An APPLIED SCIENTIST'S CAREER

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PREFACE

I am an experimental physicist by education, briefly worked in academia, but during most of my career in industry I applied scientific methods to solving physics and electrical engineering problems. My journey passed through two universities, a two-year hitch in the U.S. Army, a startup division of a huge corporation, a small entrepreneurial company that grew and was eventually bought by a large aerospace firm, a brief stint as a government civil service employee, and eighteen years as a private consultant. I learned to develop ways to solve some of the U.S. government's technical problems, primarily for the Department of Defense, and secured government funding for my organization to perform the necessary applied research. I hired and trained a technical staff, some of whom became skilled in developing and marketing their own solutions. Some times were difficult, but as a whole I enjoyed the journey, especially the technical challenges.

My story contains two threads: technical and managerial. The technical side follows the work topics, which were usually determined by the needs of the marketplace, i.e., what research areas were of current concern to government funding agencies. The managerial side follows the evolution of the organizations I chose to join and the development of contract support in my work areas. In retrospect many choices seem almost accidental; they were casual decisions that led to long-term developments. One choice was deliberate: I chose to remain primarily a scientist/engineer rather than allowing myself to become a full-time manager. At some times the management demands on me could easily have overwhelmed technical opportunities, but my choice to spend a significant part of my time on technical issues preserved my skills and eventually enabled me to extend my career as a productive technical consultant into my 80's. I justified this choice by devoting significantly more than a 40-hour week to working, but it came naturally: the technical challenges were fun; management was a necessity. Fortunately, at critical times I was supported by John Chiment, a very capable management assistant, who advised me on policy and executed the day-to-day management required by my organization.

EDUCATION

Figure 1. Immigration photo

Elementary school was a time of changes. The first three grades of my schooling were in the Netherlands as the only Protestant child in a Catholic school. Then in 1937 my family emigrated to the U.S.A. and settled in a small mining camp on Mt. Baldy in northern New Mexico at 10,000 ft. altitude. I was immediately enrolled in their one-room schoolhouse. On the first day the teacher dealt with the challenge of a Dutch-speaking child by exposing me to material from each of the first four grades. My background in arithmetic was well beyond the U.S. fourth grade, and I knew enough phonetics to recognize which word matched a spelling, even if I didn't know its meaning. The teacher finally decided to insert me into the fourth grade with some supplemental third-grade English lessons. One girl went home and told her family, "A new boy in school just passed three grades in one day!" Later we moved to a small town, Cimarron, NM where I became a minority student amid a mostly Hispanic population. I was bored in class and miserable on the playground. My know-it-all attitude didn't help me with my classmates.

Learning English was a challenge but I was well motivated once we arrived in New Mexico. I studied a little during the voyage across the Atlantic on the MS Volendam, but immersion in an English-speaking school provided a strong incentive. I still remember having difficulty with the word, "cloud", so the teacher took me outside and pointed upward. I never forgot it. Since then I've not become even marginally fluent in any other language, even after a two-year high school course in Spanish, a one-year college course in German, and a few weeks of tutoring in Russian. I regret that my parents spoke English at home to facilitate my conversion, because I lost most of my Dutch. Only when I visit the Netherlands do a few words bubble up from my subconscious memory. Eventually I discarded most of my Dutch accent. During high school a tutor taught me how to hold my tongue while pronouncing "th" instead of "d" or "s" and how to soften my "a". But I still prefer a harder "a" on "amen" and "aunt".

My schooling was interrupted by discovering that I had tuberculosis. It's fortunate the immigration people at Ellis Island didn't detect it when we arrived in the U.S.A. in 1937, because I would have been turned back. The only cure in the early 1940's was bed rest. Fortunately, the New Mexico climate was favorable, so I spent 13 months in bed at home, followed by months of gradual recovery. Home study enabled me to attend the local high school for three months after my recovery and receive credit for three years, one year more than the elapsed time!

Figure 2. Cadet at NMMI

I still don't know how my father talked the school principal into granting me the certificate of completion, but it enabled me to be accepted as a high-school junior at the New Mexico Military Institute (NMMI) in Roswell, NM, probably the best high school in the state. Its military discipline was secondary to my education, since I had been well trained to obey my father. I attended NMMI for three years, the last one a year of junior college that compensated for the missed year of high school. It prepared me for the challenges to come in a four-year college.

In the fall of 1946 I was accepted at California Institute of Technology (CalTech) in Pasadena, CA as a freshman majoring in mechanical engineering. Those of us who were recent high school graduates found ourselves in a freshman class with former GI's returning from fighting a war. The average age of our freshman class was higher than that of the current senior class. My student-house roommate was a former air-corps bombardier. He worked hard during the week and drank hard on weekends.

During my years in Roswell I had attended the local Presbyterian Church and enjoyed participating in their high-school fellowship group, so it seemed natural to visit the Sunday-evening meeting of the Fellowship Hour, a young-adult group at the Pasadena Presbyterian Church. I was drawn to notice a tall, longhaired blonde girl seated a few rows in front of me. A few weeks later the Fellowship Hour group spent a weekend at the church's beach house on Balboa Island and I arranged to sit next to her at dinner. I managed to offend her with a smart-aleck remark, so it took a while before she forgave me, but June Woolhouse eventually agreed to be my wife. We married the day after I was graduated with a B.S. almost four years later. Meanwhile we enjoyed Sunday evening meetings and other events with the many friends we met at Friendship Hour, and attended the CalTech dances and other social events. Once I had a car, in my third year at Caltech, we enjoyed sightseeing around southern California on Saturdays.

Figure 3. Engagement

All CalTech students were required to take general physics and chemistry courses during their first two years. Toward the end of the first year it dawned on me that physicists could connect basic entities of matter (e.g., atoms, molecules, electrons) with macroscopic properties (e.g., pressure, density and temperature of materials). That realization inspired me to change my major to physics, much to the dismay of my father, who wanted me to become a civil engineer. That was one of my best decisions.

In the spring of 1949 I asked one of my particularly inspiring instructors, Dr. Robt. Leighton, if students could volunteer to work in the laboratories. He accepted my offer and introduced me to two of his program's technicians. One was a master machinist, who could do wonders with lathes, mills, and other machine tools. The other was a "good-enough" expert; he could also operate machine tools and cut, solder and glue items to perform their required functions, but without the rigorous standards of the master machinist. They teased each other incessantly, but each was an invaluable tutor. My first job was threading a long ½" diameter copper rod to make studs for a board to interconnect copper wires for a magnet. I quickly learned the importance of a good lubricant to prevent jamming the threading die. Perhaps this was also a test of my patience. Apparently, I passed and was hired for the summer to help build cloud-chamber equipment in a trailer to be operated on nearby Mt. Wilson. This work continued part-time during my senior class year. I'm sure it also helped to get me accepted for graduate school by CalTech, fortunately, because I was stupid not to apply anywhere else.

Figure 4. Wedding

I graduated with a B.S. in Physics on June 9, 1950 and was married to June Woolhouse the next day. We moved into a small house previously rented by a classmate at 1663 Corson St. Its location is now under the Interstate 210 freeway.

I received a teaching assistantship during the first two academic years as a graduate student plus a salary for summer laboratory work. June was employed as a bookkeeper at a car-sales company, so we were able to support ourselves. During my third and fourth years as a graduate student I was awarded a National Science Fellowship, which paid $2000 for the academic year. We felt so rich that June moved to a job at the Huntington Library, which paid less but was much more pleasant.

The cosmic-ray work became the basis for my Ph.D. thesis, monitored by a triumvirate of Drs. Carl Anderson, Bud Cowan and Bob Leighton. I helped assemble a large magnet enclosing three large Wilson cloud chambers separated by lead plates. The combination provided about five cubic feet of cloud chamber volume in a magnetic field of nearly 10,000 Gauss. It was designed to observe the products of interactions in the lead plates produced by incoming energetic cosmic rays. My emphasis was on what were then called Lambda particles, short-lived neutral particles that did not produce a track in the cloud chamber but decayed into positive protons and negative pi mesons, which produced tracks curved in opposite directions by the magnetic field. The cloud chambers were triggered by an array of Geiger counters. We attempted to select events in which multiple particles were created in one of the lead plates by requiring several counters to fire coincidently. I scanned approximately 30,000 stereoscopic cloud-chamber photos to find a few hundred interesting events.

I looked intently, but unsuccessfully, for evidence of an antiproton, a negatively

Wilson Cloud Chamber

A cloud chamber produces a supersaturated vapor, usually by suddenly expanding a volume of saturated gas. After all dust particles have been removed the droplets form preferentially on charged ions and electrons, thereby decorating the tracks of charged energetic particles. Neutral particles leave no visible tracks.

Cosmic Rays

Cosmic rays are very energetic particles (protons, electrons, heavier nuclei, etc.) that bombard the earth's atmosphere. When a charged particle passes near an atom it may rip off an electron, leaving behind one or more pairs of electrons and positive ions. The collision of either a charged or neutral particle with an atom's nucleus may produce nuclear fragments and may create short-lived unstable particles, which we used to call mesons, but now have individual names, such as mu, pi, tau, etc. After I graduated particle accelerators, from the Cosmotron to the current LHC, have produced beams with particles of energy gradually increasing from 3 GeV to 7 TeV. Not only are the accelerated particle intensities much higher than cosmic rays, they arrive with nearly fixed energy at a predictable place and time. No longer does the experimentalist have to wait for a lucky event to occur in his equipment; the penalty he pays is to be a small part of the very large staff needed to operate and justify an expensive facility.

Particle Energies

The energy of a nuclear particle may be measured in units of electron-volts (eV), the energy gained by a unit-charge particle (e.g., electron, proton) in falling over a one-Volt potential drop. Practical units include keV, MeV, GeV and TeV, each one larger than the previous one by a factor of 1000.

charged proton. One of my advisors, Carl Anderson, had won the Nobel Prize for discovering the positive electron, the positron, in the early 1930's. A cloud chamber photo reveals a charged particle's momentum by its curvature in a magnetic field, and the particle's mass by the density of the track for a given momentum. The sign of its charge determines whether a particle's track curves clockwise or counterclockwise in the magnetic field, but the direction in which it is moving must be known. Therefore, we hoped to see multiple particles diverging from a common source in a lead plate, indicating an energetic collision, that included one negative particle with mass near the proton's. I was unable to find even one, although there were some tantalizing near misses. The antiproton was eventually produced by the Bevatron accelerator at the University of California, Berkeley and its discovery earned Emilio Segre and Owen Chamberlain a Nobel Prize.

My measurements on the decay of Lambda particles were reported in a Physical Review article[1] and at a Duke University conference. We also published a Physical Review Letter describing two unusual events called "cascade" particles[2]. An energetic charged particle suddenly changed direction, indicating that it had emitted an unseen neutral particle, which decayed after a short flight into two observed tracks, one negatively and one positively curved. My thesis[3] included the analysis of these events, as well as some theoretical materials used in the data processing. June typed it using an orange carbon backing on each page. This produced a better reproduction with the Ozalid machines in use at the time, but made correcting typos very difficult.

Figure 5. Graduate student

Prof. Richard Feynman, a Nobel-prize winning theoretical physicist, was the most intelligent person I've ever met. It was a little overwhelming to learn that he was a member of my oral exam committee. He asked me about the theory sections in my thesis, but I assured him that I had only converted existing theory into a form useful for analysis of experimental data. So he asked me, "What temperature would a black body reach at the earth's distance from the sun?" I stumbled trying to remember the value of the Stefan-Boltzmann constant, so he gave me a hint and I got the answer without using the constant. He was satisfied. I was awarded a Ph.D. in physics in June 1954. June was awarded a PHT (Put Husband Through) signed by Mrs. Dubridge, the CalTech president's wife.

UNIVERSITY INSTRUCTOR

[1] V.A.J. van Lint, G.H. Trilling, R.B. Leighton, C.D. Anderson, *Q-Value of Λ^0 decay,* **Phys. Rev. Vol. 95,** p. 295 (1954)

[2] V.A.J. van Lint, C.D. Anderson, E.W. Cowan, R.B. Leighton, C.M. York Jr., *Cloud chamber observations of some unusual neutral V particles having light secondaries,* **Phys. Rev. Vol. 94,** p. 1732 (1954)

[3] V.A.J. van Lint, *Observations of neutral V-particle decays with 48" magnet cloud chambers,* **Calif. Inst. of Tech. thesis** (1954) (available on Internet)

June and I decided I would accept an offer to be an Instructor at Princeton University. We loaded our belongings and two cats into our car and travel trailer, and started the drive across the nation. We paused for a month at Echo Lake, near Denver, CO to join a group performing cosmic-ray experiments at an altitude near 10,000 ft. There we enjoyed meeting other faculty and graduate students from Princeton, Syracuse and other universities. On weekends we visited local sights, including nearby Central City and Rocky Mountain National Park, breaking in a Roleicord camera we had bought in celebration of graduation. After a month in Colorado we drove to Princeton via a detour through Yellowstone National Park.

