four gangs working simultaneously! The noise was literally deafening as I found out at the end of the day when I could not hear anything at all! Fortunately, my hearing recovered by the next day and I put cotton in my ears after that to protect them.

Occasionally, we would work in the bottom of a double-bottomed tanker and there we encountered residual oil that was burned by the rivets. Although we had big blower fans to get rid of the noxious fumes these protections were not very effective. My experiences were not unusual; the employers simply were not interested in providing a decent environment for the people who did their work because it cost them money and time. They knew that if you did not like to work under the conditions that they set they could always hire someone else! The work did not pay very well, either, since my wage was only 35 cents/hour! Fortunately, there was a big strike by the entire shipyard work force which was successful and my wage went up to 95 cents/hour.

Many present day managers would like to return to these "good old days" when the only driving force was the desire to make a profit regardless of the

cost to the people who did the work or to the environment in which they worked. It is a very short-sighted attitude which in the long run is very costly to everyone. This is a very important motivation for NAFTA (North American Free Trade Agreement) and GAT (General Agreement on Trade) and the reason why so many commercial operations have taken flight to other countries where the cost of labor is more than ten time less than in the US.

Well, Eddie, there is a lot more material. I am really curious as to what you are going to do with this and the previous things that I have already sent to you. Let me have a copy of whatever you produce and also something about yourself, your goals, and your aspirations.

The best of luck on your venture.

Sincerely,

Albert Ghiorso 26 Mar. 1995

STATUS OF SASSY2 AND THE ELEMENT-110 EXPERIMENT

Albert Ghiorso

ABSTRACT

After almost four years of construction and development the double-focusing gas-filled recoil spectrometer, SASSY2, has reached the stage where low cross section experiments can be undertaken. This status review is for the purpose of showing the progress that has been made in preparing the device for experimental use at the SuperHILAC and for acquainting other scientists with its capabilities. The first sections are devoted to a description of the origins, construction, and development of the separator. Methods of testing and calibration are then discussed with particular attention to some of the problems peculiar to SASSY2. The important question of the overall efficiency of the device is addressed and it is shown that its yield is very high, probably greater than 50%. Some experimental results are illustrated which indicate that the spectrometer performs quite well. Although ultra low level experiments could be undertaken with the present system, it is shown that two important improvements can be made during the fall shutdown that will make the discrimation of rare events even more trustworthy. Finally, predictions relative to the ^{59}Co bombardment of ^{209}Bi to make ^{267}110 are discussed. It is shown that there is a very reasonable chance of observing this unknown element with a bombardment time of only two or three weeks, a period substantially less that used by GSI for their work on element 109 in 1982 and 1988, even though the cross section for producing element 110 may be lower by an order of magnitude.

ORIGINS

In 1984 a new kind of natural radioactivity was discovered at Oxford University, the emission of 14C particles from ^{223}Ac. It was decided to try and confirm this very interesting effect and after some months of effort Saburo Yashita and I did manage to do so by operating the original SASSY1 in vacuum in a backward regime, i.e., source-quadruple-dipole rather than the other way around. To accomplish this rather ambitious task we used the ray-tracing program, BELIN, to determine the best optical configuration of the spectrometer for this particular experiment.

In the process of making a successful confirmation I decided that we could build a much better gas-filled recoil separator that would get around the two serious handicaps that I had found to be inherent in the design of SASSY1; these were insufficient magnetic resolution and an overly large focal plane image when used for research in the heavy element region. After a few months with BELIN we came up with a DQD combination which overcame these problems; in this solution both dipoles had large quadrupole gradients to enable us to keep the total length to a minimum. This concept became SASSY2. (See Figs.1 and 2)

CONSTRUCTION AND DEVELOPMENT

The instrument was gradually put together from bits and pieces in a "do-it-yourself" manner. The biggest jobs were the milling of the pole faces and the construction of the magnet vacuum chambers, but there were

innumerable other parts that had to be designed and built. By January, 1986 the first tests had been made with alpha particles. The results were encouraging but gave only a limited amount of information so it was decided to make "floating-wire" magnetic measurements.

This is an old technique in which a very thin wire carrying a current is levitated in a magnetic field to determine trajectories; however, in this case, the dipoles had strong quadrupole gradients so that there was no stable vertical position for the wire to take. This dilemma was solved by using restraining knife edges that kept the wire at midplane without introducing any appreciable friction.

A lot of new information was gleaned by this method but it was not possible to determine any details of possible aberrations caused by inaccurate milling of the pole pieces or by interactions of the closely spaced magnets. It seemed prudent to make magnetic measurements all along the central trajectory. Unfortunately, after trying for several weeks, it turned out to be impossible to do the job because of the restricted space available so the effort was abandoned.

It was decided then to use ion beams from the SuperHILAC to make dynamic measurements of the optical characteristics of the separator. On 8 April, 1986 an 8.5 Mev/A (1H) beam was tuned all the way through SASSY2 and found to behave as expected. This beam had a rigidity of only 8.4 kG-m though, whereas our goal was 21, the expected value for element 267110 produced in the 59Co on 209Bi reaction. The important test still remained to show that 21 kG-m could be reached before the onset of saturation.

It was decided to try out recoils themselves in the meantime to see what other kinds of problems there might be. And there were lots of them!

There was a serious window problem. Since the separator operates at a pressure of 1 torr of helium there has to be a window to isolate it from the beam line where the pressure is in the micro-torr region and this window has to be extremely thin to keep beam heating and scattering to a minimum. This problem had not been completely solved yet and some more effort had to be devoted to it.

The focal plane detector being developed by Jack Walton of the Crystal Development Group at LBL was not yet ready for routine operation. The plan was to use five adjacent 30mm x 30mm silicon wafers divided in front into 3-mm slices to give fifty horizontal positions. Each wafer would have a resistive back to give vertical position for each group of 10. Encouraging results had been obtained but it was difficult to make noise-free detectors in a reproducible fashion. The new electronic system for processing the many signals had severe growing pains. The computer software system, though usable, was in a primitive state.

Somehow the system worked well enough to show that we were on the right track, but soon we were confronted by a new unexpected difficulty which was temporarily beyond our control. We were trying to run as a satellite user with ^{40}Ar while ^{238}U ions were being accelerated to the main experimenter when suddenly we found that a flood of particles was coming all the way through the separator from the other beam! It was not sufficient just to gate out the other beam electronically because the intensity was high enough to risk permanent damage to the focal plane detectors. Although we tried various stratagems to get rid of this beam we were never successful since there was an energy continuum of ^{238}U particles scattered through the

septum switching magnet that serves our beam line. That made it impossible for any more tests to be conducted and a couple of weeks later the accelerator went into a five month summer shutdown.

During the summer of 1986 further tests were made with alpha particles which showed that magnet alignment was very critical. This can be understood when it is realized that the instrument is basically three quadrupoles in series. A new check was made with the floating wire system and it was found that it would be necessary to move the magnets beyond the constraints of the mechanical couplings between the units. New flexible vacuum joints had to be invented to allow these adjustments to be made, a process that took several weeks. To reduce saturation effects in the first dipole, small iron shims were placed under the pole tips to increase the magnetic field for a given current. When the SuperHILAC came back "on the air" in January 1987 the new alignment was checked dynamically by using a TV camera to view a fluorescent screen at the focal plane. The instrument now was found to be very well aligned, a condition in agreement with new floating wire data.

Permanently-mounted Hall probes were installed in each magnet to allow better reproducibility of the magnetic field settings. Also the very complicated drive circuitry for the synchronous target wheel was completed at this time and a satisfactory test was made with the wheel rotating in the fringe field of the first dipole while carrying a set of thin aluminum foils.

