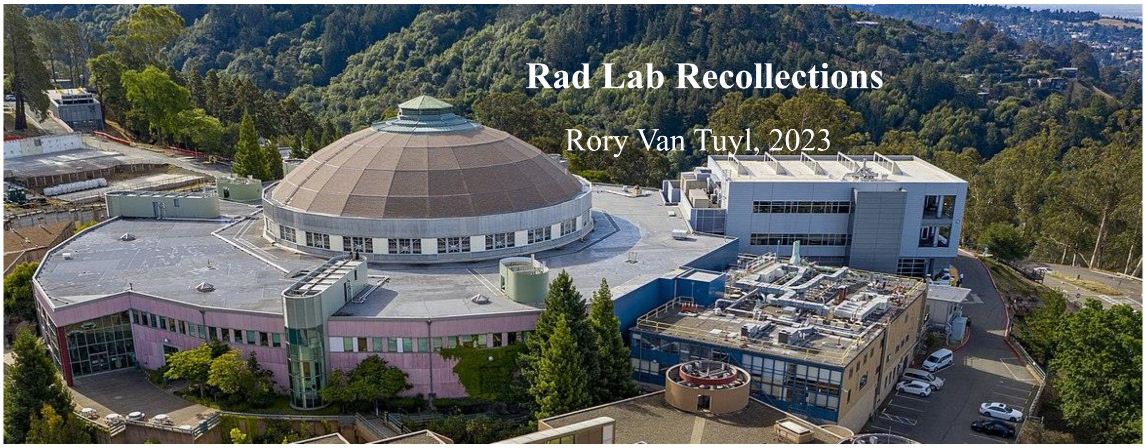

Introduction

In 1963, at the age of 20, I landed a job at perhaps the nation's foremost physics laboratory. This was my third job as an undergraduate engineering student, the sort of position we would today call an *internship.* Administered through the UC Berkeley college of engineering, the Cooperative Work-Study program placed students in real, often formative, 6-month jobs in California industries. After stints in a San Diego Physics Laboratory and a Palo Alto Aerospace Company, my third and final assignment was to be at the fabled Lawrence Radiation Laboratory in Berkeley, California.

The position itself was nothing special: a low-level electronic technician's job for which I was completely unqualified. But the world I was to inhabit there, filled with amazing machinery and apparatus, and staffed by everyone from world-famous scientists to high school dropouts, was eye-opening and a great introduction to what would be my 44-year engineering career. All told, I spent 4 years at the Rad Lab, nine months as an undergrad, and 3 1/3 years as a BS level engineer. Now, 60 years after I started there, I can still recall the environment, people, and experiences, some of which I will recount in this memoir.[1]

The HILAC

Located in the Berkeley hills just above campus, LRL was willed into existence by its eponymous founder, Ernest Orlando Lawrence [1901-1958]. Lawrence was the nuclear physicist who, in 1929, as an associate professor at UC Berkeley, invented the Cyclotron, a device for accelerating charged particles to high energies suitable for bombardment of various target substances in the quest for knowledge about the atomic and subatomic world. Lawrence's early on-campus cyclotron was used in 1939 by Edwin McMillan [1907-1991] to produce the first trans-uranium element, Neptunium (93). After McMillan departed the Berkeley campus in 1940 to perform wartime radar research, the trans-uranium work was continued by chemist Glenn Seaborg [1912-1999], who, in 1941, led the team that discovered Plutonium [94]. (Seaborg and McMillan shared the 1951 Nobel Prize in chemistry for this discovery).

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[1] Much of the information I write about here, I was unaware of in the 1960s. I have supplemented my direct personal knowledge from those years with published information now available online.

Assisting Seaborg and company was a 1937 electrical engineering graduate of UC, Albert [Al] Ghiorso [1915-2010]. Al had been instrumental in developing radiation detectors used by Seaborg et al. in their trans-uranium work.[2] Ghiorso was of such value to the research that Seaborg invited Al to join him in Chicago as part of the Manhattan project. Ghiorso and Seaborg worked together there for 4 years, during which time they jointly discovered two more trans-uranium elements, Americium [95] and Curium [96]. These discoveries were made possible by Ghiorso's development of an early *Pulse Height Analyzer* [today called the multichannel analyzer].

After the War, Ghiorso and Seaborg returned to Berkeley. They joined Lawrence's on-campus *Radiation Laboratory* and proceeded to discover more trans-uranium elements using Lawrence's 60" cyclotron: Berkelium [97], Californium [98], Einsteinium [99], Fermium [100], and Mendelevium [101]. Ghiorso developed the recoil technique that enabled discovery of element 101, atom-by-atom.