Figure 6. In Colorado

At Princeton I worked in the cosmic ray group led by Dr. George Reynolds and taught general physics under the leadership of Dr. John Wheeler. I was somewhat disappointed that a Princeton Instructor's role in class sessions was limited to explaining the problems assigned by the Professor, whose lectures three times a week in a large auditorium taught the subject matter. At CalTech graduate-student teaching assistants were the primary teachers in freshman and sophomore general physics courses. The once-weekly lectures by the supervising Professor in the large lecture hall were used for inspiring demonstrations.

We met many famous scientists at faculty get-togethers. I remember Dr. Eugene Wigner commenting that I should learn to drink. I also attended some lectures at the Institute for Advanced Study, including one by Dr. Julian Schwinger, who a few years later shared the Nobel Prize with Richard Feynman for their quantum electrodynamics models. Dr. Robert Oppenheimer chaired the seminar. He had a long neck and appeared to turn his head around without rotating his shoulders. I appreciated the explanations he provided during the lecture in the former of questions, such as, "Do you mean ……..", because I couldn't follow Schwinger for more than one sentence. Schwinger's lecture style was the opposite of Feynman's, who was always entertaining and informative. The only trouble with Feynman's lectures was that he made you think you understood the subject while you had probably missed a subtlety needed to solve problems. I had no doubt I didn't understand Schwinger's.

June's cousin Gladys was to be married near Providence, RI a few weeks after our arrival in Princeton. June had listed for me more than 50 of her relatives as close as first cousins who were expected to attend. We drove up on a weekend with our travel trailer, which June's cousin, Shirley Moran, and her husband, Jack, volunteered to sell for us at their trailer agency. I met June's aunts, uncles, cousins, nieces, nephews and her grandmother Place, and tried to remember as many of them as I could.

Before we returned to Princeton, June's aunt Hattie and her husband Earl Blinkhorn invited us to spend a weekend on Narraganset Bay aboard their 55 ft. twin-engine boat. We accepted and returned a few weeks later and, together with June's sister, Dot, spent a pleasant Saturday on board. We enjoyed the cruise in the Bay and swimming in a sheltered cove on an island in the bay. Sunday turned out differently: Hurricane Carol, which had been hanging off the

Georgia coast, had suddenly accelerated and arrived at Narraganset Bay early in the morning. The latest forecast predicted 50 mph winds around noontime, so Earl judged it to be safest for us to remain moored in the cove. Actually the wind speed exceeded 100 mph by 10 a.m., the water level rose sufficiently to lift the mooring off the bottom and Earl had to maneuver the boat with zero visibility while dragging a mooring. Eventually, weeds that had been swept onto the water surface plugged the cooling water intakes and Earl used the last of the engines' capacity to steer the boat onto a sandy beach. We moved over in the deckhouse to drop onto the beach just as the waterside windows were shattered by logs that had also been swept out by high water. We spent the next few hours hiding behind a one-foot high sand hill. After the wind quieted an island resident offered us his home for shelter. That evening the ladies were taken to the mainland by the owner of a small boat with a 50-hp outboard motor. The men went to sleep on the floor. In the early morning hours a friend of Earl's came to search for us in his boat, which had survived the storm. After seeing Earl's abandoned boat he checked at the nearest house, found us, and brought us back to Earl's home.

Figure 7. The "Skunk" partially repaired

My new Roleicord camera was ruined by salt water during the storm, but I had already become addicted to it. A few weeks later we visited New York City and bought a Roleiflex to replace it, which I used for thirty years before switching from its 2¼" by 2¼" format to a 35mm Nikon. Later it took only twenty years for me to be converted to a digital camera enthusiast. I'm now scanning the best of my old slides and pictures into a digital format.

My Princeton career came to a sudden end when I was drafted into the Army on January 24, 1955. The first semester final exam was administered to my students on a Monday. I reported to the Army the next day, so another Instructor had to grade my student's papers. I was sent to Ft. Dix, NJ and then to Ft. Jackson, SC for basic training. In anticipation of my being drafted, Professor Wheeler had contacted an Army friend who promised to help direct me to a useful assignment. I had toured a few installations, including Army Chemical Center, Aberdeen Proving Grounds and Ft. Monmouth. My first choice was a mysterious classified Project Matterhorn at Princeton University, which included some military personnel on its staff. I was not impressed by the Army labs I visited, so my second choice, sight unseen, was Sandia Base, NM, where the defense department managed the testing of nuclear weapons. I was assigned there after basic training. I learned years later that Matterhorn was the beginning of magnetically confined controlled thermonuclear fusion. If I had been assigned to it I would probably have followed a totally different career path. Two other members of my graduating class became experts in controlled fusion, John Greene and Ralph Lovberg.

ARMY PRIVATE

Ft. Jackson was cold in winter. After they shaved my head I wore a cap at night to keep my head warm. We marched, exercised, trained and were harassed regularly by the cadre. It was tough at 26 to keep up with the younger kids fresh off the high-school football team. The company Commanding Officer (CO) offered a 48-hour pass to those with the top three scores on the rifle range. June was expected to arrive the same weekend, so I was well motivated to get into condition. My background on the rifle team at Roswell got me the top score. The same week the CO received the scores on our IQ tests, and mine must have been above average, since he called me in to find out about me and suggest I go to Officer Candidate School. I declined that four-year commitment in favor of serving out two years as an enlisted man. Anyway, from this point on he enjoyed introducing me to his fellow officers as his “Princeton Professor”. Of course, the sergeant made sure that I understood he was the real boss.

I also became a squad leader with some troubled recruits: a Philadelphia youth given a choice between jail and Army, and a pleasant boy whose IQ could not have been over 70. During our stay at the rifle range we camped in foxholes with shelters. The orders were that everyone had to qualify before the company could return to barracks. Those who didn’t qualify on the first round had to fire the whole program again, while those who did qualify coached them and scored the targets. The scoring was generous, because the men in the target pits were also eager to leave. The CO had to adjust some of the second-round scores, because some first-round non-qualifiers scored as Expert on the second round, including the low-IQ fellow I coached. He closed his eyes each time before pulling the trigger. If anything hit his target it was the rocks kicked up by his bullets impacting the dirt in front of the target pits. The prospect of this kid in an infantry squad bothered me, so I asked a friendly career Army sergeant what would happen to him. He said, “He’ll be assigned as a cook’s helper, so he won’t be in combat”. I hope he’s right.

Figure 8. You're in the army now.

Another flap as a squad leader came just before my weekend pass. A member of my squad had also earned a pass, but an Army Reserve sergeant, who was also undergoing basic training, ordered him to shine the sergeant’s shoes. I went to the platoon sergeant and told him that I would not be a squad leader if required to pass on such orders: the man had earned his pass and should not be diverted from it. The reserve sergeant shined his own shoes!

After 8 weeks of basic training June and I returned to Princeton, picked up a 5x12 ft. rental trailer at Shirley and Jack’s place, packed up our belongings, and drove to Albuquerque with our two cats. The first problem occurred soon after we left the Pennsylvania Turnpike, while driving on a two-lane road winding up one of the Allegheny ridges. Around a curve we came up behind a truckload of pipe traveling at 3 mi/hr. Our ’48 Plymouth couldn’t go that slow pulling a heavy trailer, so I stopped and waited for the truck to reach the crest.

Figure 9. Arriving in Albuquerque

Then I couldn't get started on the hill; the clutch would burn out if I kept trying, so I backed about two miles down the hill to a flat spot while June held on to the cats and warned me about traffic.

Our second problem was a tire blowout on the trailer. We bought a used tire to replace it, but it turned out to be rotten. Another used tire survived until we arrived in Albuquerque, where our belongings were exposed to a typical spring dust storm. There is still fine dust in some of my books.

Figure 10. In my dreams

On reporting to Sandia Base I learned that my security paperwork had been lost, so clearing me for access to classified information had to start over. While waiting for clearance, I was assigned to the Base Engineer's office, much to the dismay of the company first sergeant, who expected such in-limbo enlisted men to be assigned to fatigue duty under his supervision. The Base Engineer assigned me to make an inventory of electric power equipment: utility poles, cross arms, transformers, etc. I plotted the data on a map, then divided the map into rectangles, and tabulated for each rectangle the equipment located within it (e.g., the numbers of 25 and 30 ft. poles, 6 and 8-ft cross arms, 15 and 25 kW transformers). This work resulted in a pile of paper more than an inch high. I considered it classic busy-work, but was informed later that the whole pile had been submitted to higher authority as part of the Base Engineer's annual report. I thought surely they would be criticized for wasting time and paper, but the response from higher authority was, "Why hasn't a similarly thorough analysis been done of the water and sewer systems?" Fortunately, I had been cleared by then and re-assigned, avoiding the prospect of measuring the depth of sewer manholes and the diameters of sewer pipes.

One of the engineers and I made a cost-saving suggestion. The rate paid to the local utility for each kilowatt-hour of electricity was determined by the peak demand, i.e., the highest rate of power consumption during the month (i.e., kilowatts). A significant fraction of the electricity was used to pump water from wells, and a large fraction of the water was used by base personnel to water the common lawns. We recommended that the employees who watered the large public grass areas be paid a bonus to work during early morning hours, i.e., prior to the normal workday. That would decrease the peak demand, which always occurred during the hottest part of the day. We estimated that the resulting decrease in electric power rate would save tens of thousands of dollars. We never received a response.

> ***Fallout Prediction***
> *Fallout prediction requires a knowledge of the amount of radioactivity carried by particles of each size (determined from measurements on fallout material), combined with a model for lofting the material (inferred from cloud photography), the wind profile (measured by weather balloons), and the rate of fall as a function of particle size (known from laboratory experiments).*

Finally, after about six months at the Base Engineer's office, my clearance arrived in late 1955 and I was transferred to the Weapons Test Division. Intense efforts were under way to prepare for the Operation Redwing series of nuclear tests at the Pacific Proving Grounds (Eniwetok and Bikini Atolls) to be conducted the following summer of 1956. I was assigned to the Program 2 office, headed by Navy Cdr. Donald Campbell, which coordinated radiation measurements, including fallout. Some islanders and fishermen had been inadvertently exposed to radioactive fallout during the Castle test series two years before, so a large effort was planned to measure the fallout distribution and to develop fallout pattern prediction techniques. The prediction outputs were to be compared with ground truth, i.e., measurements of radioactivity and particles collected at the ocean and island surface. The resources to be deployed included aircraft to measure radiation levels above the ocean and islands, small missiles to measure radiation levels in the debris cloud, skiffs with deep-sea moorings developed by Scripps Institution of Oceanography to collect particles, and ships with heavily shielded control rooms to measure radiation levels and collect fallout. Our Program 2 office scheduled the resources and coordinated all projects. While helping to fill in scheduling forms for helicopters, trucks and boats, I familiarized myself with existing fallout models.

Even this drudgework was better for me than the Base Engineer's power survey or the first-sergeant's tasks of cleaning latrines and pulling dandelions from the company barracks lawn, but I learned later that I had inadvertently offended the Air Force major who was also assigned to the Program 2 office. He had objected vigorously to his assignment, asserting that, as a recent Ph.D. graduate, there was no work suitable for him in this organization. My eagerness to assist and study fallout models undercut his argument.

The third officer in the Program office was Army Maj. John Chiment. When he retired from the army as a Colonel about ten years later I was enormously pleased that he accepted an offer to become my Management Assistant. The technical common sense and management skills he demonstrated during our Army service together showed me how valuable he would become as my assistant.

In March 1956 the Program 2 staff deployed to Eniwetok Atoll in the south Pacific. June and I drove to California with our two cats and some belongings. June planned to live with her sister, Muriel, and brother-in-law, Paul Seright, in San Gabriel while I was overseas. Our 1948 Plymouth with 92,000 miles on its odometer interrupted the trip by throwing a rod bearing about 20 miles east of Barstow. The car was towed and my father, who was then living in Corona, drove out to retrieve us. Between my father and June's father we made our way to Muriel's house. While I was overseas June, with her father's advice, bought us a 1956 Plymouth station wagon to replace our expired car.