TESTING WITH FUSION RECOILS

As soon as the separator had been realigned, tests were made with evaporation residue recoils (EVRs) coming from the reaction of 40Ar on either 175Lu or 165Ho to produce either Ac or At isotopes. The yields were found to be more than an order of magnitude greater than had been observed with the original SASSY1. The system was still not ready for low cross section experiments, however, because the new electronic system had not yet been completed and we did not have a complete set of detectors.

At this point (April, 1987) an appeal was made for funds to expedite the completion of SASSY2. Fortunately, about 50K$ without overhead was made available and this was immediately put to good use, most of it for electronic fabrication. A very important change was made in the quadrupole magnet to allow it to tune without saturation to the 100 kG/m gradient needed for focusing element-110 recoils. This was done in place by installing new pole tips to reduce its aperture from 8 to 6.5 inches. A new chamber was constructed for fixed target operation, the design allowing for a quick selection from twelve targets. It also had a wheel holding seven thin windows that could be inserted into position in front of the target position as necessary.

The next several months were spent in debugging the new multiplex electronic system. This turned out to be a very difficult process but by dint of great effort it was slowly made into a tractable unit. A full complement of detectors was assembled, tested, and found to be suitable for the job.

Using satellite beam time a number of heavy ion beams with different rigidities were tuned through SASSY2. The function Bvs magnetic field was found to be quite linear, as expected. It was a great moment when we tuned 8.5-Mev/A 98Mo with q=20+ to the center of the focal plane because it is an

ion with a rigidity of 20.6 kG-m, the predicted value for element-110 recoils made in the ^{59}Co on ^{209}Bi reaction.

Early on it was determined that the optimum pressure for the separator was about one torr of helium. The FWHM of the B peak was found to be 5% of B. The magnetic dispersion was 0.37% per detector crystal which is 0.123% per horizontal millimeter of the focal plane.

During this period we continued to be plagued by the beams from other experimenters getting into our beam line. This problem was finally solved in an ingeniously simple way by Bill Ghiorso's invention of a very fast mechanical shutter that opens on demand; the electronics that drives the device was designed so that it can handle any pulse rate automatically. This rather inexpensive solution saved the tens of thousands of dollars that would have been required to install the equivalent, a fast pulsed magnet and its power supply, to do the same job.

As SASSY2 progressed through the testing stage more bugs continued to be found and new solutions continued to be applied. Gradually the crosstalk and noise problems were brought under control so that now the energy resolution of the silicon detectors is approaching the limit set by the detectors themselves, approximately 30 keV.

There clearly are places in SASSY2 where improvements can be made to better define the data that can be taken, for instance, a Time-of-Flight detector (a very difficult item to make for a gas-filled system) and a dE/dZ (rate-of-energy loss) counter. An attempt will be made during the shutdown to add these devices. There are other important facets of the system that could be improved but these must be postponed, for we are very close to the point where it should be possible to mount a major experiment requiring high sensitivity and zero background.

CALIBRATIONS OF THE INSTRUMENT

The path of a recoil ion through SASSY2 is described by its magnetic rigidity, B. B is equal to the quantity Mv divided by q-, where B is the magnetic field in kilogauss, is the turning radius in that field, M is the mass of the ion, v is its velocity, and q- is its mean charge state. The quantity q- is a complicated function of the atomic number and velocity of the recoil, and the atomic number and pressure of the gaseous medium through which it is passing. For a given particle and gas, B is roughly constant because the v over q- ratio changes very slowly as v changes.

It is not possible as yet to calculate with the required accuracy the value of B for any given set of conditions, therefore it is necessary to calibrate the separator over a wide range in atomic number and extrapolate to unknown nuclides. This has been done for the original SASSY1 so only a small number of calibration points are necessary for SASSY2. A convenient way of accomplishing this is to scatter elastically various target atoms at 0 degrees by using medium weight projectiles such as ^{40}Ar at the appropriate energy. In this manner a suitable calibration can be obtained for recoils as high as element 98. For elements beyond this it is necessary to use radioactive fusion products such as ^{254}No made by the ^{48}Ca on ^{208}Pb reaction. Fig.3 shows B/A data taken with SASSY1. The extrapolation to element 110 should be accurate to a few percent and since the dispersion in B is of the order of 15% it is felt that this accuracy is sufficient to accommodate any reasonable variation from predictions.

An interesting problem is the energy calibration of the focal plane detectors. It is not sufficient to make a calibration using external sources because when an atom is implanted into a silicon detector one measures not only the energy of the alpha particle when it decays inside the crystal but also, summed with this, the energy of the recoiling daughter nucleus. This energy will vary from 100 to 200 keV depending on the energies and masses involved. It will also vary depending on the Fano factor, i.e., the extent to which recombination reduces the size of the recoil part of the pulse.

As a result of this effect it is necessary to calibrate the energy scale by implanting alpha emitters with known energies. Such implantation is most easily done by bombarding rare earth targets with medium weight ions to make alpha emitters in the Pb-Ac region. A typical alpha particle spectrum, observed in the 40Ar on ^{175}Lu reaction, is shown in Fig.4a. Below that in Fig. 4b is shown the gross recoil energy spectrum.

Position measurements are extremely important for SASSY2 because they add another dimension in discrimination by allowing one to correlate a series of events with one another, e.g., recoil-alpha-alpha-etc., with a high precision. A good example of this technique is shown in Fig.5 in the above bombardment. The four recoil energy spectra on the right correspond to the implantation of the various nuclei represented by the four prominent lines in the previous alpha decay spectrum. The four alpha lines on the left show how the selection was made. We have not had time to fully analyze these results yet because of the press of other problems but certain obvious conclusions can be drawn.

The highest energy peak comes from isotope(s) of Ac in reactions wherein only neutrons are evaporated; the EVR recoils that deliver these atoms to the focal plane thus will have fusion recoil energies given by the mass ratio times the bombarding energy since the effect of the neutron evaporation is small. The lower energy alpha peaks come from isotopes of Ra, Fr, and At; these atoms are made in various reactions wherein protons, alphas, and neutrons are also evaporated simultaneously and this implies that the recoil energy will vary depending on the mass, energy, and direction of the particle(s) emitted in each particular case. Preliminary calculations agree with this reasoning but more analysis of the data has to be performed.

At a low enough counting rate or with a short enough half life, the correlation of one event with another can be done crystal by crystal, but in favorable cases position identification can add another two orders of magnitude in certainty. When an alpha particle escapes from a crystal at an angle normal to its face it leaves behind only about half an MeV, depending on its energy. This energy is low, and thus the position signal to noise ratio is poor; however, though the position information is minimal it is still useful. We have performed experiments with masks to check the accuracy of our position-measuring techniques and have also made recoil-alpha correlation maps. Both methods indicate that vertical positions are being measured to within a few tenths of a mm (the detectors are 28 mm high).

Spontaneous-fission emitters, when implanted, are also a special problem in that both fragments are almost always totally absorbed in the crystals because of their relatively short range; thus one typically measures a value close to the total energy, rather than the separate fragment energies as in conventional experiments.

EFFICIENCY OF THE SEPARATOR

The absolute efficiency or yield of SASSY2 is not easily determined. Ideally, one compares the recoil alpha activity which is implanted in the focal plane detector with the same recoil activity that is caught in a foil placed next to the target. This foil is removed and measured in an external counter with known geometry. Unfortunately, with commonly available projectiles there is no unambiguous (HI,xn) fusion product in the heavy element region that has a half-life long enough to do this. The 1-minute nuclide ^{254}No is ideal for this purpose but it can be produced as a fast fusion recoil only by ^{48}Ca bombardment of ^{208}Pb. This isotope is very distinctive in that it has an easily recognized daughter, 30-minute 250Fm, and its production cross section is several microbarns. The only problem is that ^{48}Ca is a very expensive projectile which is reserved for special experiments; at the proper time a yield experiment on SASSY2 will fit into that category. When this experiment is performed we will have an absolute cross section for that reaction and thus be able to reevaluate old data obtained at three laboratories, Berkeley, Darmstadt, and DUBNA for certain important experiments of the past.