But by this point, higher energy bombardment would be needed if trans-uranium element creation were to continue. In 1957, a new tool was created: the Heavy Ion Linear Accelerator [HILAC].[3] It was located in the Berkeley Hills as part of the complex of Particle Accelerators at what became known – after Lawrence's untimely death in 1958- as the Lawrence Radiation Laboratory.[4] McMillan became Lab Director after Lawrence's death, and Seaborg became Berkeley Chancellor. The HILAC became the responsibility of Al Ghiorso. By the 1960s, when I came on the scene, the HILAC was Al's fiefdom. Al had an office in the mezzanine level, but most often, he was to be seen on the main floor, tending to his experiments, or in the control room making sure all was going to plan. Al always dressed casually, usually sporting a lab coat, and wearing leather sandals over black socks, in the fashion of Berkeley liberals of the era. Al was a down-to-earth guy, not pretentious in any way, but whenever he was in a room, there was no doubt as to who was in charge. On the wall of the HILAC control room, he had posted a graph of radioactive decay as measured in the feces of himself and a couple of co-workers who had accidentally ingested some

Al Ghiorso [right], with his mentor, Glenn Seaborg in the HILAC control room [undated]. For this photo, Al wore a white shirt and tie. Normally, he wore casual clothes and leather sandals over black socks.

[2] There were two secretaries working for E. O. Lawrence at this time. Seaborg married one, Ghiorso the other.

[3] https://history.lbl.gov/Publications/Research-Review/Magazine/1997-fall/hope/adam.html

[4] Today called the Lawrence Berkeley Laboratory to distinguish it from the Lawrence Livermore Laboratory which develops nuclear weapons.

radioactive element. He pooh-poohed the danger of radiation [he died at age 95], claiming the real workplace hazard was high voltage. My clearest memory of him was stationed peering into his pulse height analyzer watching the data dribble in for his latest experiment.

When I joined the Rad Lab in 1963, I was assigned to work among a group of Engineers and technicians at the HILAC. These guys were all working at lab benches in a single room next to the main floor of the HILAC itself, building various electronic gear meant to upgrade the HILAC performance. The engineers I recall were Bill Gagnon, Milton Hom, Lou Reginato and Link Harris. Technicians were John Noble, Jim Johnson, and Bill Stahl. We all slaved away at our benches – me in a fog of trying to understand what the waveforms on my oscilloscope were telling me about the operation of the device I was testing for one or another of the engineers. For fun, a group of us, organized by Milt Hom, would set up a net in the parking lot and play badminton at lunch.

For me, this was a great experience. These guys treated me like a kid brother who needed help and guidance. In return, I did my very best to help them out. Milt Hom was especially helpful to me (he was the first American Born Chinese I had met, and a really good guy). I recall debugging some sort of device designed by Milt, consisting of a cage of PC boards with flip-flops built of discrete components (this was just before they became available as ICs). And, I helped Bill Gagnon get a device he had built for Al Ghiorso, a shutter wheel spinning in sync with the HILAC RF pulses, installed in Al's experimental bay. And I recall my first actual transistor amplifier design being critiqued by Lou Reginato.

But the most memorable character of all was Bill Stahl. A former Navy Chief, and a veteran of WWII, Bill would have been the perfect casting choice for Popeye the Sailor Man. He loved to pull me aside whenever possible, take me to the blackboard and educate me on the subject of electronics. The courses I'd had thus far were mostly unhelpful, a bunch of equations without useful context. Bill provided the practical version in pithy sayings. "You can't change the voltage across a capacitor instantaneously." "You can't change the current through an inductor instantaneously." "Deebees is deebees." [5] Bill, a bright young kid who'd joined the navy while a teenager, had been trained in electronics by Uncle Sam, served in the Pacific during WWII, and eventually retired from the navy to a job at the Rad Lab. Bill also regaled me with tales of his adventures, including his encounter with an Australian girl he and his shipmates dubbed "Big tits". She gave all his shipmates the clap, but not Bill, since he enjoyed her while they basked together in the surf (or so he said).