The next day I flew to northern California and boarded a military transport at Travis Air

Force Base. The C97 aircraft carried me to Hickam Air Force Base in Hawaii and then to Eniwetok with a stopover at Kwajalein, all at a cruise speed of 260 knots. I had hoped to see some sights in Hawaii during the overnight stay, but enlisted men were restricted to the base. Preparing for departure the airplane was boarded in order of rank; even the natives who signed up for dishwashing duty outranked an Army private. I ended up in the tail of the aircraft, facing backwards into an undecorated tail cone. We were even treated to an extra takeoff and landing due to engine magneto problems. We were all very tired and rumpled on arrival at Eniwetok, so, naturally, our first task was to pose for badge photos.

At Eniwetok 10,000 or so men and 0 women lived in tents on a small island at an elevation of a few feet above sea level, with 100% humidity and daily rain showers. There was not much to do except work and eat, but the food was outstanding. I quickly learned I had to moderate my intake to control my weight. I didn't enjoy swimming, and poisonous stonefish dissuaded me further from getting into the balmy water. Instead, while studying the literature, I stumbled on an error in an appendix to the then current edition of Glasstone's book on Effects of Nuclear Weapons, so I prepared a report to correct it. It required extensive calculations of exponential integrals, which, today would be performed easily by a desktop computer. In 1956 it took me more than 100 hours with a Marchant calculator. At least the effort helped pass the time between normal working hours. The report was published and eventually declassified[4]. It is available on the Internet under "ITR-1345", and has attached a summary of the Operation Redwing tests.

The lower-yield tests were conducted at Eniwetok Atoll while the high-yield tests were conducted at Bikini Atoll, so we flew between atolls in C47's, the military version of the DC3 aircraft. One day we took off at Bikini, circled around 360^{o} and landed just as one of the two engines stopped. The pilot claimed he had not turned if off! I understand that the C47 could not fly on only one engine, so we narrowly escaped a swim in the lagoon.

In preparation for the high-yield tests everyone boarded Navy ships to sail about 30 miles away from Bikini Atoll. A group of news reporters, including one lady, Dorothy Kilgallen, had been invited to view the first big explosion, Cherokee, an airdrop over one of the Bikini islands. The test was repeatedly delayed by unfavorable winds while the media representatives, confined to a Navy ship, tried to generate stories from their daily briefings. June sent me newspaper clippings, and it was interesting to see how they progressed. At one day's press briefing a reporter's question was answered by, "No comment". With no additional input the same speculation was reported the next day as, "Possible", the following day as, "Probable" and later as an assumed fact. I guess the reporter

Marchant Calculator

The Marchant electromechanical calculator had a keyboard, buttons for addition and subtraction and a handle to shift the carriage. For multiplication one number was entered into the keyboard. The add button was held down until the first number was added the number of times required by the units digit of the multiplier. The carriage was then shifted to the right and the first number was added the number of times required by the tens digit, and so on. Later models had a row of buttons to automatically perform the required number of additions. Dividing two numbers required subtraction until the remainder became negative and then backing up one step.

[4] V.A.J. van Lint, *Gamma rays from plane and volume source distributions,* **Armed Force Special Weapons Program Report ITR-1345** (Sept. 1956)

hoped to get at least a denial in response, which, I'm sure, would have led to further questions and speculations. In the absence of news, some reporters try to create news.

Finally, after about three weeks, the Cherokee device was detonated just before dawn. I remember feeling intense heat on my face and seeing through very dark filters a bright rising fireball. Once the ball appeared to cool I removed the filters and the scene was still as bright as daylight, but the sun had not yet risen. Then we watched the cloud develop during sunrise. We were told we could describe to our families what we saw during this test, but nothing else that we might know about it. I was surprised at how little of what I described in my letters to June was reported by the media. Probably the reporters were glad to head home after three weeks' confinement on a Navy ship. Actually, they missed noticing that the burst point was about 3 miles to the northeast of the intended target, which became obvious when sampling rockets completely missed the debris cloud. I assume that one Air Force bombardier's career was cut short that day.

During each Bikini shot that included fallout measurements the Program 2 staff embarked on a navy communications ship, USS Estes, and staffed a communications center to control the Program's ships and aircraft. Our work continued around the clock, so assignments were rotated among the program staff. Since he expected me to share in the officers' work, Cdr. Campbell made a private arrangement with the ship's executive officer for me to board as a civilian rather than an enlisted man, i.e., I exchanged my khaki shirt for a Hawaiian sport shirt before embarking on the shuttle boat to the ship. As an enlisted man I would have been rousted for, "clean sweep down fore and aft" at 5 a.m. no matter at what time I went to sleep. I ate in the junior-officer's mess with other junior civilians. I wondered what would happen if the steward Petty Officer learned he was serving an Army private. He didn't.

The last shot in the series, TEWA, was expected to produce the largest fallout pattern. The official fallout predictor, a member of the staff at the U.S. Navy Radiological Laboratory (NRDL), was unavailable because he had contracted pneumonia. I was delegated to predict the fallout pattern, since I had studied fallout modeling in my spare time. As a result, a number of U.S. Navy ships and aircraft were deployed at the direction of an Army private, of course with a Navy Commander in the loop! I heard no complaints; they found the fallout where it was predicted and collected good samples. I'm disappointed that the final report of the project, WT-1317, does not mention my contribution to the TEWA predictions. A declassified extract is available on the web.

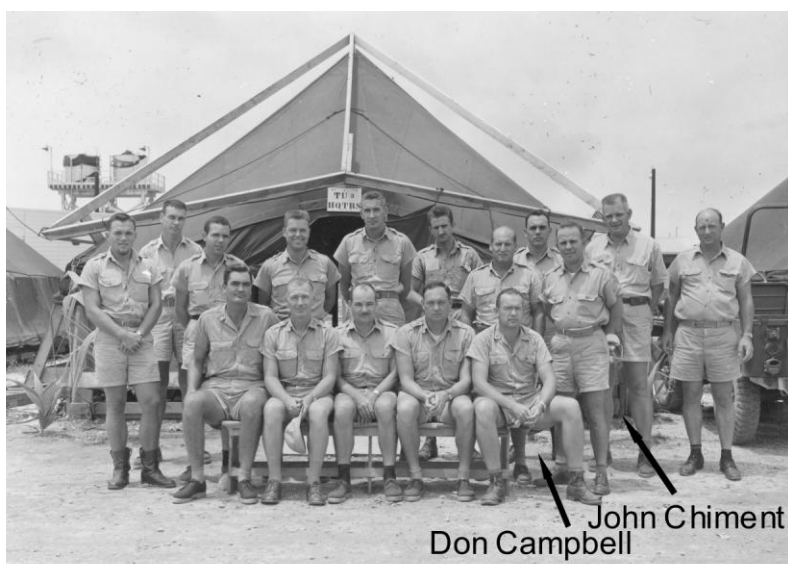

Figure 11. Task Unit 3 officers

During the final days of

our deployment the Program 2 officers and I devoted all our spare time to preparing a quick-look summary of the radiation measurements, especially the fallout patterns. A draft was complete when we departed, so Cdr. Campbell prepared a cover sheet listing the authors in order of their contributions. Since I had been able to devote the most time to the report, he listed me first and himself last. Later, in Albuquerque, when the draft came back from review, there were two comments next to the author list. The first said, "List in order of rank". This comment was lined out and replaced by, "List in alphabetical order", which happened to be the same as the order of rank. Cdr. Campbell stood firm and the report was eventually published with my name first. I've not found a declassified version of ITR-1354.

Finally, in August 1956 I returned to the continent. June met me at Travis Air Force Base for a brief vacation, starting at Lake Tahoe. We had also arranged for interviews with potential employers, since I would complete my obligation to the Army in four months. The first interview was at NRDL in San Francisco. I had worked with a number of their staff during the Pacific tests. We also visited the Atomic Energy Commission's Lawrence Livermore Laboratory. Then we went to the Los Angeles area where I interviewed at Douglas Aircraft and at Ramo-Wooldridge (the predecessor of TRW). Eventually, we went to San Diego to interview at General Atomic (GA), which was headed by Dr. Fred de Hoffman, a prestigious theoretical physicist who had served at Los Alamos during the war. GA was a newly formed division of General Dynamics Corp. I liked the spirit of the interviewers and June liked San Diego. As we drove east toward Albuquerque we decided to accept a GA offer if it was as much as $10,000/yr. Later I received an offer from Princeton involving experiments at the Brookhaven Cosmotron, but I didn't want to be part of the large experiment team required by such expensive facilities. GA offered us what we wanted; Ramo-Wooldridge's offer was a lot more, but we decided on GA. It was another good decision.

During December 1956 we experienced a major snowstorm in Albuquerque. I managed to leave Sandia Base in time to get to our home, but John Chiment was delayed and became marooned on Wyoming Blvd. He joined another stuckee who had a full tank of gas, and they kept the auto engine running for heat. This was serious: another man froze to death in his car about two blocks east of Wyoming Blvd. Albuquerque has expanded to miles beyond this point by now. The snowstorm was followed by a blizzard that coated the surface of the snow with sand. It looked like the normal desert surface but with large piles near buildings. We put on our chains and drove to Sandia Crest to take pictures, one of which still hangs in our living room. The color has faded on the original.

Figure 12. Sandia crest Dec. 1956

My enlistment was finished at the end of January 1957, but I was required to stay on my last day to be honored by the general as "Enlisted man of the month". He offered to get me commissioned as an officer if I would re-enlist. I answered politely, "I'll think about it, sir."

Remembering that I was still subject to Army discipline for a few hours, I restrained myself from telling him what I thought of it.

STAFF MEMBER at GENERAL ATOMIC

Finally, we drove to San Diego with our two cats, found an apartment and I reported to GA. It was located in temporary quarters at a former elementary school building on Barnard Street. My supervisor was Dr. Park Miller, a solid-state physicist formerly at the University of Pennsylvania. I hoped to work in solid-state physics, because it still provided opportunity for a small group to achieve useful results. Since GA already owned a helium liquefier that enabled us to reach very low temperatures, we decided to measure the skin depth in a thin superconducting film by measuring the penetration of an oscillating magnetic field. The film was prepared by evaporating lead onto a glass substrate. The oscillating magnetic field was produced by current in a planar wire spiral on one side of the film and detected with a coil on the opposite side. The experiment assembly was submersed in liquid helium and placed between the pole pieces of an electromagnet so that superconductivity could be turned off and on by activating and deactivating the magnet.

> ***Superconductors***
> *Some materials become superconducting, i.e., perfect conductors of electricity, at very low temperatures. An electrical current, once started, will persist as long as the material remains cold enough. A sufficiently large magnetic field can quench the superconductor, i.e., return it to a normally resistive material. A small magnetic field applied to one face of a superconducting sheet induces a current in the sheet that blocks the field from penetrating it. If the sheet is thin enough, comparable to the superconducting skin depth, a small fraction of the field will penetrate.*

The result was a total surprise. The expected sinusoidal signal was observed in the detecting coil when the magnetic field suppressed superconductivity, but instead of a smaller oscillating signal the superconducting film produced sharp spikes in the receiving coil. Further studies showed that the spikes only occurred when the magnetic field produced by the exciting coil changed sign. Instead of allowing a tiny leakage, as determined by the skin depth, the film intermittently suspended superconductivity, allowing the field to penetrate in chunks, after which it was locked in. Later, nothing happened if the field applied by the exciting coil was of the same polarity as the locked-in field. But when the polarity was reversed the film temporarily became non-superconducting again and allowed the locked-in field to reverse. We had a memory device! The locked-in chunks of magnetic flux could represent digital bits. I reported the results at the Fifth International Conference on Low Temperature Physics and Chemistry[5] at the University of Wisconsin in August 1957. Park and I also submitted an application for a patent, which was eventually granted to GA. We learned that IBM had invested a large effort in investigating memory devices based on superconducting thin films, so we hoped eventually to earn some royalties, but the technology never became competitive with room temperature magnetic and semiconductor structures. Park and I continued to think about applications of superconducting thin films, but soon we directed most of our attention elsewhere.

[5] V.A.J. van Lint, *Penetration of magnetic fields through thin superconducting films*, **Low Temperature Physics and Chemistry,** p. 321 , University of Wisconsin Press (1958).