The indirect evidence that we have collected so far indicates that the yield through the separator is rather high. This statement is based on the observation that the focal plane image obtained with 56Fe reactions with rare earth targets is almost entirely contained within the vertical confines of the detector crystals. Simultaneously, the horizontal width of this image occupies less than 30 crystals (each is 3 mm wide). Since the dead area or opacity of the crystals due to borders is only a few percent of the active area, we deduce that the transmission through SASSY2 must be very high; however, since it is possible that the magnetic configuration could effectively collimate the recoils that reach the focal plane and thus reduce the angular acceptance of SASSY2, an independent measurement is desirable to show that this is not the case, at least as far as EVRs are concerned.

EXPERIMENTAL PROGRAM

Although this instrument was constructed with the goal of using it to find element 110 it is clear that it will also be useful in looking at other regions that have not been explored yet. Since the problems are similar, the case of element 110 will be examined in detail as an illustration of the capabilities of the device.

The reaction that we have chosen to make element 110 is the bombardment of ^{209}Bi with ^{59}Co. This choice is dictated by the practical consideration that the projectile is readily available as a 100% abundant isotope. The competing projectile, ^{64}Ni, used to bombard ^{208}Pb, is only 0.9% abundant and thus is extremely expensive. An important consideration may turn out to be the number of neutrons in the product nucleus since, beyond 157 neutrons, SF appears to be dominant for some reason that is not yet clear. It could be simply that alpha half-lives continue to increase with neutron number while spontaneous fission half-lives decrease or stay constant. The isotope, ^{267}110, that will be produced with 59Co has 157 neutrons, a particularly stable configuration, whereas the isotope, ^{271}110,produced in the ^{64}Ni case has 161 neutrons. Judging by the example of lower-Z nuclides it is quite possible that its spontaneous fission half-life could be substantially shorter than its alpha half-life. The non-specificity of

the fission process together with a small alpha branching ratio thus would make genetic identification essentially impossible because of the small production cross section to make this nuclide.

As will be shown later, for this experiment to be feasible we will require beam currents of the order of 500 particle nanoamperes of 59Co delivered to targets with great realiability over a period of several weeks. To see whether this is a reasonable requirement a test was made using the ABEL injector to determine how much beam could be tuned to the SASSY2 target. We were gratified to find that indeed the SuperHILAC could reach this goal; a similar test with the ADAM injector showed that the best beam current that could be accelerated was several times lower in intensity. It is abundantly clear that the element-110 experiment will require the dedicated services of ABEL. The separate question as to whether the beam window can handle such a large current over a long period of time has not yet been addressed.

The function of the separator is to isolate the desired, but always minuscule, EVRs from other particles which are always made with abundances many orders of magnitude greater. These include the bombarding beam, elastically scattered target atoms, deep inelastic fragments, and fusion-fission products. In Fig.6 are shown the calculated relative intensities of these particles as a function of magnetic rigidity (a), flight time through 50 cm (b), dE/dZ ©, and kinetic energy (d). Although SASSY2 does an excellent job of separating unwanted products, it is clear that the addition of a Time-of-Flight system would add yet another dimension by discriminating against the tails in the velocity distributions of other particles.

A measurement of velocity in a gas-filled magnetic separator is very difficult to perform because the system is filled with only 1 torr of helium. In SASSY1 a system was built and used successfully that employed two avalanche counters one meter apart, each with 1 torr of pentane. “Thin” (30 micrograms per cm^2) windows of Parylene C were used to keep the pentane gas out of the helium. The method did work but it was far from being trouble-free because the slightest error resulted in destruction of the fragile films. Another drawback was the fact that the windows scattered some of the recoils out of reach of the detectors. We believe that we can overcome these disadvantages by using the much thinner and tougher films of polypropylene, which we have made recently using a technique pioneered at LLNL.

Another possible addition to the detection array is an energy loss (dE/dZ) device just upstream from the silicon detectors.This would be a 50cm-long Frisch-grid chamber that would collect electrons liberated by passage of recoils through the helium just before they are implanted. The size of the pulse will depend upon the atomic number of the particle and its velocity. (See Fig.6c)

Another reason for including T-O-F and dE/dZ counters is that they will be able to discriminate between alpha particles and recoils observed in the Si detectors during the beam pulse. Recoils will always be accompanied by T-O-F and dE/dZ counter pulses whereas alphas can never be. In those cases where there is a continuum of low energy recoils that overlap the alpha region these counters should be able to separate the two kinds of particles.

SOFTWARE PROGRAMS

For a system with 50 detectors, each with 2048 channels for each of a dozen variables, it is imperative that good monitoring be available on-line so that incipient problems can be spotted as soon as possible. The data-taking computer, an LSI-11/73, has a rather small memory so it is not possible to display all of the data at once with full resolution. Instead, the information is compressed by reducing the number of channels and by combining spectra; thus, in one on-line program the data from each of the five wafers, combining the outputs from its ten segments, is displayed in energy and position, during the beam pulse and in between beam pulses, on a 256-channel basis. By using other variants of the program one can sort low energy, high energy, low energy position, and high energy position data into similar histograms. There are certain window options that allow a limited amount of data analysis while data is being taken such as automatic printout of interesting events as they occur. Dynamic selection of a particular configuration of the program depends on the experimental needs of the moment. All of the raw data is recorded on magnetic tape.

Under development is a system employing another LSI-11/73 and a large hard disk which will allow much more data analysis with a very short time delay by passing the data from one computer to the other. The necessary hardware is on hand now and the software is being generated.

A very extensive program is being written for off-line analysis. Because of the very large number of channels involved and the need to accommodate a wide variety of experimental conditions the software has become quite complicated and is not yet complete; however, it has already proved its usefulness in demonstrating recoil-alpha and alpha-alpha correlations.

THE ELEMENT-110 EXPERIMENT

What can we predict about the isotope, $^{267}110$, that we hope to identify by bombarding ^{209}Bi with ^{59}Co?

First of all, we can be fairly confident that it will be an alpha emitter rather than a spontaneous fission emitter. The evidence for this is that SF decay seems to be quite hindered compared to alpha decay for nuclides in the nearby region from Z=106 to Z=109. If one examines the alpha particle decay data in the region a reasonable extrapolation for $^{267}110$ is an energy of about 11.7 MeV (See Fig.7). From this energy we would expect a half-life of the order of 100 microseconds. It will decay to the unknown nuclide $^{263}108$ whose alpha energy and half-life, similarly, should be about 10.8 MeV and also roughly 100 microseconds. The succeeding nuclides are all known thus identifying the genetic series and pinpointing the Z and A of the starting nucleus in the chain.

We expect that the cross section to produce $^{267}110$ by the reaction that we have chosen will be in the range from 1 to 10 picobarns. The reasoning behind this prediction is the following. A straightforward extrapolation from the GSI data on their production of nuclides from Z=100 to Z=109 yields a figure of 3 picobarns, as shown in Fig.8. It must be noted, however, that the excitation energy of the 59Co on ^{209}Bi reaction is 21 MeV at the Bass-barrier, whereas that for the ^{64}Ni on ^{208}Pb reaction is 13 MeV. An unsuccessful GSI experiment with ^{64}Ni was run at an excitation energy of 18+-3 MeV. The latter fusion is a little colder and so more like the comparison reactions used for the data in Fig.8 . On the other hand the “extra-push” required should be

slightly less in the former reaction because of the lower Z of the 59Co. Since the relative contributions to the reaction yield are not known we are assuming a standoff and thus a nominal 3 picobarns seems possible.

Another way of guessing at the cross section is the following: a Bass-barrier calculation gives a fusion probability of 10-5 leading to an EVR cross section of 1200 picobarns at 302 MeV; the deexcitation of this nucleus by the emission of one neutron should decrease this value further by a factor of about 100. To give an idea of the trends in this region a plot of all of the measured cold fusion cross sections in this region is shown in Fig.9.