As educational as the work experience was, the real interest at the HILAC was stuff outside the confines of our workspace. One item was a gigantic Cockcroft-Walton generator being readied, I believe, as an upgraded injector of charged particles for the main accelerator. It was built with semiconductor diodes and capacitors stacked in a ladderlike configuration perhaps 20 feet tall, capped by a mushroom-shaped dome to prevent sparks from its 750kV apex. The HILAC itself was a giant tube, perhaps 10 feet in diameter, which was a waveguide for the 70 MHz RF wave

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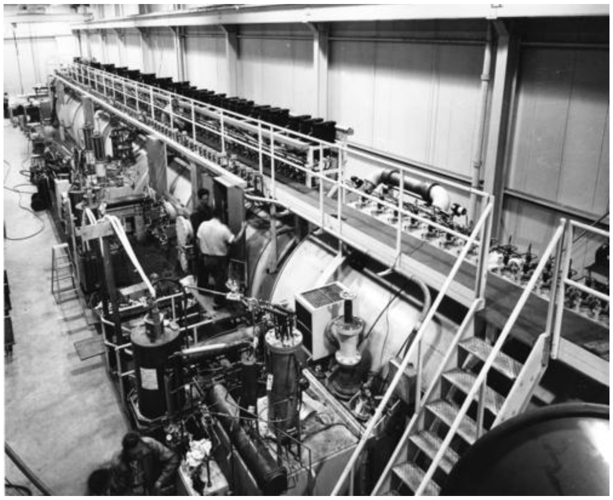
[5] I=C*dV/dt. V=L*dI/dt. If neither I nor V is infinite, neither can their tine derivatives be. "Deebees is Debees" was meant to clarify that there is no difference between dBs based on power ratio and dBs based on voltage ratio: $dB=10Log(P1/P2) = 10Log(V1^2/V2^2)=20Log(V1/V2)$. Obvious once understood, but students such as I did not always get the idea from classroom lectures. Bill Stahl set me straight.

that propagated longitudinally, dragging charged particles along with it.[6] I also got to see pulse-forming networks built of gigantic oil-filled capacitors and inductors wound of thick copper wire on large spools. The synthetic transmission lines stored charge which was released in a high voltage burst when a high-voltage thyratron tube switched ON. I felt like a Lilliputian wandering around in a giant radio set.



The Berkeley Heavy Ion Linear Accelerator [HILAC] came online in 1957. Basically a 70 MHz circular waveguide with centered drift tubes [right] that supported 70 MHz RF pulses, it accelerated heavy ions as heavy as Argon to energies sufficient to create trans-uranium elements. It came online in 1957, was upgraded in 1971, and was used to produce elements 102-106. Al Ghiorso was in charge.

They say everyone remembers when they heard about Kennedy's assassination. Here is my recollection. On Friday, November 22, 1963, Glenn Seaborg -then the U.S. Atomic Energy Commissioner- was escorting some high-level Russian visitors around the HILAC. They were meeting in Al's office right above our workspace when Judy, the group secretary, came rushing down to see if anyone had a radio. John Noble had been listening to his transistor radio while soldering. He turned it over to Judy so she could take it up to Seaborg, then told us what was going on. President Kennedy had been shot in Dallas! Seaborg rushed back to Washington that day, and for us, things pretty much ground to a halt for the rest of the day. We all went home for what turned out to be a 3-day weekend culminating in Kennedy's funeral on Monday.

Under the terms of the Work-Study program, my assignment ended in January when I returned to school to finish my junior year. But I was able to return for the following summer [1964] to pretty much the same job, earning a much-needed salary. That fall, I returned to school full-time for my senior year, after which I would need to find full-time employment. My nine-months stint as an electronics tech had set me on a definite course. I enjoyed hands-on engineering, a feeling that would never leave me over the course of the next 44 years.

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[6] Linear accelerators are waveguides for the RF energy, but the particles travel in the center section, shielded by "Drift Tubes" that prevent them from reversing direction on the negative portion of the RF voltage.

Return to the Rad Lab

Life is full of unexpected twists and turns. In the Spring of 1965, as I was job hunting while finishing my degree, I was approached by Link Harris, one of the HILAC engineers I had worked for. Link was an ambitious guy with a master's degree who felt hemmed in at his Rad Lab position. He had accepted a job at a Berkeley start-up called Alpha Scientific Labs, and wondered if I would like to come work for him there? The salary was good, the work interesting, and the location convenient. So, in the summer of 1965, having earned my degree, I joined Link in the industrial section of Berkeley, to begin my engineering career.

Alpha basically manufactured laboratory magnets and their power supplies, and instruments needed to measure their magnetic fields. I did a little design work on gaussmeters and was finally starting to find my balance when, at the end of 1965, I was let go as business started to sour. Link Harris, to his great credit (and my everlasting gratitude) pulled some strings at the Rad Lab and was able to get me a prompt interview the same day I was let go. There were two interviews: one with Quentin Kerns, who headed up a group working on devices such as spark gaps used for physics experiments; the other interview was with Dick Mack, who headed a couple of groups of instrumentation engineers who supported physics experiments. Unbelievably, I actually got to choose which of these jobs I wanted! I went with Dick Mack's organization, assigned to a first-rate group of instrumentation engineers headed by Fred Kirsten. Blind luck had set me on this path!