Developing peaceful applications of nuclear energy was the primary goal in founding GA. Earlier arrivals had formulated a unique concept for an inherently safe nuclear research reactor, TRIGA. It used a fuel element composed of uranium and hydrogen alloyed with zirconium. The homogeneity of the uranium fuel and hydrogen moderator ensured inherent safety. The nuclear part of the design was checked by operating a "critical assembly", a mock-up of the reactor configuration operated at very low power. Dr. Ralph Stahl, one of the staff members who had interviewed me, was in charge and he asked me to plan neutron measurements with activation foils.

Nuclear Reactor

A nuclear reactor requires a self-sustaining process, with a neutron causing U^{235} nuclei to fission, emitting more neutrons, at least one of which causes another fission. Since the likelihood of a neutron causing fission increases with decreasing neutron energy, most reactors provide material to slow down, i.e., thermalize, the energetic neutrons. The reactor can be stabilized by this process: an increase in power produces an increase in temperature, which causes the average neutron energy to rise and the fission efficiency to decrease. In many reactor designs the hydrogen in water serves this purpose, but there is a delay while the heat is conducted from the uranium-containing fuel elements into the adjacent water channels. The TRIGA fuel had no such delay, because the uranium fuel and hydrogen moderator were intimately alloyed together with zirconium.

Neutron Activation Foils

Exposure to neutrons converts some elements into radioactive isotopes. The rate of conversion depends on the material and the neutron energy spectrum. Exposing an assortment of materials and counting the resulting radioactivity in each can determine the neutron fluence and energy spectrum.

Park had brought three University of Pennsylvania graduate students with him to GA. They had completed all their required course work except for Nuclear Physics. Their experimental physics theses were to result from their work at GA. Since I had a nuclear physics background, Park asked me to provide them a short course on Nuclear Physics. I enjoyed preparing the lectures and was pleased to work with them in the laboratory during the next few years. June and I remain in contact with two of them.

A GA study group concluded that a 30-MeV electron linear accelerator (LINAC) would be a valuable research tool with applications in many different fields, so such a machine was ordered. My background in nuclear physics combined with my interest in solid-state physics led me naturally into the effects of nuclear radiation on solid-state matter. My recent experience with nuclear tests convinced me that electronic equipment located at considerable distances from nuclear explosions could be disturbed and damaged by the burst of radiation, especially the intense ionization pulse. I also realized that ionization from energetic electrons produced by a LINAC would produce similar effects as gamma rays from nuclear fission.

Park was experienced in preparing and selling proposals for technical work to government agencies, so together we formulated a proposal on Mechanisms of Transient Radiation Effects on Electronics (TREE). Meanwhile, GA provided some funds so I could perform preliminary experiments at a 10-MeV LINAC located at Applied Radiation Company in Walnut Creek, CA, the vendor for our forthcoming accelerator.

We needed an amplifier to condition the expected pulsed signals. While transistorized amplifiers were becoming common, they would be too sensitive to stray ionizing radiation, so we limited our search to vacuum tube units. Our initial choice was a Hewlett-Packard general-purpose amplifier with modest frequency response, probably 10-MHz or so. This experience led us to develop more capable equipment for the GA LINAC facility.

> ***Radiation Effects***
> *The effects of exposure to energetic nuclear particles can be categorized into Ionization and Displacement Effects.*
> ***Ionization Effects***
> *A charged particle moving past an atom may rip off an orbital electron, leaving the atom as a positively charged ion. The electron and ion can them move under the influence of an electric field, i.e., make the material conductive. Either may react with other atoms, exchange charge, form new complexes, recombine, etc., affecting the material properties.*
> ***Displacement Effects***
> *A charged or neutral particle may collide with a nucleus, displacing the entire atom from its location. The recoiling atom produces more displacements as it slows down. Even in solids at room temperature there is a considerable thermal rearrangement of the resulting displacement cascade, with some long-lasting damage.*

We expected semiconductor materials to show the largest response to ionization pulses, so small bars of silicon and germanium were prepared with current and voltage electrodes. Harold Roth, one of Park's graduate students, was performing a thesis on tellurium, so we included it. We also measured the long-term effects of accumulated exposure to ~10-MeV electrons. Since lower temperatures would inhibit annealing, some of our exposures were performed with the sample in a dewar (vacuum-insulated flask) filled with liquid nitrogen. We were surprised that the long-term damage in tellurium was completely annealed at room temperature, whereas silicon and germanium only recovered partially.

The results of these preliminary experiments were reported at the 2nd UN International Conference on Peaceful Uses of Atomic Energy in Geneva, Switzerland in September 1958 and published in the Proceedings[6]. The following year we included pulsed ionization effects on the simplest semiconductor device, a PN-junction diode, and published the experimental results together with an elementary theory[7].

Meanwhile, some individuals in the Department of Defense were becoming concerned about TREE on military equipment, i.e., the unique effects of pulsed radiation. A leader was Mr. Peter Haas, at the Diamond Ordnance Fuze Laboratory, then located along Connecticut Ave. in Washington, DC. His concern grew out of his experiences trying to measure the electromagnetic pulse (EMP) produced by nuclear explosions: some suspicious results could be attributed to transient radiation effects on the measuring electronics. He encouraged the Armed Forces Special Weapons Center (AFSWP) to fund our mechanisms work as well as proposals from Army laboratories to investigate effects on military equipment.

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[6] V.A.J. van Lint, P.H. Miller, Jr., *Electrical effects of high-intensity ionizing radiation on non-metals,* **Proc. 2nd Int. Conf. on Peaceful Uses of Atomic Energy, Vol. 29**, page 336 (1958)
[7] V.A.J. van Lint, *Transient radiation effects in semiconductors,* **Proc. 2nd Conference on Nuclear Radiation Effects in Semiconductor Devices, Materials and Circuits,** Cowan Publishers (1960)

In time the 30-MeV LINAC was installed at GA under the supervision of Dr. Robt. Beyster. One of its output ports was dedicated to my radiation-effects experiments. Experience at Walnut Creek had taught us that we needed a high-quality amplifier system and a well-instrumented remote recording station. At first we asked the GA Electronics Department to develop the amplifier, but they struck out. Meanwhile, the Physics Department hired an electronics specialist, Guy Kelly, to support other experiments, so he helped me with my amplifier problem. We decided to buy replacement pre-amplifiers and vertical amplifiers for Tektronix 545 oscilloscopes to use as wide-band gain stages. Normally they drove a high impedance (i.e., the deflection plates of a cathode-ray oscilloscope) so we had to provide high-current output stages to drive the signals into 75-Ohm terminated coax cables to our recording oscilloscope. We also connected remote-controlled servos to the gain and mode selection knobs on the Tektronix preamplifiers. They saved us a lot of test time, because each entry into the exposure area required turning off the LINAC, opening the massive shield door, entering to perform the task, reclosing the door and restarting the LINAC. That instrumented test station served radiation effects experiments for more than 20 years.

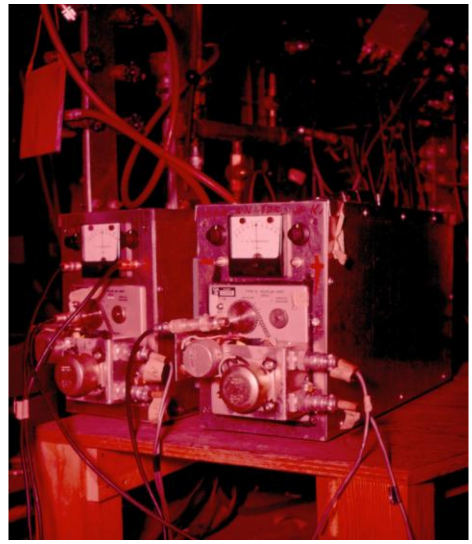

Figure 13. Remote-controlled amplifiers

Circuit Impedance

The impedance of an electronic circuit determines how much current must be supplied to produce a desired voltage. A high impedance, such as an open circuit, requires minimal current, but at high frequencies the circuit capacitance decreases the impedance. A transmission line, such as a coaxial cable, can transmit a signal faithfully if it is terminated in its characteristic impedance, usually 50 or 75 Ohm.

RADIATION EFFECTS PHYSICS RESEARCH

Research on transient radiation effects became a long-term mainstay of my group. The first participant was Professor Wm. Parker, who was on sabbatical leave from Reed College in Portland, OR. We hired a young man to be our technician, Ray Denson. He turned out to be incredibly good with his hands and a fast learner. Eventually, Dr. Gordon Wikner joined us with his brand new Ph.D. from University of California at Berkeley. As Park Miller's interests broadened, our group assumed responsibility for displacement damage work (i.e., long-term effects of atoms knocked out of their normal sites in solid lattices) that he had started together with his graduate student, Hal Roth. As the work increased we hired some new M.S. graduates from San Diego State College, including Howard Harper, John Harrity and Chuck Mallon. We also had some summer hires, including Burr Passenheim who worked summers as a high-school student and eventually joined our staff as a Ph.D.

Park Miller had a contract from the Air Force Cambridge Research Laboratory to investigate displacement effects in semiconductors. Other investigators had measured the effects of exposing germanium and silicon samples to neutrons and to electrons with energies around 1 MeV. Most neutron impacts produce very complicated defect clusters, while ~1-MeV electrons produce mostly single displaced atoms. We decided to use the LINAC with energies in the 10 to 30-MeV range to investigate the effects of gradually increasing defect complexity. Park and I attended the 1960 International Conference on Semiconductors in Prague, Czechoslovakia to report our initial results.[8]

Figure 14. Park Miller & Vic in Prague

NASA became another customer for whom we investigated displacement damage in silicon produced by electrons, such as those in the earth's trapped radiation belts. Exposure to the electrons and protons trapped in the earth's magnetic field gradually decreased the power provided to satellites by silicon solar panels. Understanding the mechanisms of damage led to predicting the rate of degradation and selecting better doping for the silicon.

We used the reproducibility of low-temperature damage in tellurium and its recovery as a means to compare the effectiveness of different exposures, including 450 MeV protons at the Carnegie Tech synchrocyclotron. Thus we avoided sample-to-sample variations by using the same sample to measure the response to different irradiations at liquid-nitrogen temperature, annealing it at room temperature between exposures.

A major theme in our programs was correlation: understanding how the effects of different exposure types and energy spectra can be compared. We asserted that most ionization effects of energetic electrons and gamma rays were equivalent for the same ionization energy deposition, although the effects of heavily ionizing protons or atomic recoils could be different due to processes occurring within dense ionization tracks. Similarly, we tried to establish correlations in displacement effects based on the energy spectrum of atoms recoiling from radiation collisions.

Figure 15. At General Atomic

Our growing program developed a need for theoretical physics support, so we hired Don Nichols, an M.S. physicist. Don and I worked out the implications of ionization formed along high-energy particle tracks, rather than as isolated electron-ion pairs, and showed that the relation between ionization-induced conductivity and ionization intensity that was currently used needed revision[9]. I'm still surprised that our simple argument remained controversial. We were even tasked to perform experiments to confirm our mod-

[8] V.A.J. van Lint, E.G. Wikner, P.H. Miller, Jr., *High energy electron irradiation of semiconductors,* **Proc. International Conference on Semiconductors,** p. 306 (1961)
[9] D.K. Nichols, V.A.J. van Lint, *Theory of transient effects of ionization in insulators,* **IEEE Trans. Nucl. Sci. NS-13**, No. 6, p. 119 (1966)

el[10].

One day at GA a nuclear chemist, Dr. Roman Schmitt, walked into my office with a question about solid-state physics, "What is the distance that an energetic recoil atom will move in a solid?" Roman had a technique and an idea. The technique was to use radiochemical counting techniques to measure the fraction of radioactive recoils from a photonuclear reaction that escaped from a target foil into an adjacent ultra-pure aluminum catcher foil. If I could tell him the range-vs-energy relation for the recoils he could estimate the average energy of the recoil atom. Conservation of momentum then allowed him to estimate the energy of the other emitted particle, e.g., the neutron in a (γ,n) reaction[11]. I answered that I could tell him the spectrum of the emitted particle energies more accurately than the atom's range-energy relation was known, so I suggested we write a proposal for a joint program to determine the recoil atom's range. We did. Subsequently, I attended a conference in Gatlinburg, TN when the keynote speaker, Prof. Harvey Brooks of Harvard University, noted the importance of range-energy measurements, such as those we had proposed. I was seated next to an Air Force civilian scientist, who immediately made a note of the remark. We soon received funding from the Air Force Materials Division. Roman's people, especially Connie Suffredini, made measurements on a wide variety of elements while Don Nichols and I set up Monte-Carlo calculations using a Thomas-Fermi-Firsov model of interatomic potentials to compare with the results. The Army Research Office also provided funds for a small program to measure the charge states of the recoil atoms by applying a high voltage between the target and catcher foils. The Air Force program resulted in two papers in Physical Review and an article in a series of review books on Solid State Physics.[12]

Recoil Atoms

When a gamma photon is absorbed by a nucleus the photon's momentum is imparted to the nucleus. When a neutron is then emitted, usually isotropically, the nucleus recoils from its momentum. The residual nucleus then moves a distance in the solid matrix, called its range, before coming to rest. If it starts near enough to the surface of the target foil, it can move into an adjacent catcher foil. If it is also radioactive, the fraction of residual nuclei that escape can be measured by counting the activities in the target and catcher foils.