What is our chance of seeing ^{267}110?

If we make the following assumptions: 500 micrograms/cm^2 target, 500 particle nanoamperes beam, 1 picobarn cross section, and 50% yield of the recoils into the focal plane detector, we would observe the implantation on the average of one atom of ^{267}110 every five days. Since there is credible evidence that the overall yield is likely to be substantially higher than 50% we regard these numbers to be quite conservative.

The expected decay sequence is shown in Fig.10. Because the implanted atom lodges well within the silicon crystal the geometry for observing full energy alpha events after implantation is about 55% and thus, on the average, we should detect an element-110 alpha decay every ten days. There can be as many as six successive alpha decays in the genetic chain following the ^{267}110 decay; since the origin of these decays does not change because the recoil kick from each alpha particle is so small, it can be seen that each implantation must lead to an electronic "star" of alpha particle events at each implantation site. In about half of the cases an alternate scenario is the decay of three alpha particles ending with the spontaneous fission of ^{255}104. Because either mode is so distinctive, the detection of even a single atom of element-110 will be definitive if the experiment is done in the essentially zero background environment that we anticipate.

A simple test done with ^{59}Co on ^{209}Bi at a low beam level showed that the gross in-beam background was only a few hundred c/m when extrapolated to the high beam level that will be used in the search for element-110. Similar experiments at GSI had to cope with in-beam background levels ten thousand times higher. The out-of-beam background is extremely small in the high energy region that is involved.

This raises the legitimate question of the long-term operational characteristics of the separator. We have not had enough experience with SASSY2 to make a firm statement on this subject because only a small amount of beam time was available during the last couple of months before the summer shutdown. Fortunately, what operation that we did have, was usually flawless; of necessity, the system was left unattended during the Owl Shifts without any problems.

To make the necessary runs at high beam intensity requires that the Bevatron is either not running or is using an ion in the medium-Z range when SASSY2 is scheduled. This coincidence has not been possible very often. What is urgently needed as soon as possible is a number of runs, each 3 to 6 shifts in duration, for performing low cross section experiments in the heavy element region.

Ideally, ^{58}Fe would be the projectile of choice in order to reproduce the GSI experiments that discovered elements 107, 108, and 109. This is out of

the question, of course, because we do not have any ^{58}Fe and it is extremely expensive. An alternate approach is to use V51 which has been accelerated to high intensity at the SuperHILAC; we would use it to make isotopes in the element-10^5 to -10^7 region where the cross sections are reported to be a few hundred picobarns; other possibilities are the use of ^{55}Mn and ^{50}Ti. As soon as feasible, the definitive ^{48}Ca yield experiment mentioned earlier should be performed because only that will tell us what our level of sensitivity actually is.

COMPARABLE EFFORTS IN OTHER COUNTRIES

In the Soviet Union an electrostatic device called VASSILISSA is now operating. A sort of velocity separator, it sorts out slow speed recoils from high speed beam particles by using the fact that in vacuum they have a lower energy per charge and thus are much more responsive to electric fields. It has been operating fairly successfully for about a year, using mostly light ions, and as far as one can tell it is quite competitive with SHIP and SASSY2 for many uses. It appears that it is not possible yet to implant recoils into silicon detectors (because of very high beam backgrounds, I infer); instead, the recoils pass through a thin window into a large Frisch grid chamber running at low pressure. This does give them 100% detection yield after implantation but the result is that they have no spatial separation as in a multi-crystal system. From Peter Armbruster I have learned recently that they have attempted unsuccessfully to use VASSILISSA to find their so-called ^{272}110, the ca. 5-ms SF emitter that they produced in bombardments of ^{235}U and ^{232}Th with ^{40}Ar and ^{44}Ca, respectively.

We know that they are also building (or have built) a gas-filled magnetic separator for the heavy element region but nothing is known about their progress.

GSI has constructed a dipole-quadrupole gas-filled device very similar to SASSY1 which should be in operation early next year. They plan to use it first with light ions to learn about its characteristics and then look for element-110 soon afterwards.

SUMMARY

After a long and difficult period of development, SASSY2 has advanced to the point where it is ready to tackle the very demanding problem of identifying super-rare elements made by cold fusion in heavy ion reactions. It possesses these qualifications; it has the excellent resolution that is necessary for separating them from unwanted products; it has the very high yield that is necessary for perceiving an atom or two in a reasonable period of beam time; it has the capability, by means of the genetic method, that is necessary for identifying a new atom uniquely; and, finally, it has the very low background that is necessary for pin-pointing such events.

ACKNOWLEDGEMENTS

SASSY2 was constructed with the help of several people who should be acknowledged.

Saburo Yashita was vital in getting the project started with his computer work on the optical design.

Jack Walton developed the remarkable silicon detectors which are essential to the successful operation of the spectrometer.

Bill Ghiorso has been and continues to be very active in many of the mechanical and electrical phases of the design and construction; the design

and manufacture of the target wheel, the beam shutter, and their drivers were due to him and his help and advice with other items have been most valuable.

Alfred Wydler designed the very complicated electronic circuitry and debugged it into successful operation with his usual care and tenacity.

Richard Leres wrote the ingenious software to make the electronics a viable system and continues to make improvements in the on-line data accumulation and reduction programs.

Diana Lee wrote the off-line data reduction program and has very patiently assisted in the analyses of many of the SASSY2 runs.

Matti Leino and C.H. Lee were very helpful in the early stages of the project.

Klaus Halbach was very generous with his time in giving important advice in the design of the magnetic configuration.

Patrick Somerville made many useful calculations and analyses in the testing of SASSY2 along with designing a convenient interactive program for the determination of the parameters needed for SASSY2 experiments. I am also very grateful to him for his valued assistance in the preparation of this report.

[THE SHE] NIGHTMARE!!

September, 1988
10 pages 4263 words

The SuperHeavy Elements

This is a true story. It is a demonstration of how research into unknown areas sometimes proceeds. It also shows the need for extreme caution when a discovery is made that is "too good to be true" for ninety nine times out of a hundred that is usually the case!

In the more than 50 years that I have spent pursuing the discoveries of new isotopes, new elements, and new types of accelerators I have always found it very stimulating to venture into completely new territory, but this particular exploration, the search for the hypothetical Magic Island of the SuperHeavy Elements (SHE), promised to be by far the most exciting since the discovery of fission. If we had succeeded, there is no question that it would have been one of the most outstanding episodes in the history of chemistry.

By 1976, the Island had become a sort of Holy Grail and the many attempts using a variety of methods to find it had enlisted the individual efforts of groups in many countries. A decade before, two theoretical papers by Bill Myers and Vladek Swiatecki of the Lawrence Berkeley Laboratory had triggered a call to world-wide action. They had pointed out that a tremendous stabilization against spontaneous fission would be gained by nuclear shell closure and this might produce an island of relatively stable elements that were separated from the nuclear mainland by an ocean of decay. The magic numbers, 126 for protons and 184 for neutrons, were proposed and it was pointed out that some of the elements might even have half lives long enough to exist in nature! A year later other scientists proposed that 114 protons could also be a magic number.

The "Giant Haloes" Fiasco

Following the Berkeley publications the Island had been searched for with great diligence but with a notable lack of success; there was an occasional claim but always closer scrutiny showed that the claim was erroneous. The one that garnered much attention for almost a year, an interesting demonstration of the scientific method gone astray, was made just a few months before our experiment. This was the famous Florida State/U.C. Davis experiment of 1976 wherein a team of physicists published a sensational claim that they had discovered SHE in nature! There was even a university celebration and the news hit the scientific community like a bombshell.