Fred was an excellent boss. He had grown up on a farm in Folsom, California. While in the service, he had participated in the 1954 Thermonuclear test on Bikini Atoll, after which he entered the nuclear instrumentation field at the Rad Lab. At the time I went to work for him, Fred was involved in supporting Victor Perez-Mendez [1923-2005], a physicist who specialized in developing equipment for physics experiments. I was assigned to supporting Victor and his group, who were then working with *Magnetostrictive-readout spark chambers*.[7] My first assignment involved updating an old wired-program data acquisition platform called Alpha-63. Racks full of logic cards had to be rewired to accommodate the spark chambers readout data, the result stored on an IBM tape drive, and the whole thing made ready for the upcoming experiment.[8]

The next major assignment was more interesting. I characterized a new set of spark chambers intended for use in an experiment at the brand-new Stanford Linear Accelerator [SLAC] in Palo Alto, tweaking their drive conditions for best performance. I also got to design some fast logic modules I called Delay-Register boxes, and was tasked with interfacing all the electronics to an

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[7] These were particle detectors consisting of He-Ne gas enclosed in a window of Mylar. Inside were two sets of wires, one horizontal, the other vertical. When a particle was externally detected as entering the chamber, a voltage pulse of 10,000 V for a few tens of nanoseconds was applied between the two wire grids, and a spark ensued. Current from the spark flowed through these wires, which passed over an external magnetostrictive wire, inducing an acoustic pulse in the wire. By timing the arrival of pulses from the horizontal and vertical magnetostrictive readout wires, it was possible to digitize the x-y coordinates of the particle's passage.

[8] As a side note: one aspect of this project resulted in my first-ever technical conference talk and publication: R. L. Van Tuyl, V. Perez-Mendez, K. Lee and R. L. Grove, "Delay-Line Multiplexing Method for Storage and Display of Information from Magnetostrictive Spark Chambers," in *IEEE Transactions on Nuclear Science*, vol. 15, no. 1, pp. 163-166, Feb. 1968, doi: 10.1109/TNS.1968.4324848.

IBM computer slated for use at SLAC. I made numerous trips to SLAC to set things up prior to the actual running of the experiment.

Back at Berkeley in 1967, I was tired of working for Victor on spark chamber projects, so asked for something different. Fred gave me a plum assignment, with an opportunity to work for a truly world-class physicist.

The Master Experimentalist[9]

Clyde E. Wiegand [1915-1996] was a regular guy, free of all pretension, and- unlike so many of his peers- Clyde personally ran all his experiments rather than delegating them to a squad of grad students. I felt honored and privileged to be able to help him with his work.

Born in Oregon, Clyde was, like so many tech-oriented kids of his time, interested in radio. In fact, he worked his way through Willamette College to a physics degree over a 7-year period [1933-1940] working full-time at radio-related jobs. While at Willamette, he read about E.O. Lawrence and his new $1M cyclotron. Taking the bull by the horns, in September, 1941, Clyde went directly to Lawrence in Berkeley to announce he wanted to work on the cyclotron. Lawrence told him to enroll in grad school and come back when he wanted a job. Clyde signed up for graduate school in September, and on Dec. 1, 1941, started work at Lawrence's cyclotron in a grunt-level job. As Clyde put it some years later "On Sunday following that week came Pearl Harbor. I've been with the Laboratory ever since."

At the beginning, Clyde worked on electromagnetic separation of U-235 from raw uranium. But after he had impressed physicist Emilio Segrè[10] with his electronics expertise, Segrè got Clyde transferred to his isotope characterization project. Clyde always considered his lucky break.

Clyde and the Manhattan Project

After a year and a half of classes and tech work, Clyde was called into the office of Prof. Robert Oppenheimer, who asked if he would be willing to go to the New Mexico desert as part of Dr. Segrè's group, to work on a war-related project [the Manhattan Project]. Clyde said yes.[11] So, starting in 1943, at Los Alamos, Clyde worked in Segrè's group along with another grad student 5 years his junior: Owen Chamberlain [1920-2006]. This team was tasked with researching fission – spontaneous fission in particular- rather than developing the bomb. They found that the first sample of Plutonium from the Oak Ridge laboratory which became available to them in 1944

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[9] Wheaton, B., 1977 AIP Oral History interview with Clyde E. Wiegand
https://www.aip.org/history-programs/niels-bohr-library/oral-histories/4962

[10] Segrè was at the time a refugee Italian Jew who had been director of the Physics Lab at the University of Palermo, where he discovered the first artificially-produced element, Technetium (in a sample irradiated in Lawrence's Berkeley cyclotron). At the pre-war Rad Lab, he was employed as a research assistant, where he helped to the discover Astatine and Plutonium 239. [Wikipedia: Emilio Segrè.] After the War, Segrè would be elevated to the status of professor and group leader at the Rad Lab.