Another program area grew out of my Army experience: measuring the rate of electron and ion reactions in ionized gases. Ionizing radiation passing through air produces free electrons and positive ions. These species, especially the electrons, make the air conducting, which affects the propagation of radio waves and affects the intensity of the electromagnetic pulse produced by a nucle-

Microwave Propagation

A radio wave shakes any free electrons in its path. The net effect is a shift in phase of the wave determined by the density of free electrons and an attenuation of the wave determined by the rate at which the free electrons collide with atoms. Measuring the phase shift and amplitude loss of a microwave passing through a region of ionized gas can determine the rate at which the electrons are immobilized, either by recombining with ions or attaching to neutral molecules .

[10] V.A.J. van Lint, J.W. Harrity, T.W. Flanagan, *Scaling laws for ionization effects in insulators,* **IEEE Trans. Nucl. Sci. NS-15,** No. 6, p. 194 (1968)
[11] (γ,n) notation means gamma-ray (γ) is absorbed and a neutron (n) is emitted.
[3] D.K. Nichols, V.A.J. van Lint, *Energy loss and range of energetic neutral atoms in solids,* **Solid State Physics, Vol. 18**, p. 1 (1966) F. Seitz, D. Turnbull, editors. Academic Press (New York)

ar detonation. The magnitude of the air conductivity is determined by how long the electrons remain free before being immobilized. The energetic electrons from the LINAC produced a controlled ionization pulse and the microwave diagnostics measured the resulting evolution of electron density. A proposal to the Air Force was eventually successful.

The ionized-gas project forced me to learn FORTRAN computer programming. The experiment produced oscilloscope photos of the output of a microwave detector, whose voltage output was a function of microwave power. We had to apply a nonlinear calibration curve to each point of the oscilloscope trace to compute the microwave power as a function of time and then combine the microwave powers from two traces to calculate the time-dependent electron density and electron collision frequency. This point-by-point process was manually very time-consuming, so I requested that a professional programmer write a FORTRAN code to perform the calculation. The initial version produced some strange results, such as electron density histories with sudden jumps even when the oscilloscope traces were smooth. After learning to read the FORTRAN code I discovered the problem: the detector calibration was provided as discrete points specifying microwave power at specific output voltages. The computer calculated a least-squares fit of a power-law curve to the points, but when an experimental voltage was identical to a calibration point the programmer used its calibration microwave power instead of the power-law fit. This produced a small glitch at each calibration point, because the power-law fit did not pass exactly through all the calibration points. This and similar later experiences taught me that programmers needed a thorough understanding of the science to produce reliable codes.

The ionized-gas research also produced some exciting cooperative ventures with the atomic-beam people, led by Dr. Wade Fite. The culmination was an experiment in which we measured both the disappearance of electrons (using microwaves) and the ion species (by extracting them through a tiny aperture into a mass spectrometer) during the aftermath of an ionization pulse. The results were presented in many technical reports, a few journal papers and some conference presentations. I remember one conference at which I presented a paper on ion-ion recombination in ionized air, which we measured using an extremely sensitive 300-MHz resonant cavity (about three ft. in diameter). Dr. Leonard Loeb, then elderly but a pioneer in gaseous electronics, was incensed that we had dared to measure a reaction rate that one of his students had measured by a different method and published. I tried to explain that the ions might be different due to ours being younger (i.e., less time to react with other species before recombining), but to no avail. I think he resented a nuclear physicist daring to invade his Gaseous Electronics club.

RADIATION EFFECTS on MILITARY EQUIPMENT

GA received a Request for Proposal from the Air Force to investigate the effects of radiation pulses on the guidance electronics of the Falcon infrared guided missile. I was hesitant to divert my group's efforts from what I considered to be more important studies of radiation effects mechanisms, but I volunteered to help the GA Electronics Division get into the radiation effects business. We wrote a successful proposal, the Electronics Division hired two people to conduct the program and I agreed to teach them. As it turned out, the newly hired leader of the project was not competent but, fortunately, the other new hire, Doug Willis, was a receptive learner. It was obvious that the customers were going to hold me responsible for the results since my group's reputation had sold the contract. I supervised completion of the project and helped write the final report. Doug transferred to my group and we became active investigators of radiation effects in electronic equipment.

This was my first experience with a problem that would recur during my career: others in the company selling a program, benefitting from my organization's reputation, but not performing the work with what I believed to be adequate quality. I felt that their work would reflect badly on my organization. In a later case I believed that the immediate customer, an aerospace systems contractor, only wanted an endorsement from our reputable company, not a serious investigation. I could not resist speaking out, but the result was more hurt feelings than solutions.

> ***Quartz Pressure Transducers***
> *A quartz crystal is piezoelectric: equal and opposite charges appear at its surfaces when it is squeezed. The pressure pulse induced into a material can be determined by bonding a quartz crystal to it and measuring with an oscilloscope the induced charge as the voltage history.*

Bob Poll, who we hired from Convair, became a leader of many projects investigating the response of electronics systems to pulsed radiation. One of Bob's first tasks was to investigate the effects of pulsed radiation on quartz crystal pressure transducers that were to be used to measure stresses in material samples exposed at an underground nuclear test. We formulated methods of minimizing the radiation-induced disturbances, leading to a successful test and a good long-term relationship with the test's project officer, who eventually became an Air Force general.

> ***Missile Inertial Guidance***
> *An inertial guidance system measures the vehicle's accelerations (in three directions) integrating them twice to calculate change in position. It becomes lost if a disturbance alters the calculated current position. If all such critical "state vectors" are protected by storing them in undisturbable memory, the calculations can resume after a disturbance with little error.*

Subsequently, we supported concept studies for missile guidance equipment that could tolerate nuclear-explosion radiation pulses. We formulated a concept called "Fail-Safe" for digital guidance computers, in which critical information was stored in "hard" memory, i.e., memory that would not be disturbed by the radiation. When the system is exposed to a radiation pulse a radiation detector triggers a reset, by which computations would restart using initiation values previously saved in the "hard" memory. An example of "hard" memory is a magnetic memory with its write electronics disabled. The concept was invented independently and called "Circumvention" by the designers of the Minuteman II guidance electronics.

Subsequently, the GA radiation effects physics and engineering activities proceeded in parallel while I tried to maintain good information flow between them. The engineers were able to tell the physicists what problems appeared important for systems, which led to contract proposals to develop the physical understanding necessary to find solutions; the physicists taught the engineers how to apply the current mechanisms understanding to their hardware problems. Most of our profits came from the engineering activity, especially routine electronic device testing. Many were re-invested to develop new physics programs.

Meanwhile, military systems managers were becoming increasingly aware of the potential deleterious effects of short radiation pulses from nuclear explosions, and military electronics technology was evolving to become increasingly sensitive to brief disturbances. The Minuteman II ballistic missile was in development using an early generation of microcircuits for its guidance electronics. GA received a sizeable contract for exposing samples of each microcircuit type to pulsed ionization at LINAC, using our amplifier and recording systems to document the response. We were the first to observe ionization-induced latchup, which rendered the exposed circuit inoperable.

> ***Latchup***
> *Normally an electronic device or circuit responds to a short ionization pulse with an output excursion that lasts no longer than a few times the circuit's inherent recovery time. Some microcircuits, however, can be triggered into a meta-stable state, similar to turning on a silicon-controlled rectifier, from which they can recover only by removing and re-applying their power. In some cases they will burn out if power is not removed quickly enough.*

The growing awareness of the many possible effects of nuclear explosions on our strategic assets prompted the Department of Defense to convene a Vulnerability Task Force chaired by Dr. Albert Latter. He was president of RDA, which had been formed as an amiable spin-off from the Rand Corporation. I was invited to participate as a consultant to the Task Force, which met intermittently for many years to review various military systems. This experience taught me the importance of preparing good specifications for system development, especially ones that include effective and practical means by which compliance must be demonstrated. Systems contractors are motivated to meet the legal requirements in their contract, but doing more could be costly. Adding more requirements after the contract is signed is particularly costly to the government, because the contractor usually tries to recover other costs he did not anticipate during the initial negotiations.

By 1963 our contract base had grown to require more senior leadership. Fortunately, Dr. Ralph Stahl became available after he had completed his work on TRIGA development. I remain everlastingly grateful to Ralph for his assistance. He was an extremely capable physicist, good friend and highly honorable person.

One day in 1963 a phone call invited me to attend a Saturday meeting at Avco Corp. in Lowell, MA. Ralph Stahl and I flew there to meet with Mr. Hank McCard and his associates, who described their needs. They had a contract to develop nuclear-test readiness program instrumentation to place inside one of Avco's Mark 11A ballistic-missile reentry vehicles. Diagnosing the response of the reentry vehicle with on-board recorders required that all the electronics tolerate high radiation levels. Avco was developing the instrumentation, but offered us a subcontract to perform radiation analysis and testing and formulate hardening recommendations, our first million-dollar contract. I learned later that Avco had started to do their own radiation hardening, but the Air Force had threatened to force them to subcontract the entire instrument package if they didn't get competent radiation-effects support.

> ***Readiness Program***
> *In the fall of 1958the U.S. and U.S.S.R. had each voluntarily ceased nuclear testing in the atmosphere, but the Soviet Union resumed unexpectedly in September 1961 with a vigorous test series. An atmospheric test ban treaty was signed in August 1963, but the U.S. resolved to be prepared to resume testing quickly if the Soviets did so.*

Ralph became the key leader of the effort, which finished with a successful underground nuclear test of the instrumentation package two years later. We increased our staff considerably, especially with electronic engineers. Leo Cotter transferred from the GA Electronics Division. A number of others were hired from General Dynamics Convair, which was undergoing program cutbacks at that time.

As the radiation effects group grew it became obvious that I needed administrative help if I was to perform any technical work. I learned that John Chiment, now a Colonel in the Army, would soon retire, so I asked him to be my management assistant. He agreed and assisted me faithfully throughout my employment at GA. He always gave me good advice and carried out the day-to-day management tasks that would otherwise have prevented me from participating in technical work and project development. I credit my later success as a consultant to John; he enabled me to maintain my technical skills in spite of ongoing management demands.

> ***Thermo-mechanical Effects***
> *Normally, when a material is heated it expands in all directions. If it is heated by a pulse shorter than the time it takes a sound wave to propagate across the material, it cannot expand but produces a pressure pulse sufficient to hold its original dimensions. But the pressure is relieved at surfaces, producing rarefactions that propagate and interfere with each other. The resulting positive and negative pressures may distort or fracture the material.*

Around 1965 we became aware that the X-ray pulse from nuclear explosions could deposit enough energy in electronic devices to cause thermo-mechanical damage, such as breaking the contact between "flying wire" leads and semiconductor chips inside device packages. Ralph Stahl and I simulated the effects using short-pulsed electron sources, such as a Field Emission Corp. Febetron. Correlation tests we performed at the Distant Mist underground nuclear test encouraged the sponsors to continue our laboratory approach. Ralph became one of our nation's leaders in electronic-device thermo-mechanical damage, supervising numerous laboratory and underground nuclear test projects. We realized that a military system could be disrupted by the breakage of even a single wire-bond among the tens of thousands in a complex system. Quality control over bond strengths was vital, so we exposed thousands of devices at underground nuclear tests to check on their quality. We monitored the device integrity with a commutator system that interrogated all units two or three times before arrival of ground shock at the test cassette. Only this way could we distinguish effects of the radiation exposure from other ancillary disturbances. Others who exposed devices passively, checking their quality long before and after exposure, could not distinguish between radiation-induced failures and those due to normal unreliability or the test's ground shock.

We used similar interrogation methods on magnetic memory devices, measuring their responses on an oscilloscope with a moving-film camera to record multiple traces. Others who had used passive exposures found inconsistent results, possibly because the devices were disturbed by magnetic fields generated by welding equipment used for unrelated experiments near the test cassette. I always believed that the extra effort invested in more complex instrumentation was rewarded in reliability of the data.