The genesis of this experiment was an interesting fossil puzzle that had existed in the literature for many years. It posed the question of the origin of the rare Giant Haloes that are occasionally found in monazite, a (Ce,La, Th)PO_4 mineral. A radioactive halo is generally defined as any type of discolored, radiation-damaged region within a mineral. When the inclusions are very small (1 m), the uranium and thorium daughter alpha emitters produce a series of discolored concentric spheres surrounding the inclusion. In thin section these appear microscopically as concentric rings whose radii correspond closely to the known ranges of the members of the U and Th chains of alpha particles But, occasionally much larger haloes with ranges corresponding to as much as 10 and 14 MeV have been found and it seemed that these could not have been caused by the known alpha particle chains. What could their origin be? There were no known alpha particles with these very long ranges.

Robert Gentry, a staff member of Columbia Union College, a small Seventh Day Adventist college in Maryland, had been studying these haloes for some time and by 1968 had become convinced that they were caused by extinct SHE alpha emitters. Not having the resources necessary to take the next step of identifying these supposed SHE, he sought to get the attention of the most influential person that he could think of who might be interested in the subject. The college was not very far from Washington, D.C. so what better person could he appeal to than Glenn T. Seaborg who was then the Chairman of the U.S. Atomic Energy Commission. Gentry called to introduce himself at 11 AM one morning in December and was meeting with Seaborg at 3 PM that very afternoon! That was a good choice for the subject of SHE was dear to Glenn's heart and soon Gentry became a Visiting Scientist at the Oak Ridge National Laboratory. Glenn had called Robert's work to the attention of highly-respected Lewin Keller. Lew, who was also interested in SHE, happened to be the Director of the Chemistry Division at ORNL with much personal experience with the actinide elements.

Gentry needed very sensitive method to look for the hypothesized exotic atoms, a sort of super microscope that could identify with absolute certainty a small concentration of atoms and now he had the assets of a big laboratory at his disposal. First he tried electrons to excite x-rays to identify the atomic number of the inclusions but without success. In an effort to obtain higher sensitivity his attention turned to the so-called PIXE method (Particle-Induced X-ray Excitation) which had been under development by Tom Cahill on leave from UC Davis and Neil Fletcher and his colleagues at Florida State University. This method used a 5-MeV proton beam from the Florida State Van de Graaff accelerator to excite the L x-rays from each element, the x-rays being detected with a nearby LN-cooled Si crystal.

With this setup they analyzed several Giant Halo inclusions and zeroed in on a particular line at 27,250 eV. This was only a few eV from a calculated value of 27,253 eV for element 126 ! And other lines seemed possible for elements 124 and127 ! Of course, these results immediately excited the scientific team and after many runs they came to the conclusion that, they had identified the characteristic L x-rays of element 126 --- possibly other SHE elements as well! Their data was quite erratic and not readily reproducible, but this was attributed to the fact that they were bombarding the haloes with a concentrated proton beam sufficient to damage them by heating them to incandescence. There were other inconsistencies which should have warned them that they were on dangerous ground; it was really a borderline experiment. But overexcited by what they thought they had found, normal caution was thrown to the winds and they decided to publish their findings. The paper seemed to indicate that their work was unambiguous. Clearly, it would be a major discovery.

I first heard of the experiment by telephone from Oak Ridge National Laboratory and immediately relayed the sensational news to a number of people in our Laboratory. The first reaction of everyone was that it must be correct because observation of the characteristic L x-rays from a SHE had been made and it was well known that accurate predictions of these energies existed. If the measurements were made properly they must have found the superheavy elements!

But if one accepts their findings some important conclusions can also be drawn. Just as U and Th are found in inclusions in monazite in minuscule quantities compared to what is found in ores, just so, one would expect that SHE would exist somewhere in ores in infinitely larger quantities than what had been found in the rare Giant Haloes. It would seem that without question the total amount of SHE in the

world must be very large, probably as much as tons! How could they have escaped detection?

It was horrifying to contemplate that tiny atomic bombs might now be possible because the very large atomic number might reduce the critical mass necessary for a nuclear explosion by increasing the number of neutrons per fission! One could visualize an intense world-wide search for the ores that contained the SHE. Other unstable elements, above and below the Island in atomic number, would be produced by neutron and heavy ion bombardment of these new stable elements. The speculations were endless and very, very exciting.

For a few days the claims were the talk of the nuclear world. Careful examination of their publication, however, soon revealed a number of potential flaws with their experiment and within months the fantastic claim was shown by three other laboratories to be completely erroneous. It was found that the particular x-ray line that they had thought was due to a SHE was actually due to the excitation of a previously unknown gamma-ray from praseodymium. This was produced by the proton beam's reaction on the cerium in the inclusion. At this point the bubble burst; what they had found was indeed too good to be true!

What causes the Giant Haloes? The best guess is that they are simply caused by a slow diffusion of U or Th atoms. This would explain the fact that there are not any sharp rings and why the maximum energy is larger than in the normal small halo.

The Berkeley Nightmare

The Giant Halo fiasco just whetted the appetite of others for it did not negate the existence of the Island, it simply meant that the proper experiment had not been performed yet. There was still the strong possibility that even if SHE did not exist in nature, the Island could harbor elements with relatively long radioactive half lives but it meant that these could only be reached by heavy ion bombardment of the known elements.

We had already tried the simple experiments and they had not worked. Now we decided that we were ready to try our hand at the next step and play our trump card; we would use a very heavy actinide target and a neutron-heavy projectile. Since our best target/projectile combination did not have enough neutrons, we knew that we could not penetrate to the most stable region; but we could try and reach the shore of the Island where the half lives might be in the range of hours to months.

The essence of our planned experiment was simple but carrying it out properly would require a substantial effort. A target of ^{248}Cm would be bombarded by 48Ca ions that had been accelerated by the SuperHILAC to an energy sufficient to surmount what is called the "coulomb" barrier. This barrier is an electrostatic hurdle that must be overcome before Ca with 20 protons can be brought close enough to Cm with 96 protons to allow the two atoms to fuse together and form element 116. The curium target with 152 neutrons and the calcium projectiles with 28 neutrons, is a combination that we hoped would fuse together and end up with such a superheavy compound nucleus. After the evaporation of three or four neutrons the result would be atoms that had never been observed before. These atoms would have proton numbers far beyond that of the 106 protons of element 106, the heaviest element known at that time. We had produced that element two years before using the same accelerator but a different apparatus and 20 years later had named it Seaborgium.

Our previous experiments to produce SHE had been unsuccessful because of insufficient sensitivity and also because discrimination against known transuranium elements that produced interfering alpha and spontaneous fission (SF) activities was not good enough. Now a top-flight team had been assembled under the leadership of

chemist **Rollie Otto** to undertake an experiment which we had every reason to believe would remedy these faults. The plan was to isolate chemically a SHE fraction as quickly as possible that would be free of any actinide elements and detect its alpha or SF radioactivity. A tracer, the actinide element ^{241}Am, would be added at the very beginning of the chemical process to allow us to monitor how well the separation had succeeded.

Our long-time colleagues, **Ken Hulet** and **Ron Lougheed** of the Livermore laboratory, had made a beautiful curium target that was uniformly thick by vaporization of curium fluoride onto a thin beryllium metal substrate. This was to be a recoil experiment wherein the intense beam of calcium ions would pass through the substrate before entering the target. The reaction products would be ejected from the target by the beam and caught by downstream catching foils. The catcher foils would be dissolved and then exposed to different kinds of chemistry. To reach the Island the beam was not composed of the ordinary atoms of calcium (mostly mass 40) that one finds in nature; we had to use the very rare heaviest isotope of that element with mass 48. This is a very expensive isotope (about $100,000/gram) because an elaborate commercial process is needed for its separation from ordinary calcium so it was necessary to make sure that the efficiency of the accelerator in accelerating it was as high as possible.