[11] Even though Oppenheimer had never told him the goals of the project, Clyde had figured it out. Clyde stated that, for security reasons, they only referred to their workplace as "LA", leading people to assume it was Los Angeles. And the word "Uranium" was never uttered, even at Los Alamos, for security reasons.

exhibited 5 times the decay rate as did isotopically pure Plutonium, and would therefore be unusable in the gun-type weapon designed for the Uranium bomb. This key finding led to the very difficult-to-achieve implosion design [Fat Man] adopted for the Plutonium bomb exploded in the Trinity test, and later dropped on Nagasaki.

In 1969, as I was leaving the Lab to start my new career at HP, I went to say goodbye to Clyde. At the time, I knew nothing about his wartime work. When he asked me why I'd selected HP, I made some comment about how I would much prefer to work there than at a company like, say, Lockheed, which made weapons that could kill people. "It never bothered me," said Clyde. At that instant, I knew he must have worked on the bomb, and felt embarrassed for having said what I said.

In his 1977 AIP history interview, Clyde offered a more nuanced view of how he felt about the bomb being used:

> *On the morning after the bomb test, I met Oppenheimer at base camp. He was walking in the opposite direction from the way I was going, but he paused to say, "Clyde, we have to get some of these [bombs] over the Japanese cities."*
>
> *I think that this weapon should not have been used on the civilian population. I don't see the excuse for that. Maybe we shouldn't get into that. But how can the United States talk about morality around the world, when practically within a few days of having this weapon, the first thing we did was to throw it on the civilians?*

Clyde and the Anti-Proton

Clyde Wiegand was -and still is- most well-known for his co-discovery of the *anti-proton*. In 1955, Lawrence's Rad Lab had moved off campus to its permanent home in the Berkeley hills. Lawrence's 184-inch cyclotron was in operation there, along with the new giant synchrotron – called the *Bevatron*- capable of more than 6 GeV accelerated particle energies. This accelerator had been built with one experiment in mind: discovery of the theoretically-predicted *anti-proton.*

With the accelerator in place, two experimental groups were given a crack at finding the anti-proton. One of these groups consisted of Prof. Emilio Segrè, Prof. Owen Chamberlain, Dr. Clyde Wiegand, and PhD candidate Tom Ypsilantis. Chamberlain and Wiegand were the principal architects of the experiment. It was the cleverness of their experiment design that turned the trick. Bombardment products – hopefully containing anti-protons- were magnetically steered and focused in such a way that the time-of-flight could be measured between two scintillation counters for every particle. Only those particles whose velocities fell within a certain range, and which could be shown to not be due to accidental coincidence events, or events due to other particle types, could be deemed to be anti-protons. Accidentals could be excluded on the basis of probability, but the events due to the large flux of Pions needed to be flagged and rejected. The key device for ensuring the legitimacy of apparent anti-proton events was a specially-designed *Cerenkov Counter*, which was able to detect the velocity of particles by generating a light pulse when too-fast particles transited. Only events in which no Cerenkov pulse was detected qualified as legitimate.

In Clyde Wiegand's obituary, Owen Chamberlain, Tom Ypsilantis and Herb Steiner wrote about Clyde's contribution to the famous experiment:[12]

> *…no one deserves more credit for [the experiment's] success than he did. His design of the quadrupole magnets and novel velocity-selecting Cerenkov counter, his prowess with fast electronics and his ability to put together an experiment and make it work were crucial ingredients in the successful outcome of this experiment.*

This well-deserved acknowledgment made its way into Clyde's obituary mainly for one reason: he had been unaccountably denied the Nobel Prize in Physics in 1958 when Segrè and Chamberlain received the honor. So why didn't the Nobel Committee include Wiegand and Ypsilantis in the award? In Ypsilantis' case, it was because grad students were *never* awarded Nobel prizes. And the likely reason Wiegand was not included was his employment status: he was a staff scientist, not a professor. I believe that Chamberlain, a principled and fair-minded man, was deeply bothered by this slight of his colleague. Their personal and professional relationship weathered the storm. But in the hierarchical world of international physics, this was probably not the first, or last, incidence of such unfair treatment.