ORGANIZATIONAL CHANGES

The GA High Energy Fluid Dynamics (HEFD) division experienced major changes in its market in 1965. It had grown around the Orion concept developed by Dr. Ted Taylor for propelling a space vehicle by a sequence of small nuclear explosions. No technical flaw had been uncovered, but the concept was politically unpalatable for further development. Therefore, the principal theoretical and experimental investigators were finding other markets for their talents, especially in the design of underground nuclear tests. However, all of us dependent on government contracts were finding it difficult to justify the GA overhead rate, which resulted in a labor cost approximately three times the performer's salary. We proposed to GA management that HEFD and Radiation Effects form a cost center with an overhead rate determined by our own overhead expenses while paying our share of the corporate General and Administrative (G&A) costs.

As a result GA formed the Special Nuclear Effects Laboratory (SNEL) led by two Associate Directors, Dr. Moe Scharff and me, and relocated to the second story of the GA factory building in Sorrento Valley, i.e., the low-rent district. John Chiment and his HEFD counterpart performed valiantly in arranging the move and organizational transition. We trimmed expenses and cut the cost to the customer to approximately twice the performer's salary. Our revenue increased sharply; we were benefitting from a growing market and making important contributions to national defense and the technology base.

The summer of 1966 was reasonably calm at work, but traumatic for the van Lint family. June and I drove our Dodge station wagon with four children and travel trailer to Palo Alto, CA for me to attend the annual IEEE Nuclear and Space Radiation Conference. After the conference we drove to central Oregon where the trailer whiplashed the car into a roll-over. The children and I were only slightly bruised, but June suffered a dislocated neck. Cervical fusion surgery a few weeks later left June in a coma. After 4 weeks she was flown to San Diego, and a few weeks later she became aware, but remained quadriplegic. During the first half of 1967 she spent Sunday nights to Friday evenings at Rancho Los Amigos Hospital in Downey for rehabilitation. I really missed her during the week, especially so since the situation at GA was in turmoil.

Figure 16. June after surgery

Many members of the SNEL staff remained disturbed by the mismatch between our business and GA's main line of work: developing and selling nuclear high-temperature gas-cooled reactors (HTGR) for generating electrical power. We required prompt fiscal information to maintain projects' expenses within their budgets; the HTGR concerns were at a much larger and longer scale. Our total laboratory profit was a factor of 100 smaller than the annual HTGR costs. Our business depended on individual entrepreneurs conceiving and selling research projects, but GA had no means to provide incentive rewards. Personnel classification was an issue; for example, in spite of his great value to SNEL, GA would not classify John Chiment as a Staff member because he did not have a Ph.D. nor was he functioning as a creative scientist. Since the corporate G&A was applied only to direct labor hours we contributed more than our share because we maximized direct labor to keep our overhead low. Some of our contracts required us to provide objective advice to government agencies, but our association with General Dynamics could create conflicts of interest. In some cases other divisions of General Dynamics moved into fields we had developed and used corporate authority to override our best judgments.

During the first half of 1967 a group of senior SNEL staff members discussed these problems. They recommended that General Dynamics form a separate corporate entity (to minimize potential conflicts of interest and enable us to serve as unbiased advisors to the government) with its own burden center (to control overhead rates) and personnel policies (to allow us the flexibility to reward contract-selling entrepreneurs). I did not participate in the study group, but endorsed their initial recommendations. While sympathizing with the problems the group tried to solve, I objected to the deadline they then imposed on GA management, which grew out of their parallel effort: they developed a plan to resign and form their own company. Their recommendations became an ultimatum to which Dr. Fred deHoffmann, the GA president, could not accede. I refused to join the group; instead I wrote a memo to Fred presenting my own views of the problems and challenges. His response on July 1 was to form a Defense Science and Engineering Council, including himself, the cognizant Vice President, Bart Smythe, and the John Jay Hopkins Laboratory Director, Ed Creutz. I don't think the Council ever met, probably because some members of the study group pre-empted.

In early August four SNEL principals announced their resignations, effective in two weeks, to form the Science, Systems and Software Company, known as S-Cubed. One of the four was Bob Poll, who was serving as lead investigator on my largest contract, the one in support of the Minuteman program for the Air Force Space and Missile Systems Organization (SAMSO). The final report on that contract was due at the end of September. Bob offered to serve as a well-paid consultant to help us complete the contract. Obviously, if we accepted it would be telling SAMSO that we couldn't complete the work without Bob.


Figure 17. Family in late 1967

The other founders of S-Cubed were from the HEFD part of SNEL and also key investigators on critical contracts. Ralph Stahl, a participant in the study group, told me he had not decided whether to join S-Cubed, but promised to give me six weeks' notice if he decided to resign. I asked him to take on Bob Overmyer, another of our senior physicists, and teach him the thermo-mechanical effects technology and business. Ralph eventually resigned, but not before transferring his responsibilities and knowledge to Bob in excellent order. I was delighted to hire Ralph back some years later, after he had become dissatisfied with working at S-Cubed and SAIC.

GA's response to the "S-cubed revolution" was to re-organize SNEL, forming a Defense Sciences and Engineering Center (DSEC) with Bart Smythe, our cognizant GA vice president, as acting Manager. It was composed of a Theoretical and Computational Sciences Division, Experimental Sciences Division, Applications Science and Engineering Division, C/H Project, and Vulnerability and Hardening Division (mine). Dr. Ernest Wilkins, a personable mathematician, and I were appointed Associate Technical Directors. John Chiment was appointed Assistant to the Manager. A flurry of memos followed with organization charts, procedures, action items, etc., including one considering hiring an Albuquerque company formed by a group of retired military officers to help us, "establish good relations with the various agencies located in Albuquerque", i.e., use their contacts to help us sell to the Air Force. A while later Ernie Wilkins was named director of DSEC.

As soon as the first resignations were announced I met with my key leaders to decide how to meet our commitments and sell follow-on contracts. I met immediately with the SAMSO Project Officers and assured them that we would finish our commitments and seek follow-on work. They reported that when Bob Poll visited them they asked him why I had not joined S-Cubed; he answered that I owed GA for its help when my wife was injured. I said that while it's true that GA helped me, I refused to join because I disapproved the S-Cubed founders' tactics, not out of gratitude to GA. Then the Project Officers asked me if, in addition to our current contract commitments, we could prepare and field an underground nuclear test experiment that Rockwell Autonetics said they could not construct in time. I promised an answer within a week. After checking with contacts at Convair to ensure that they could provide the required electronics assembly, I answered that we could. It helped that Joe Breeden had recently transferred to GA from Convair and was available to coordinate the activities with his former associates. We fielded the project successfully. My straight-forward approach secured our position with SAMSO. I think that a similar approach for the HEFD programs would also have succeeded, i.e., providing direct management support to the principal investigators at the agencies. Instead, the HEFD staff received encouraging memos.

In the middle of this turmoil Gulf Oil Corporation bought General Atomic from General Dynamics, so we became Gulf General Atomic (GGA). It was ironic that on August 23 a memo from Fred deHoffman quoted a General Dynamics spokesman responding to a Nucleonic's Week article with, "There is no foundation to the report that General Dynamics has offered its General Atomic Division or any of its component parts to other companies." Then on October 19 Fred announced the sale and formation of Gulf General Atomic Company. I suppose it is possible that such a complex deal was consummated in less than two months, but I doubt it. For the record, Fred quoted the spokesman rather than providing his own opinion. It illustrates the quandary that besets an honorable manager when a truthful release could jeopardize a valuable deal. The sale also explains why GA waited until the following year to sue the S-Cubed principals for their alleged use of GA resources while planning their new company; GA could not introduce the confusion of a lawsuit during delicate negotiations. That delay provided S-Cubed opportunity to receive funding before being forced to settle a lawsuit.

During the next year, 1968, the radiation effects activities grew modestly, in spite of losing Ralph Stahl to S-Cubed. The theoretical HEFD activities were decimated, and mostly moved to S-Cubed. The experimental HEFD program, led by Dr. Howard Kratz, remained, I assume because S-Cubed was reluctant to incur the costs of operating the Green Farm high-explosive test site. Eventually, the remains of the theoretical HEFD programs were turned over to the Accelerator Physics Division led by Dr. Bob Beyster, together with a sizeable allocation of overhead funds to rebuild them. He wisely hired Dr. David Sowle from Convair to lead the effort. Dave already had an excellent reputation with the principal customer, Defense Nuclear Agency.

By the fall of 1968 the situation had stabilized, but the problems besetting contract-supported research at GGA remained. I prepared a memo for Fred recommending that the research functions of GGA be organized into two laboratories, each headed by a Director: one for development of commercial products, the other for contract-supported research. I proposed a staff Technical Director to advise the managing directors on quality of technical work and assignment of appropriate talents to jobs. Probably as a result of my memo, Dr. John Russell, Bob Beyster's deputy in the Accelerator Physics Department, and I were tasked to propose the organization and charter of a Space and Defense Division for GGA.

On January 8, 1969 we were all surprised by an announcement: Bob Beyster, the head of the Accelerator Physics Division had resigned to form Science Applications International Corporation (SAIC). GGA responded by forming a Defense and Space Division headed by Dr. Chalmers Sherwin, a GGA Vice President. One of its constituents was the Defense Sciences Department headed by me with John Russell as Deputy Manager. This department included all former activities of Accelerator Physics, Vulnerability and Hardening (my activities), Atomic Beam laboratory and Experimental HEFD, i.e., most of GGA's work for the Dept. of Defense. We quickly organized into a Nuclear Branch managed by Dr. Chuck Preskitt, Radiation Effects Physics Branch under Dr. Jim Naber, Radiation Effects Engineering Branch headed by Dr. Bob Mertz, Experimental HEFD Branch managed by Dr. Howard Kratz, a Theory Branch led by Dr. Dave Sowle, Atomic Physics Branch, for which we soon hired Dr. Roy Neynaber from Convair as manager, and the Accelerator Facility led by Mr. Trevor Overett. We set a goal for the department overhead rate slightly higher than the SNEL rate had been, but well below the historical value for Accelerator Physics. Many adjustments were required, because the Accelerator Physics staff was used to spending the high overhead they were forced to charge. After much negotiating the Department started to function with John Chiment performing a vital role in coordinating the management activities. We formed a Management Staff, consisting of the Department Directors, Branch Managers and John Chiment to discuss policies and the development of future programs, including how to counter SAIC recruiting. We asked John Russell to lead new-business development, including potential services for Gulf Oil Corp.

We thrived, lost a few people to SAIC but generally maintained ongoing programs. Radiation Effects Engineering was particularly profitable, providing funds for developing new program areas. We provided a modest profit to GGA, although it was miniscule compared to the HTGR costs. We contributed more than our share to the corporate G&A overhead. Our success was recognized on July 1, 1970 in another re-organization, when we became the Defense Sciences Division of Gulf Energy and Environmental Systems, Inc. (GEES), with me as a Vice President of GEES. GGA was now another subsidiary of GEES. Soon we renamed our division Gulf Radiation Technology, or Gulf Rad Tech, to provide a more memorable title.

Later in 1970, as a GEES Vice President I attended the annual Gulf Oil Corporation management review at a luxury resort in Boca Raton, FL. Various corporate entities made presentations in the mornings; the afternoons were left for sports and recreation. I learned a lot, and composed a memo to my colleagues expressing pessimism about our future in Gulf. I noted that Gulf was not accustomed to developing technology through in-house research. Instead, they used their financial resources to buy other businesses after the usefulness of their products had been demonstrated. For example, I had expected the president of Gulf Research and Development Co. to emphasize the value to Gulf of his organization's developments. Instead he talked about his success at cutting costs. I heard Claude Wild talk about his Washington, DC lobbying activities, which eventually led to his indictment for making illegal political contributions. The Gulf atmosphere was not favorable to a research-oriented division, even one with a modest profit.

One day near the end of the year the GEES president called me in to tell me about a special Gulf Oil Co. fund to which all senior managers, now including me, were invited to contribute a stated percentage (I think it was 5%) of their salary. The fund was administered by a corporate lawyer and used for political contributions. The president assured me that my contribution would be repaid as a Christmas bonus. I declined, but imagined the effect of such a request on someone seeking career advancement in Gulf, since it was passed directly down the management chain. I was shocked by the illegal promise of repayment, but not surprised later when Gulf officers were enmeshed in the Watergate aftermath.