For this purpose **Mike Nitschke** and I spent many hours working with the crew to satisfy ourselves that this was the case. This meant that we had to make sure that the accelerator had the energy that we desired and that the beam was focused properly for the experiment. During the week devoted to this particular experiment everything seemed to work out but it required our full attention as well as that of the crew. We did succeed in delivering the strongest beam of 48Ca that had ever been used and we felt that we had a good chance to reach our goal. This would be the first time this particular experiment had been done and we thought that there was a finite chance that it would succeed. We were acutely aware that the Russian team at Dubna would shortly be doing the same experiment very soon. The competition with them had been especially keen for the honor of discovering elements 104, 105, and 106 and we wanted to get there first once again. The discovery of the superheavy elements would be the biggest prize of all!

The reaction recoils ejected from the target would be caught in a stacked series of thin catcher foils separated from one another to dissipate the heat from the beam. Without our realizing it, this arrangement was to lead us into a trap. For our experiment a dozen catcher foils of thin aluminum had been glued onto rings about a centimeter in diameter with silver paint that had been dissolved in butyl acetate. This assemblage of separated foils was then mounted next to the Cm target in a special probe mounted near the exit of the linear accelerator.

After an overnight bombardment the foils were removed and placed in a dissolving solution along with the americium tracer. This was the first step in the chemistry that would isolate the coveted SHE atoms. The plan was to obtain as quickly as possible two fractions a superheavy element fraction and an actinide fraction that would contain the ^{241}Am tracer. The resultant fractions would be analyzed for alpha and SF activity.

However, something unexpected happened in the very first step. When the catchers were removed at the end of the bombardment it was noticed that the shiny foils had become coated with a barely visible blackish crud and when they were dissolved, small amounts of what looked like carbon granules appeared on the surface of the dissolver solution; they simply would not dissolve. With great perspicacity the chemists knew that if this unexpected crud was carbon it could have

an important chemical influence on what happened. Since element 116 was expected to have the chemistry of a noble element it, too, would not dissolve and instead would be adsorbed by the carbon. To play it safe, the leader of the experiment, **Rollie Otto**, decided that a third fraction should be prepared, a "Carbon" fraction, just in case the SHE atoms landed there. To check on this possibility the carbonaceous material was caught on a filter paper as the dissolver solution was passed through it so that it could be analyzed directly with a solid-state detector without any additional chemistry. *This filter paper did not contain any of the Am tracer* but when it was examined with an x-ray counter it was found to be emitting copious quantities of x-rays that looked as if they were coming from *gold* activities. That was certainly consistent with the hypothesis that a noble element would be adsorbed on the carbon!

It was thus that three fractions were separated for analysis rather than two. The first, a superheavy element one, according to conventional ideas of the chemistry expected for element 116; the second, an actinide fraction, a lanthanum fluoride precipitate complete with a high yield of the tracer; and the third, the unplanned one which could be expected to snare superheavy element atoms if they had the chemistry of noble elements. It was a very clever concept and took advantage of the appearance of the mysterious crud. Now it was up to the analyses of the three fractions. The actinide fraction had a lot of beta activity so it was decided to follow decay of its SF activity with a large area proportional counter that I had at the SuperHILAC.

Within a half hour there was great surprise when word was passed from the counting room that a single spontaneous fission count had been observed in the "Carbon" fraction! Could it possibly be that this simple fortuitous chemistry had done the job all by itself! It was obvious that it could not be an actinide element since there was no sign of the tracer in the "Carbon" fraction and the legitimate actinide fraction had a very high yield of ^{241}Am. Slowly the counts continued to come in and the excitement and anticipation grew. Could it really be that a miracle had happened?

In a matter of a few hours it became clear that the "Carbon" fraction not only had spontaneous fission activity; indeed it had about as much as was observed in the actinide fraction! This was extremely exciting news to everyone. And the half-life seemed to be somewhat different, too, but both were in the range of a few hours. There was no getting around it; we had performed a classical nuclear chemistry separation and concentrated the SHE atoms in a chemical fraction all by itself by the use of chemistry. There seemed to be absolutely no doubt that this is what had occurred. **Glenn Seaborg**, who had followed the progress of the experiment closely, was equally excited and agreed with the interpretation of the results. If our observations were correct we had *chemically* isolated a SuperHeavy Element!

Now it was up to the instruments to finish their job. I remember having a nagging doubt for some reason and felt only half convinced. However, I was quite willing to accept the result as a gift of the gods. It was Friday and, having spent a strenuous week and with this experiment essentially completed except for following the decays, I went off with my wife, Wilma, to Pt. Reyes, in the afternoon to do some birding, our favorite form of relaxation. I had arranged with Seaborg that I would call in later in the day to Berkeley and see how the counting was progressing and whether anyone had come up with an alternate explanation for the experiment.

I reached him about 4:30 PM and found that the ratio of the fissions in the "Carbon" fraction to those in the actinide fraction was still holding up. Now I felt it likely that soon I was going to have to pay off my $100 bet with Glenn. [Years before I had made a bet with him that the Superheavies would never be found. It was a wager designed only to be lost. I would be more than pleased now to pay off the bet!] We both felt that the discovery was a very important matter so, since we were talking

on an unsecured telephone line, we were very cautious and talked with our old wartime code habits by not being very specific about the experiment and using coded words for what had been found. I remember that he was 100% convinced. After all, we had separated a superheavy element chemically, hadn't we?

That evening, I went up to the SuperHILAC to check on the counting and make sure that everything was operating properly. I had been puzzled all day by the fact that the two fractions seemed to be decaying at the same rate. What a coincidence this was! About midnight, becoming very suspicious, I arranged it so that I could look at the pulse spectrum of the integral counter that was counting the LaF3 fraction that contained the actinides. I was shocked to find that most of it's spontaneous fissions were not being counted! Apparently, the filter paper which contained the very -active precipitate, was charging up electrically and thus preventing the proportional counter from counting properly. Because the threshold for making an integral count was set too high maybe less than 10% of the SF's were being counted. It meant that instead of being comparable, there were ten or twenty times as many spontaneous fissions in the actinide fraction as in the carbon fraction. My confidence of SHE in the carbon fraction immediately went down from 50% to 1%. I knew that something was wrong but I had no idea what it was. I decided to sleep on it and let my personal computer work on the problem. I have often employed this stratagem to work on a problem while I am asleep and often it does offer new insights.

And once again it did:

The nightmare

Early the next morning I was jolted awake by a truly disturbing nightmare. This was the first time in my life that I have had a nightmare caused by an experiment and I hope that it will be the last! In this very vivid dream I was driving a car containing other people when I came abreast of another which contained a bunch of Russians and we immediately began racing. I had the faster car but I couldn't pass them on the narrow road because of traffic! I was not about to let that stop me so I drove over onto the shoulder at high speed to go around them. In this way I had just come abreast of them when all of a sudden I was confronted by some obstacle (a brick wall, I think). Very much frightened, I tried to brake the car and found that I couldn't. The brakes wouldn't work and I realized that we were out of control! We were going to crash!

At this point I woke up.

Out of control!

In the shower I reviewed the dream because it had been so terrifyingly real and realized immediately that it had been prompted by yesterday's big SHE run that was still being counted. The mind is a wonderful thing; the symbolism was especially acute, complete with the race with the Russians to find the SHE first and it had come to the conclusion that the experiment was **out of control**. Now it was up to me to figure out why and suddenly I understood what had happened! It was all so simple…

The fact that we could see the dark crud meant that its thickness was of the order of 20 micrograms/cm^2 on each side of the foils. Since each aluminum catching foil was 300 micrograms/cm^2 thick this meant that the carbonaceous films were 5-10% as thick as the aluminum foils that had been dissolved and that same fraction of the recoils would stop in those layers. If that material was not capable of being dissolved, then the actinide recoil atoms lodged inside it would not be dissolved and neither would the tracer penetrate the films. We had not performed any chemistry on those atoms at all! I decided that the source of the carbonaceous crud was most likely the organic binder used in the silver paint that had been used to fasten the foils to the

rings. I knew that the beam was depositing about 100 watts in the probe during the bombardment so that it would become very hot. It was very likely that the binder would be vaporized and end up as carbon on the aluminum foils. Whether the carbon was deposited immediately or slowly over the course of the bombardment I could not know but within a factor of two it seemed like a very reasonable scenario so I went up to the Lab to confirm it.