The Anti-Proton discoverers in their experimental bay at the Bevatron in 1955. L-R: Emilio Segrè, Clyde Wiegand, Ed Lofgren [manager of Bevatron development], Owen Chamberlain, Tom Ypsilantis. For obscure reasons, Segrè and Chamberlain received the 1958 Nobel Physics Prize for this work, but Weigand and Ypsilantis were excluded by the Nobel Prize committee.

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[12] Chamberlain, Owen; Steiner, Herbert; Ypsilantis, Thomas (January 1997). "Obituary: Clyde E. Wiegand". *Physics Today*. **50** (1): 79–80. https://pubs.aip.org/physicstoday/article/50/1/79/409655/Clyde-E-Wiegand

The Bevatron, a 6 GeV synchrotron used from 1954 to 1993, when it was decommissioned prior to demolition. In its early days, the Bevatron was used by Clyde Wiegand and others to discover the anti-proton [1955]; starting in 1971, the HILAC was used as an injector of heavy ions to the Bevatron, which accelerated them to GeV energies. This combination was christened *BEVALAC* by Al Ghiorso.

Wiegand's Mesonic X-Ray Experiments

In 1963, theoreticians had predicted that exotic atoms could be created in which negatively-charged *mesons* replaced electrons in their atomic shells.[13] Emilio Segrè suggested to Clyde that he might want to try to study these *mesonic* atoms. Clyde set out to study these atoms by observing x-rays emitted as the unstable atoms decayed. For this, he would need a means to detect x-rays. Fortunately, an engineering group at the Rad Lab[14] had significant expertise in fabricating Lithium-drifted silicon detectors optimized for x-ray detection.[15] Clyde used his experimental expertise in 1966 to design a system for irradiating various solid target samples with a beam of K-mesons, and detecting the resulting x-rays emitted by the targets.[16] This initial experiment proved the feasibility of his approach, which led to a number of improvements in the experimental setup with a view toward collecting a much larger data set.

This is where I came into the picture. In 1967, I had asked my boss, Fred Kirsten, for an assignment not related to spark chamber projects. The timing was just right for stepping in to help Clyde Wiegand with his data acquisition task for the improved K-Mesonic x-ray experiment. I was empowered to select and purchase type of digital tape deck that could handle event-by-event data

[13] Y. Eisenberg and D. Kessler, Phys. Rev. 130, 2352 (1963).
[14] Headed by Fred Golding.
[15] These are basically large p-i-n radiation detectors, with apertures measured in cm, where Lithium doping has compensated for residual impurities, in order to create a large intrinsic volume for generation of hole-electron pairs by absorbed radiation, e.g. x-rays.
[16] Wiegand, C. and Mack, D. A., *Measurement of K- Mesonic X-Rays from Li, Be, B, and C,* UCRL-17390, 1967.

storage, and to design and build the necessary interfaces between Clyde's digitized x-ray detector outputs and the tape deck.

Given my experience with data interfacing, this would be a straightforward task. But I took advantage of the opportunity afforded by a non-critical schedule and the degree of independence I was given, to design an enhanced add-on to the system: a dedicated pulse-height analyzer display. This design, which was the best design I did at the Rad Lab, used a magnetostrictive wire to form a 20,000-bit serial memory capable of storing and displaying Clyde's data in the form of an x-ray spectrum.[17] I was able to observe Clyde Wiegand's hands-on approach firsthand in the experimental bay, where he was on top of everything. He had minders to watch the data collection when he was not present, but basically, it was a one-man operation. Not surprisingly, the results were outstanding, and were published internally and externally, along with Clyde's detailed analysis and connection to theory.[18,19,20]

My work for Clyde Wiegand was the high point of my Rad Lab experiences. Not only did I do my best design work, I was able to directly assist a fine scientist to do his outstanding work. I remember Clyde fondly to this day.[21]

1968: A Big Year

On March 31, 1968, President Lyndon Johnson announced he would not run for re-election. The country – and especially Berkeley- was coming apart under pressure of opposition to the Vietnam War. I have, etched in my memory, a vision of walking between buildings at the Rad Lab, fearing the country was coming apart. Johnson's announcement allowed me to breathe a sigh of relief.[22]

I had returned to doing spark chamber work for Victor that year. The task was to get his latest brainstorm, called the *Sparkostrictive* chamber, to work.[23] It was doomed from the start. It required the formation of three sparks in series, one within the chamber itself, and one between each of two chamber wires and their corresponding acoustical readout wires. The whole reason for such a design was simple: magnetostrictive wires would not function in the presence of a magnetic field, and an upcoming experiment called Ke4 would be placing spark chambers in the jaws of a giant

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[17] Today, the use of a magnetostrictive serial memory seems primitive, but at the time, semiconductor memory was not available, and magnetic core memory was complex and bulky. The serial memory– along with my custom-designed logic- did the job quite well. However, this was just for monitoring purposes. Actual data for the experiment was what had been stored on the magnetic tape drive.