Not only was Rad Tech the wrong type of organization for Gulf, we were also too small to be interesting. During one of his visits to San Diego, I heard Gulf's President, B.R. Dorsey, comment that Gulf was not interested in "Mom and Pop" operations, by which he meant anything producing less than multi-million-dollar profits. These observations led in November 1970 to my meeting a number of times with Bob Wolfe, head of GEES contracts. We recommended to the president of GEES, Art Rolander, that we seek a partner for Rad Tech outside Gulf with which to form a corporation separate from Gulf. Gulf would profit from part ownership of the corporation.

Starting in early 1971 a partner/sponsor for Gulf Rad Tech was sought. Most of Rad Tech moved away from the original GA campus to a leased building on Convoy Court to facilitate the planned corporate separation from GEES. Unfortunately, in my opinion, instead of promoting a potentially profitable association, GEES negotiators were asking for an excessive amount of up-front cash. Negotiations were started with reputable companies, including EG&G, Maxwell Labs (in my opinion the best fit), and Material Sciences Corporation. Since 1971 was also a difficult year for contract research and we had to cut back our staff and profits, reputable organizations were unlikely to buy Rad Tech for cash.

When these potential acquisitions dried up in early 1972 the Rad Tech Management Staff performed an intensive planning effort to form a business plan. We also prepared a briefing for Gulf management proposing the formation of a pseudo-independent division supporting itself with contracts. It would develop a broad technology base and commit to assigning top priority to solving technology problems for Gulf. I was scheduled to brief the cognizant Gulf Vice President during his next visit to San Diego. I used only 20 minutes of the half-hour scheduled to provide time for questions and answers. As soon as I finished the Vice President looked at his watch and said, "Good, we're back on schedule for the Personnel folks".

Then in the summer of 1972 Intelcom Industries, a public over-the-counter corporation, indicated interest in acquiring Rad Tech. I didn't take them seriously: their revenue was less than a third of ours and their financial reports looked sick. Yet, in September they made an offer that included three million dollars up front, a note for one million and stock valued at another million dollars. I didn't believe they could raise the money, but the GEES financial Vice President, Creighton Gallaway, assured me that his contacts at First Boston Bank reported that Intelcom had multi-million-dollar financial backing. So we proceeded to plan an acquisition in which Rad Tech would become the principal division of Intelcom.

In December 1972 we hit a snag. Bob Berry, the president of Intelcom Industries, showed me a Rad Tech financial forecast that he had received from GEES. I had never seen it and it badly overstated our expected profits, so I blew my stack. Unfortunately, I had to eat crow, because my Deputy Manager, Bob Mertz had received the forecast back in August while I was on vacation. He hadn't taken Intelcom seriously either, so hadn't studied it carefully and forgot to pass it on to me. I was surprised that Berry didn't seem to mind that he had been misled, but I came to understand his response better the following year.


Figure 18. Family in Jan. 1973

The acquisition was supposed to be consummated around Jan. 1, 1973, but the process dragged on. We became increasingly suspicious of Intelcom finances, so in March 1973 Gulf auditors performed a quick review of Intelcom financials. Their report cited major disagreements between corporate and subsidiary Financial Statements: the corporation reported a profit while all subsidiaries had losses. A large part of the corporate "assets" were nothing but capitalized selling expenses and there was a serious shortage of working capital. Intelcom was effectively bankrupt.

Regarding working capital, I remembered the head of the Automated Marine International (AMI) division reporting on a conversation he had with his customer in Japan. AMI was constructing satellite-based navigation equipment to be installed on tanker ships in Japan. The contract called for partial payment on equipment delivery, with final payment after shipboard installation. AMI was still having difficulties making the equipment function properly but needed cash badly, so they shipped an <u>empty</u> relay rack to Japan and billed for equipment delivery. A memorable telephone conversation followed the customer's discovery that the rack was empty.

Meanwhile, Bob Berry informed GEES that he could only get a $2-million bank loan, but offered a one-year $1-million note to complete the promised down payment. I found out that the $2-million bank loan was to be secured by $1.7 million in Rad Tech receivables plus all of Rad Tech's equipment plus personal guarantees from three principals. Since $1.7 million was guaranteed by Gulf, being mostly receivable from the government, it was remarkable that the bank would not loan as much as $300,000 on all Rad Tech furniture and scientific equipment, including the LINAC, without additional personal guarantees. Bob asked me to sign the guarantee, telling me that there was no risk because the bank would never try to collect from individuals. He also said it would enable the Board of Directors to grant me a stock award. I declined, saying that I had confidence in the future of Rad Tech but not in other Intelcom divisions. I also pointed out that the need for working capital ruled out any chance of making the required payments on the one-year note to Gulf. I remember Bob's answer, "Vic, when you get a company like Gulf committed to your success, they won't let you fail".

We in Rad Tech expected the Intelcom deal to be cancelled in light of the audit and the revised offer. As an alternative we recommended forming a self-standing Gulf-owned corporate unit, since we already had plans to operate independently of GEES. But the GEES president, Art Rolander, and Vice President, Creighton Gallaway, recommended to Gulf executives that the revised offer be accepted, and Gulf executives agreed. I was shocked. In my opinion Art and Creighton were too embarrassed to admit that they had not adequately checked out Intelcom's finances much earlier.

I resolved to resign, but offered my colleagues on the Management Staff the chance to change my mind. On a Tuesday morning I delivered copies of the following memo to them.

1. *Rad Tech is buying itself from Gulf – Intelcom is providing only brokerage services. Rad Tech has to pay all debts and provide its own cash flow.*
2. *I don't believe Rad Tech was worth $5 Million last September.*
3. *The value of Rad Tech has decreased since last September.*
4. *Intelcom's state of solvency doesn't appear to provide any help for Rad Tech at this time, especially in cash flow.*
5. *Should we be successful and pay all debts, the principals in Rad Tech, whose talents earned the money to pay them, would still own only a small share in Rad Tech (probably <5%)*
6. *My personal loyalty to the company for which I work has been strongly eroded by the terribly impersonal manner in which this deal has been handled in San Diego and Pittsburgh.*
7. *I'm allowing myself 48 hours to change my mind. If not, I plan to announce my resignation Thursday morning.*
8. *I apologize for my vacillation. I've been terribly preoccupied by trying to save Rad Tech people's jobs. Now I'm convinced that what we are doing is only postponing the inevitable. There is just too much burden.*
9. *Please tell no one in Rad Tech before Thursday morning.*

The following Thursday morning I was invited to attend a meeting at Ralph Stahl's house with the Rad Tech Management Staff and Creighton Gallaway and Bob Berry. First, Bob Berry assured us that we would be successful, and that Rad Tech would help Intelcom become financially sound. Creighton promised that if Intelcom wasn't able to meet its commitments, Gulf would simply repossess Rad Tech. Then each of my seven colleagues spoke in favor of continuing with the deal and asked me not to resign. I agreed reluctantly. I didn't realize at the time that Creighton's promise could not be kept. If Gulf had withdrawn from the deal after agreeing to it, Intelcom would have been forced into bankruptcy and the Intelcom stockholders would probably have sued Gulf. It is no wonder that Bob Berry was not disturbed the previous fall at receiving incorrect information; it was potential ammunition in a lawsuit. I wrote Creighton:

Creighton:

My analysis of the situation now is:

1. *I believe the present deal between Gulf and Intelcom is unfair to Rad Tech, since the price that Rad Tech will have to end up paying Gulf for itself is excessive.*
2. *I am disappointed that Gulf, to which I believe I have demonstrated a high degree of long-term loyalty, pursued this deal without adequate consideration for the people in Rad Tech.*
3. *I bitterly regret not resigning two years ago.*
4. *The alternatives available to me now are both distasteful:*
    a. *Sanction Gulf's behavior by staying, or*
    b. *Jeopardize Rad Tech's future by leaving*
5. *I am persuaded by my senior associates that together we have a good chance of success in spite of the handicaps.*
6. *Therefore, I will not submit a resignation at this time.*
7. *Rad Tech's senior management, including me, will exert maximum effort to ensure the success of Intelcom Rad Tech.*

So Rad Tech officially became part of Intelcom Industries.

As we were departing, the company that started as General Atomic became increasingly focused on marketing high-temperature gas-cooled reactor (HTGR) electric power stations. A small demonstration plant was operating successfully at Peach Bottom, PA, but a full-scale plant at Fort St. Vrain, CO fell years behind schedule, eventually operating at slightly reduced peak power. I read that contracts were signed for ten more commercial plants, but that cost growth forced Gulf to negotiate release from all of them. A joint venture in nuclear-reactor fuels with United Nuclear Corporation was formed and eventually dissolved. A nuclear fuel reprocessing plant in Barnwell, SC was constructed but abandoned. The losses must have totaled well over a billion dollars.

Subsequently, Gulf Oil Co. was acquired by Chevron Oil Co in a "white knight" deal. Chevron then sold General Atomic to the Blue brothers, but withheld the surrounding property for commercial real-estate development. General Atomic returned to its original concept to develop innovative products and services. One of GA's successes is the Predator drone program. GA continues to develop HTGR concepts for next-generation power plants and to perform magnetic fusion research with its Doublet plasma machine.

If the Army had assigned me to project Matterhorn at Princeton in 1955, I might have eventually become part of GA's magnetic fusion plasma research.

INTELCOM RAD TECH

The official date of the acquisition of Rad Tech by Intelcom was March 31, 1973, the last day of the Intelcom Industries fiscal year, although the deal was not signed until a few weeks later. Without Rad Tech Intelcom Industries reported revenue of $2.7 million and operating costs of $4.2 million. It also wrote off $0.7 million of prior year's costs that had been listed as capital assets for a net loss of $2.23 million. The notes in the financial statements commented that including Rad Tech would have made the revenue $9.0 million and the loss $2.16 million, i.e., Rad Tech would have contributed a tiny profit even after paying the full GEES G&A allocation. The

Intelcom balance sheet reflected the acquisition, but still showed a negative current balance (current assets minus current liabilities) of $1.2 million. Stockholders' equity was slightly positive at $0.6 million, but only due to a ridiculously high appraisal of $3.4 million for the Rad Tech laboratory equipment, on which the bank had only been willing to lend $0.3 million.

By the time I went on vacation in August 1973 Intelcom's cash flow problems had infected Rad Tech. Vendors were not being paid. Some local vendors insisted that their bills be paid before they would sell more products for cash. Every Friday payday we wondered whether the bank would honor the paychecks. One Thursday night, as I listened to the 11:00 p.m. news, I heard that our bank, U.S. National Bank, had been taken over by the U.S. Government and sold to Crocker Bank. Fortunately, the paychecks were paid the next day.

Since Intelcom was already in default on the loans from Gulf, Bob Berry told me before my vacation that he would be discussing the situation with Mr. Wm. Moyles from Gulf Corporate Development in Pittsburgh. I told him I was prepared to divert from my vacation to Pittsburgh if it became necessary for me to talk with Mr. Moyles.

During our vacation my sons, Larry and Kenny, and I took a three-day backpacking hike in the Grand Tetons, a wonderfully calming experience. The last evening on the trail I perched on a rock, watched the sun set over the Grand Teton, and reviewed the Rad Tech situation. I concluded that the situation was intolerable and resolved to resign unless there was a commitment to improve Intelcom Industries' finances.

When I returned to work a major change was announced: The Intelcom Board of Directors was to be reorganized with Mr. Moyles, Mr. Joe Robinson (Gulf's on-site monitor), Mr. Cliff Brokaw (from the investment firm of Blythe, Eastman and Dillon), and me as new members. Meanwhile, Cliff would assist Intelcom to relieve the pressure on our cash flow by rearranging financing with Gulf and the bank. I felt that would be a step in the right direction if it were accompanied by careful control over spending.

I soon met Cliff Brokaw when, together with Bob Berry, we visited some of the Intelcom divisions. I was disappointed that Cliff didn't seem concerned about lax fiscal control. I was shocked during a PSA flight from San Jose to San Diego when he related, across the aisle, a joke that included the forbidden "N" word. I eventually decided that he wasn't a racial bigot, but simply didn't respect anyone who wasn't rich.

It seemed like negotiations with our bank were continual. We were perpetually in default on our loans. When I pointed out that we couldn't make the payments required by a proposed revised agreement, Max Kemp, who was our controller, said in agony, "My signature is on the existing loan!" Apparently, Bob Berry had turned to him when I refused to guarantee the loan, and Max had bought the fairy tale that the bank would not go after the personal guarantors.