Shortly after I had arrived, checked the equipment, and found nothing that disagreed with my explanation, Glenn called on the phone to see how the "SHE" fraction was doing. It must have taken me at least five minutes to convince him that it was a false alarm but, finally and very reluctantly, he accepted my explanation. Shortly after that **Rollie** called up but it took 10-15 minutes to persuade him that the SHE had vanished. I really felt bad for having to deliver the sad news after our having "discovered" the superheavies for all of 18 hours.

In retrospect, it was an interesting experience, even though very disappointing. We had prepared very well, according to the technology of the times; we had overcome many obstacles just to make the excellent bombardments; we had performed the experiment with professional expertise; we had made an acute observation during the experiment that should have lead us to discover the SHE; and, finally, we had chemically separated what could well have been a member of that tribe. Everything was perfect expect for that last statement. There was no SHE candidate. We had been misled by an amazing (amusing) artifact of the experiment.

Albuquerque talk March 10, 2006

AG: I did not read the notes, I merely gave an interpretation of them, off the cuff

Long Range Detection Research

Slide 1

Joe-1 Mushroom picture Caption: First atom bomb test by the Soviet Union, 1949. The evidence for an explosion was heralded by a cloud that moved across the earth.

AG Note 1:

This is an untold story concerning the detection of the first atomic bomb exploded by the Soviet Union in 1949; this was labeled Joe-1 by the U.S. and took place only four short years after the first test explosion by the US in 1945. I have been told that in secret circles some members of the President Truman administration were sure that what had been detected was more likely a runaway nuclear reactor accident than an atomic bomb explosion. This was because the CIA was adamant in claiming that it was literally impossible for the Soviet Union to have constructed a bomb as quickly as they had and this organization seemed to have the exclusive ear of the president. That is why, when Truman announced that there had been a "nuclear explosion" in the Soviet Union, he carefully did not say that it was a bomb. On the other hand the chemists who analyzed the fission product debris were certain from their decay measurements that an atomic bomb had to be its origin. Thus it was that confirming evidence was needed. Was it a bomb or not?

Slide 2

A typical WB29 in flight (http://home.att.net/~sallyann4/robison-col.html) Caption: Samples of the cloud were picked up near the Siberian borders in special filters by high-flying converted B-29 planes and analyzed in California by TracerLab radiochemists.

AG Note 2:

First, let us examine how the information was gathered. The first evidence that an atomic bomb had been detonated by the Soviet Union came from aircraft that had picked up explosion debris from the airborne cloud while flying routine missions near the edges of Siberia. These planes had been on patrol since spring, 1949 and were operated by AFOAT-1, a newly formed Air Force group that was tasked specifically to look for evidence of Soviet bomb tests. In California the filters were analyzed into various chemical fractions to determine the fission product spectrum by TracerLab, a commercial firm near Berkeley under contract to the Military. The radioactive decay of the activities that were detected showed that they were fission products that had been freshly produced and thus could not have arisen from a nuclear reactor accident. These determinations made the debris really interesting but more material was needed to increase the power of the analyses by examining its plutonium fraction. Fortunately, a much larger sample of the cloud had been collected by the Naval Research Laboratory as it passed over Washington, D.C. dropping rainwater into giant collectors on the roof of their building. The rainwater was chemically concentrated to

produce a significant heavy element fraction and sent on to Los Alamos. There it was found to contain 3 alpha c/m of plutonium. This in itself, did not prove that the explosion was an atomic bomb since a uranium reactor would also contain plutonium even if it had been operating for only a short length of time. The important definitive measurement needed was the isotopic composition of the Pu. In particular, how much Pu-238 was present? This was the real smoking gun that would determine how the cloud had been produced

Slide 3

The n,2n reaction on Pu-239 makes Pu-238 in a bomb explosion.

AG Note 3.

How does one tell the difference between an atomic bomb explosion and a runaway nuclear reactor explosion? The definitive way is to look for Pu-238. Physicists knew that Pu-238 would be made by fast neutrons in a nuclear explosion by the n,2n reaction whereas in reactor-produced plutonium, fast neutrons are slowed down by the moderator and the relative number of Pu-238 atoms that would be produced relative to Pu-239 would be fewer by about a factor of a hundred. However, since the half-life of 86-year Pu-238 is about 300 times shorter than that of Pu-239 it should be readily detectable by measuring its alpha activity even if the amount made is very small. The problem was that at that time there was no instrument available in the whole world that could make this measurement. Or was there?

http://photos.aip.org/images/catalog/ghiorso_albert_f1.jsp

Slide 4.

Use the two pictures side by side P.132&133 HGS Captions: SGT and GTS near centrifuge. AG inserting sample in grid chamber.

AG Note 4:

It so happened that about six months after the Joe-1 incident the discovery of elements 97 and 98, berkelium and californium in Berkeley was published by the Seaborg group of which I was a member; this work called attention to the fact that we had developed an enormous sensitivity for analyzing small amounts of alpha activity. These new elements had been made by the bombardments of microgram quantities of americium and curium by beams of helium ions to make small amounts of alpha activity coming from elements 97 and 98 in different bombardments. My role in the team had been, and would continue to be, to devise and use suitable instrumentation that would be needed for the work with the heaviest elements. I had found from the literature that what was available was meager and inadequate so I had to invent and develop new methods. After a period of years these techniques had been honed to fit the goals set by the search for heavier elements and thus the program came to depend completely on our 48-channel pulse analyzer

Slide 5.

The Berkeley alpha grid chamber schematic. The slide has its own caption.

AG Note 5:

Explain how a grid chamber works The device used for this research was a copy of the first instrument that we had constructed at the Metallurgical

Laboratory at the University of Chicago. It used an array of 48 old-fashioned biased thyratron pulse height detectors arranged to measure the pulse height of each detected alpha particle. It had been vastly improved at Berkeley by the substitution of a gridded alpha particle ion chamber for the ordinary ion chamber that I had used at Chicago. With this system it was possible for us to measure accurately the energies of the alpha particles that were emitted from a thin sample. Its sensitivity was such that it could make accurate measurements that were able to differentiate among the various alpha-emitting isotopes in the heavy element region. At that time I think that this facility was the only one in the world that was capable of analyzing very weak sources that might not emit more than an alpha count per hour and it was the key tool in the discovery of elements 97-101.

Slide 6.

http://www.lanl.gov/history/wartime/images/ProjectYBadges/s/spence-roderick_w.gif

Photo of Roderick Spence is at this URL

AG Note 6:

I was essentially unaware of the Joe-1 incident, not only because of the intensity of the work associated with the discoveries of elements 97 and 98, but also because of the level of secrecy that was imposed around the possibility that the Russians might have exploded an atomic bomb. In May, 1950 when I was approached by Roderick Spence, the head of the Los Alamos team that was analyzing the Joe-1 activity, it never occurred to me to connect his request with the Soviet explosion as published in the news media. Spence merely asked if I could analyze a small sample for them (only 3 alpha c/m) saying that the measurement was important for national security. He said that it was related to a highly classified matter that could not be divulged to me since I did not have the necessary security clearance. I agreed to make the measurement and the sample was sent to me by a special courier. I knew that it must be very special indeed if it had to be hand-carried from Los Alamos by a security officer! I inserted the small platinum plate into the grid chamber and started to collect data. After a few hours I knew where the sample had come from because it was very much like one that I had measured in 1945 when I was in Chicago.