[18] Wiegand, C. E., Measurement of K- Mesonic X-Ray Spectra of Medium and Heavy Elements, UCRL-18782, 4/8/1969. https://escholarship.org/uc/item/3xx7g7j1

[19]Wiegand, C. E., Measurement of K- Mesonic X-Ray Spectra of Medium and Heavy Elements, *Phys. Rev. Lett.* **25**, 1235, 1969 9 June, 1969

[20] Wiegand, Clyde, Exotic Atoms, Scientific American, November, 1972

[21] Gary L. Godfrey, a scientist who, as a grad student, worked with and published papers with Clyde in 1972-76, concured. https://foothill.edu/psme/marasco/OHSP/godfrey.html

[22] Of course, the war would drag on for another 7 or so years under Nixon, but the societal pressure was somehow relieved when LBJ opted out in 1968.

[23] Grove, R., Kauffman, L., Perez-Mendez, V., *Sparkostrictive spark chambers with piezoelectric readout*, Nuclear Instruments and Methods, vol. 62, issue 1, June, 1968, pp. 105-108. https://www.sciencedirect.com/science/article/abs/pii/0029554X68906228#!

magnet.[24] I was able to nurse the chamber into operation by creating a lengthened 10kV trigger pulse. Further, I was able to improve the reliability of forming the two readout sparks by building small autotransformers to increase the potential between chamber wires and readout wires. The chambers worked, barely, but were not really in any condition to be used in an actual experiment. But Victor had arranged for a collaboration with a University of Hawaii group headed by a newly-hired UH professor named Bob Cence, and the Ke4 experiment was put on the Bevatron schedule. For months, I worked alone, then with postdoc Brian Jones and UH grad student Rich Morgado, in a vain attempt to get these things going. The three of us bonded, sort of like I would imagine soldiers in a foxhole might bond during a hopeless battle. But in the end, the experiment limped into operation with unreliable detectors.[25] The contrast between this experiment and Clyde Wiegand's experiment was sharp and telling.

Victor Perez-Mendez [L] poses with a Magnetostrictive-readout spark chamber, along with PhD candidate Johnny Sperendi, in Building 80 (1968). Johnny's experimental setup area was adjacent to the one I used along with Brian Jones and Rich Morgado in a vain attempt to get Victor's *Sparkostrictive* Chambers to work reliably.

But 1968 was the year I needed to do an original research project in order to complete the requirements for my Master's degree in Electrical Engineering. This involved a lot of time away from the Rad Lab, which I spent on-campus fabricating an experimental transistor-like device as part of my thesis research. This was extremely educational. I learned about semiconductor processing from hands-on experience, a skill I would be called on to use for many years in my engineering career. After fabricating the device and having it packaged in the EE Department, I

[24] The chambers were to be mounted on a giant sheet of inch-thick aluminum to support them in the jaws of the magnet. One day, as things were being set up, the magnet power supply failed, inducing eddy currents in the aluminum sheet, causing it to levitate while the mechanical design engineer stood on it. He ducked, and was not injured. But it was a sort of omen for the future of the Ke4 experiment.

[25] R.J. Cence, F.A. Harris, B.D. Jones †, R.E. Morgado +, L.M. Shiraishi, D.E. Yount, V. Perez-Mendez, R. Van Tuyl §, D.B. Clarke **, *A Sparkostrictive wire-chamber Spectrometer*, Nuclear Instruments and Methods, Volume 111, Issue 2, 15 August 1973, Pages 379-391
https://www.sciencedirect.com/science/article/abs/pii/0029554X73900864?via%3Dihub
This publication described the Sparkostrictive chambers this way: "The chambers, while capable of one-track efficiencies of virtually 100% and three-track efficiencies averaging better than 98% per track, are extremely delicate and require constant attention." An understatement if ever there was one.

characterized it during many evening hours spent at the Rad Lab. The result of this was my thesis "Semiconductor Variable Delay Elements."[26]

Conclusion

In the Spring of 1969, I left the Rad Lab, headed to Palo Alto California, the home of my new employer, Hewlett Packard Company. Why? HP was an engineer's company; the Rad Lab was a Physicist's domain. At HP, I could play a lead role. At the Rad Lab, I would always have been a support person. For the next 40 years, I worked for a great company, but I never forgot the formative experiences I had at the Rad Lab.