Slide 7.

http://www.atomicarchive.com/Photos/Trinity/index.shtml

Use the Mushroom Cloud that developed after 12 seconds Caption: The first atomic bomb test in the world July 16, 1945

AG Note 7:

That sample had come from Trinity, the first atomic bomb explosion in the world! It had been set off mounted on a tower in the Alamogordo desert region a few hundred kilometers south of Los Alamos to test the implosion design for plutonium. I deduced that this new sample must have originated somewhere in the Soviet Union! [Flash back to the Metallurgical Lab in 1945. The Los Alamos bomb physicists wanted to determine the number of neutrons created in the Trinity explosion by measuring the amount of Pu-238 that was produced by the n,2n reaction on Pu-239. They came to Chicago to use the thermal column of the old CP-1 reactor that had been set up at a nearby site. Their plan was to use its thermal column to measure the number

of slow-neutron-induced fissions of Pu-239 in their explosion sample and compare it with that of the original Pu. The difference in the fission/alpha ratios would be due to the alpha activity of Pu-238 but because several uncertain corrections had to be made it was not a straightforward measurement. When they heard about our newly-completed 48-channel pulse height analyzer (In 1945 multi-channel devices were not yet common.) they contacted me to see if I could help them by measuring the Pu-238 content directly with my alpha pulse analyzer. We decided that it would do their job more readily and accurately so I agreed to analyze their sample. That sample contained hundreds of alpha counts per minute so it was easy to make the measurement that they needed and I made the analysis quickly. That is how I came to know the typical signature of the alpha spectrum of Pu after an atomic bomb explosion. The amount of Pu-238 formed is approximately proportional to the efficiency of the bomb and this means that the ratio of the alpha peak at 5.5 MeV (Pu-238) to 5.16 MEV (Pu-239 + Pu-240) gives one a good idea of the efficiency of the explosion. All of this took place some four years before the Joe-1 explosion.] Unfortunately, I could not find my original pulse analysis of the Trinity test. I remember that the Pu-238 intensity was close to 10% by activity of the total alphas.

Slide 8

(The final alpha spectrum of the Joe-1 test shown in natural color)
Caption: The final pulse analysis of the Joe-1 test shown in natural color to preserve it as a piece of history.

AG Note 8

The Russian A-bomb sample emitted less than 200 alpha counts/hour so I had to run it many times to determine the Pu-238 content with a reasonable statistical accuracy. Final results of the Joe-1 test measurements were:

5.15 MEV Pu-239,240 2.959+-0.022 Net c/m
5.50 MEV Pu-238 0.149+-0.007 Net c/m
%Pu-238 of total activity was 4.8%

The ratio of 238 to 239 that I measured was roughly half that produced in the Alamogordo test. In contrast the Joe-2 test which I measured in October, 1951 showed an efficiency which was comparable to or better than that of Trinity. I understand that the first was a carbon copy of the U.S. device, its design obtained from spies, whereas their second test was an original Soviet design! My measurements showed that both devices used Pu-239.

Slide 9

My Pu-241 idea: Pu-239+n >Pu-240+n >Pu-241

AG Note 9

While pondering the problem I thought of an additional measurement that I could make that would show how long the reactor had run to produce the Pu in the bomb. Why not measure the radioactivity from the Pu-241 that also must be in the material? That isotope, with a half-life of 14 years, decays by emitting beta particles (electrons) with a very low energy. These electrons could not penetrate through the window of the usual Geiger counter so I decided to try measuring the number of betas directly by using a windowless proportional counter, i.e., one where the sample is placed inside the counter. I

decided that since the sample had been highly purified it was probably free from fission products. This seemed to work out very well: now I was able to measure how much Pu241 (and thus roughly an estimate of the amount of Pu240 there was in the sample. The Pu241 isotope is made by successive neutron captures in the reactor, as shown here. Pu239+n >Pu240+n >Pu241

The amount of this isotope depends on how long the reactor had been operating before the uranium was removed from the pile to be processed. I roughly calibrated the counter's efficiency to count the beta particles from Pu-241 by measuring the amount in samples of plutonium of various "ages" that had been recovered early on as the reactors came up to full power. When I made this comparison it showed that the amount of Pu-240 was between 2.2% and 3.3% in the Joe-1 sample if it was produced in the same way as the American reactors. It showed that the reactor where it was made had operated for about a year or so.

Slide 10

Use photo of Oppenheimer, Seaborg, and Lawrence

AG Note 10

I was asked to come to Washington, DC to report what I had found about Joe-1. As far as I can figure out from Google at this late date, the committee that I reported to was probably the Committee on Atomic Energy of the Joint Research and Development Board of the Armed Services. Robert Oppenheimer was its chairman and he took a personal interest in developing techniques for detecting foreign atom bomb explosions. Unfortunately, I do not remember anything about the hearing itself. It was clear that these measurements proved without question that the Soviet Union had joined the Atomic Bomb Club and that the CIA was completely wrong in its estimates of the potential of that nation. I found it fascinating to play the role of detective to find out what had happened on the other side of the world. It may have been because of the very hush-hush nature of this work that I did not document what happened subsequently. I can't recall other names or details, unfortunately; I was only 35 and probably terrified!

Slide 11

Photo of the G.N. Flerov picture on the chart in Rm 146. I will use it as a backdrop while I talk about the stuff below.

AG Note 11

As the years went by I became aware of an interesting international symmetry of the situation. After fission had been discovered by Hahn and Strassmann in Germany in 1938, Giorgi Nikolaevich Flerov published the very important discovery that fission of an atom of U-238 occurred spontaneously (all by itself without external excitation) at a very tiny rate. Because of this, Flerov's reputation immediately rose in the scientific community and he became well-known world wide. His scientific stature in the Soviet Union also became enhanced so that his advice was respected, and soon he became highly placed in the USSR. He noticed that the Western world of scientists had stopped publishing entirely in the heavy elements field after the discovery of fission and he deduced that there must be a large diverting effort aimed at developing nuclear weapons because of that discovery. He sent a letter to Joseph Stalin pointing this out and urged him to start a similar program. Stalin already had such a program to develop an

atom bomb on the books but with a much lower priority than WWII. Flerov's advice jumped the priority to #1 and he became one of the influential scientists in its development in the Soviet Union. When the war ended, Flerov became the head of his country's search for new transuranium elements beyond element 101. I had named that element mendelevium to honor the great Russian chemist who had organized the table of the elements and Flerov and his people appreciated that gesture; but now they too wanted to make their own contributions to expanding the roster of elements and this put them in direct competition with me as the head of Berkeley's efforts. He was given free rein to develop a suitable program to be first in the world. Berkeley built linear accelerators and he chose to build large cyclotrons. This resulted in a friendly competition between Berkeley and Dubna over the next 40 years in the search for the elements beyond element 101. In particular, I remember element 104 as being a bad stumbling block for them. They had made the mistake of proposing the name kurchatovium before they had proved that that their discovery was correct. Igor Kurchatov was the scientist who headed their bomb project so Flerov was particularly keen on preserving that name. Unfortunately for him, though we tried many times, when we tried to confirm his candidate for element 104, we found that it did not exist; its discovery was an artifact of the way in which the experiment was performed! Flerov never gave up, however, and hoped that we would accept the name, just as we had for nobelium. We did not oblige him; we named element 104 rutherfordium after Ernest Rutherford.

Slide 12

Photo of the Vasilev cartoon on the chart in Rm 146

AG Note 12

Flerov sent this cartoon to me via Mike Nitschke in Nov.1976. Flerov wrote the number "104" on the light bulb with the verbal message to me via Nitschke, ----"Ghiorso will finally see the light!" But Flerov died in 1990 without changing our finding that his element 104 candidate that he had named kurchatovium was completely wrong.