In this memoir, I have tried to summarize my experiences as a student intern and novice engineer during a special time in history, at a special place with a history of its own. The Lawrence Radiation Laboratory was a product of postwar support for basic atomic science. The research dollars have moved elsewhere since then, but research continues, though without those two redoubtable machines: the HILAC and the Bevatron. The people I met there, the experiences I had, and the lessons I learned, will be with me forever. It was, in retrospect, an amazing experience.

Acknowledgement

Special thanks to Peter Robrish for sharing his memories of the 1960s Rad Lab in the Segrè-Chamberlain group, and for explaining some details of the anti-proton experiment to me.

[26] Van Tuyl, Rory Lynn, Semiconductor Variable Delay Elements, UCRL-18668, December, 1968. https://escholarship.org/uc/item/8bs3w7h5

Appendix

Memories of Luis Alvarez

Rory Van Tuyl 9/14/2023

I worked in the position of instrumentation engineer supporting physics experiments at the Lawrence Radiation lab, Berkeley, from January 1966 to March, 1969. The job was quite varied and interesting, with opportunities to not only design electronic instrumentation, but also to characterize and improve detectors called spark chambers. As a bonus, I was able to attend classes on the Berkeley campus of UC, where, after 3 years of part-time study and research, I earned the MSEE degree.

The Lab was full of Nobel Prize winners, such as Ed McMillan, Owen Chamberlain, Emilio Segrè and probably others. But perhaps the most colorful of the bunch was Luis Alvarez, the father of the Hydrogen Bubble Chamber.

At this time, Alvarez was in his 50s [b. 1911], and though quite famous and well-regarded, had yet to earn his Nobel [it was awarded in 1968, near the end of my Rad Lab tenure]. The accomplishment that won him the prize was the development of the Hydrogen Bubble Chamber.[27] But the group he headed was at that time involved in some offbeat experiments, such as high-altitude cosmic ray detection, searching for hidden chambers in the Great Pyramids, and the quest to discover the Magnetic Monopole. My office mate did a lot of work for the Alvarez group and held them in low regard. [This attitude was common among us engineers, and not limited to the Alvarez group, as we observed the slapdash efforts of physicists to get things done].

But Alvarez was a brilliant man, a characteristic he demonstrated during the time I was at the Rad lab. The Zapruder Film of John F. Kennedy's assassination was filmed with a cheap hand-crank movie camera. The film speed was crucial if one wanted to determine the time between shots that hit Kennedy. Alvarez noted – based on frames of the Zapruder film published in Life Magazine- that the film speed was correct, based on observation of people clapping in frames of the film. Alvarez determined the fastest one could clap and used that as a sort of metronome inherent in the film. (He was roundly booed by conspiracy theorists).

I met Alvarez in person only once during my time at the Rad Lab. When I attended classes on campus, I would walk down the hill to campus, and hitchhike back up. One day, Alvarez picked me up, which I much appreciated. But he was stone cold at my response to his question about where I wanted to be dropped off. It turned out that my office was in the same pair of buildings as his, and of his bubble chamber analysis group. Films of particle tracks from the chamber were analyzed by humans called "scanners"- mostly impoverished grad students doing this tedious job just for the money. When I told Alvarez I was going to the same building as he, his demeanor changed to one that struck me as a sort of contempt. I think he assumed I was a "scanner" -hence low on the pecking order of the lab.

[27] Invented by Donald Glaser, made practical by Alvarez and an army of engineers and physicists under his direction.

My only other personal contact with Alvarez was quite different. It turns out that Alvarez, then in his 70s, served on the Hewlett-Packard board of directors during the 1980s, during which time I was a project manager at HP's Santa Rosa site. I had read Alvarez's 1987 autobiography when it came out, and was lucky enough to get a seat across from him at a picnic table during a post-board meeting dinner with local employees in Santa Rosa. I mentioned the book, which led to us talking about his work on the Atomic Bomb during WWII. Alvarez had been assigned to measure the size of the Hiroshima blast. He and his team developed a complex system for measuring the blast from a sensor package dropped shortly after the bomb itself. The results were to be radioed to a receiver in the chase plane, in which Alvarez was riding. But the system never worked correctly in pre-bombing tests. His group were confident, however, that the last glitch had been corrected the night before the B-29 took off for its bombing run. They were right. The detectors that were dropped from the chase plane determined the bomb yield to be 13 kilotons. But the Trinity test had logged in at 20 kilotons, and this was the number President Truman used in his announcement, not the number measured by Alvarez. This greatly displeased Alvarez. Subsequent analysis of blast shadows on the ground tended to support Alvarez's measurement, and the historical record has now been corrected.

The following year, 1988, was Alvarez's last. I feel lucky to this day to have had the chance to talk to and work around this brilliant man.