The new Board met in January, 1974. I provided a Rad Tech financial forecast, including the expected loss on a commercial nuclear fuel rod scanner being developed under Chuck Preskitt's leadership. During the meeting Bob Berry contradicted me by claiming to have later, more optimistic information from Chuck. I estimated that Intelcom Industries would show a $1 million loss by the end March, i.e., for the fiscal year. Bob claimed it would almost break even. Cliff Brokaw stopped the discussion by saying, "Such operational matters do not need Board consideration." I was shocked. I had assumed that the corporation's financial health would be at the top of the new Board's agenda. Instead, it appeared that such matters were to be handled privately between Cliff and Bob.

Next day I wrote Joe Robinson a note to be relayed to Bill Moyles, repeating my concern for the financial future and volunteering that I had no confidence in Cliff Brokaw. Joe called me a few days later to suggest that he, Bill Moyles and Cliff Brokaw would meet with me the evening before the next month's Board meeting. The message seemed clear: Cliff is in charge of Intelcom. The next day I resigned.

Bob Mertz was appointed President of Intelcom Rad Tech. As of the following March 31, 1974, Intelcom Industries reported a loss of $1.7 million on $10.5 million revenue; current liabilities exceeded current assets by $1.2 million. The loss was even worse than I had predicted. In spite of the loss the balance sheet showed a positive stockholders' equity of $1.3 million. Closer inspection reveals that Gulf had exchanged $1.9 million of its notes for preferred stock to clean up the balance sheet and to encourage the bank to renegotiate its loan. Bob Berry had been right when he told me, "Gulf won't let you fail."

I'm still surprised at Intelcom's longevity. It survived, after a fashion, as IRT Corp. for 21 more years. For the next four years the annual reports showed losses ranging from $1.4 million to $2.7 million. During this time all the former divisions of Intelcom Industries were sold for much less than their accumulated losses. In 1977 Gulf's entire interest (originally valued at $3 million, was purchased by a group of ten IRT employees, reportedly for approximately $60,000. The following eight years showed modest profits and losses, all less than $0.4 million. Then a profit of $1.0 million in 1985 was followed by a loss of $4.4 million in 1986, at which point Bob Mertz resigned and Cliff Brokaw took over as CEO.

In Dec. 1988 the remains of the government contract business was sold to Maxwell Labs and IRT concentrated on developing automated inspection equipment for commercial markets, especially an X-ray system to inspect the quality of printed-circuit-board solder joints. Five employees, including Bob Mertz, left to form a competing company, Four Pi Systems Corp. IRT sued, settled, then sued again. In August 1994 a hung jury left IRT insolvent, so it filed for Chapter 11 bankruptcy. Its assets were sold to Thermo Electron Corp. where it was merged with Nicolet Imaging Systems, a subsidiary operating in a related business,. Six years later Nicolet was sold to GenRad (formerly General Radio). Meanwhile, Four Pi Systems became part of Hewlett Packard's Agilent spin off. So, by a circuitous route both organizations eventually found homes as parts of major manufacturers of electronic test equipment.

Looking back it appears obvious that a merger between Rad Tech and Maxwell Laboratories would have been a technical and financial success. Gulf would have realized an effective sale price well in excess of $4 million, counting the value of stock. Instead they received about $400,000 more than the value of the receivables, i.e., less than the settlement cost of the lawsuit Rad Tech employees filed for their severance pay.

In retrospect, the Intelcom offer had all the attributes of a classical con: make an offer the mark will love, one almost too good to be true, then get the mark so heavily involved that he cannot withdraw when reality sets in. I think if Gulf had withdrawn from the deal in the spring of 1973 the Intelcom stockholders would have sued, since their investment would have been zeroed by immediate bankruptcy. Perhaps Art Rolander and Creighton Gallaway suspected that, so they recommended to Gulf that the sale proceed. If so, Creighton's promise to my colleagues that Gulf would repossess Rad Tech if Intelcom defaulted on its commitments was particularly hypocritical. I wasn't savvy enough to recognize it.

I didn't take Intelcom's offer seriously at first, but Creighton Gallaway assured me that Intelcom had the necessary financial backing. I wanted to quit when it became obvious how bad Intelcom's finances were, but my senior advisors unanimously asked me to stay. Eventually, I left because I refused to condone the misinformation that Bob Berry and Cliff Brokaw provided Gulf and the bank. I thought that Bob Mertz would reform the operation quickly when Bob Berry was finally fired in 1975, but he appeared to continue promoting image over substance. The IRT finances gradually improved during the early 1980's, but Bob Mertz was promptly blamed for the large loss in FY1986.

For many years IRT was effectively bankrupt: its current balance and stockholder equity were negative and its loans were in default. Yet the bank was persuaded repeatedly to be tolerant; recovery was promised to be, "just around the corner". Foreclosure would have netted the bank very little: only the furniture and some scientific equipment whose usefulness required the IRT staff. Only by letting IRT operate could the lenders hope to recover their investment.

MY TECHNICAL THREAD

While the organization around me changed from a small radiation effects group to a GEES division and then to an effectively bankrupt corporation, I retained a foothold in technical work thanks to John Chiment's excellent management support. He advised me and executed the policies and plans we developed together. We complemented each other: John was practical and somewhat hard-boiled; I was more tolerant of scientist's and engineer's idiosyncrasies. I usually asked John to review my memos; sometimes he responded, "Vic, now that you've written it down, drop it in the wastebasket." He was usually right. While sometimes he disagreed with my decisions, I could always count on him to execute them faithfully. Thanks to John, and in spite of the organizational turmoil, I could still devote part of my time to technical matters.

My technical activities evolved with market opportunities. At first I concentrated on radiation effects in semiconductor and insulator materials, gradually transferring responsibility to new PhD graduates, Dr. Gordon Wikner, Dr. Jim Naber and Dr. Terry Flanagan. Then we expanded into radiation effects on electronic devices (e.g., capacitors, resistors, transistors, microcircuits), with Bob Overmyer in a key role, and electronic equipment (e.g., Falcon missile guidance, SleighRide instrumentation), so I had to learn electronic circuit analysis. We transferred some electronic engineers from GA Electronics Division and from Convair and hired some from elsewhere. Thermo-mechanical effects of pulsed X-ray exposure came next, for which Ralph Stahl quickly became the leader.

As part of our correlation efforts we performed experiments at an underground nuclear test to demonstrate that the effects of the radiation pulse from a nuclear explosion could be accurately predicted from laboratory exposures and analysis. We participated in the Diluted Waters event at the Nevada Test Site in June 1965 with John Chiment serving as our project officer. I helped design the experiments, set up the recording instrumentation and analyze the data. Test articles were exposed above a tapered vertical pipe leading down to the nuclear device. Response signals were transmitted by thousand-foot long signal cables to a group of trailers housing oscilloscopes and tape recorders. During the test all functions were automatically initiated by timing signals, while the test participants observed from a location a few miles away from the experiment assembly.

Underground Nuclear Test

Underground nuclear tests of electronic equipment used a tapered evacuated pipe to transport radiation from the nuclear explosion to the test article. Closure devices along the pipe, called "stemming", are designed to contain the radioactive debris after the radiation pulse has passed. Tests were performed in both vertical lines-of-sight in drilled holes and horizontal ones in mined tunnels.

A few seconds after the detonation we saw an unwelcome sight: a dirty orange cloud signaled a leak of radioactive gas, i.e., a partial failure of stemming. Those of us planning to recover data records from the recording trailers quickly suited up in overalls, glasses, gloves and hoods with all seams taped, in spite of the ambient temperature in the 90's. When reentry was allowed we raced to our trailer, inserted slides into the detachable Polaroid camera backs without developing the films and carried them out to safety. After this race to protect our valuable data I almost blew up when the radiation safety people arrayed our camera backs on hot asphalt in preparation for their radiation surveys. Afterwards we shipped the camera backs with undeveloped pictures to the Polaroid Company for special processing to preserve the images. Our 4000-speed negatives were readable and Polaroid was able to preserve them with a fixing solution. The positives were all fogged; we dubbed them pictures of polar bears in snowstorms. Projects that had used 10,000-speed film lost all their oscilloscope data. Of course, data recorded on magnetic tapes were unaffected. We recovered almost all our data and our experiment was declared successful.

My organization participated in one or two underground nuclear tests per year for the next few years. A few investigated transient radiation effects, but most of them were devoted to thermo-mechanical effects. Some also supported military systems. We varied the fielding staff to spread the travel burden and share the test experience. Ralph Stahl

Nuclear Electromagnetic Pulse Phenomena

Electrons are ejected energetically from atoms when the radiation pulse from a nuclear explosion interacts with matter. The resulting displacement of charge produces a radiating electromagnetic pulse in the atmosphere and induces currents into nearby conductors. A high-altitude nuclear explosion can produce a fast-rising intense electromagnetic pulse (EMP) that exposes areas on the earth with dimension hundreds to thousands of miles. Spacecraft exposed in vacuum may have large currents induced on them by the emission of electrons from surfaces, called system-generated EMP (SGEMP). When the radiation penetrates the equipment wall and emits electrons within the electronic assemblies it is called internal EMP (IEMP). Within the atmosphere the effects are modified by conducting air in the radiation source region, and are called source region EMP (SREMP) effects.

became a key leader in underground nuclear testing for two decades, first at GA and then at Jaycor, branching out into thermo-mechanical effects, SGEMP, IEMP and SREMP experiments.
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Meanwhile, our laboratory work branched into Internal Electromagnetic Pulse (IEMP), the effect on electronic assemblies of secondary electrons emitted from surfaces and transferred between materials. Dr. Eric Wenaas, a theorist with a background in surface physics, quickly learned IEMP physics and proved to be a skillful analyst and entrepreneur. His initial work was with Douglas Aircraft for the Spartan anti-ballistic missile. Then Eric and I together developed programs in effects of the wide-area nuclear electromagnetic pulse (EMP) that combined his skill in electromagnetic coupling with my appreciation for electronics function. Together we also explored the System Generated Electromagnetic Pulse (SGEMP) produced on and inside spacecraft by exposure to radiation.

I became involved in a controversy over EMP effects on the commercial telephone system. I believed that the equipment's normal encounters with lightning and other electrical interference produced a considerable degree of tolerance to pulsed electrical disturbances. Others were predicting immediate EMP-induced failure. We all agreed that EMP tolerance was not ensured by natural stresses and that the complexity of the telephone system prevented achieving high confidence in its EMP survival. I recommended that the military should plan to use the telephone system during conflict, but be prepared for its failure by having a backup capability for the most critical communications. Others recommended that a more expensive broad-capability survivable backup be developed.

Techniques for EMP testing were particularly controversial, because major test facilities incurred large costs for facility construction and operation. I earned some enmity by advocating segmenting the problem between electromagnetic coupling and equipment tolerance. Such a partitioning would allow analysis to be supported by simpler tests, such as low-intensity exposure to electromagnetic fields and direct-drive electrical stimulation of electronic equipment. Others wanted to develop increasingly complex and expensive large-scale high-intensity test facilities.

CONSULTING

I resigned from Intelcom at the end of January 1974 and sought consulting jobs. Two major customers soon engaged me: Defense Nuclear Agency (DNA) and Jet Propulsion Labs (JPL). R&D Associates, a company formed as a mutually agreeable spinoff from the Rand Corp., was under contract to provide technical review and advice to DNA, so Mr. Pete Haas, the Deputy Director for Science and Technology (DDST) at DNA, suggested that I be engaged by RDA as a consultant. That work proved to be particularly interesting. Not only was I asked to provide

X-Ray Generation

Two approaches to generating intense X-ray pulses are available, both starting with high-power electrical pulses:

> *Create a hot plasma to radiate X-rays. This works efficiently for X-rays of 0.1 to a few keV, but becomes increasingly difficult at higher energies.*
>
> *Interact energetic electrons in a target to generate bremsstrahlung (i.e., "stopping radiation"). This works efficiently to produce X-rays of MeV energies, but becomes decreasingly efficient at lower energies.*

analysis and advice on areas in which I had worked (TREE and SGEMP), but DNA also asked for advice on their large simulator and pulsed-power program. I learned a lot about megaVolt, teraWatt power technology and high-temperature plasma physics.

DNA was funding three major participants to develop advanced technologies for simulating the X-ray pulses emitted by nuclear explosions, Physics International, Maxwell Laboratories, and Naval Research Laboratory. The requirement was to produce intense, short (10's of nanosecond) pulses of X-rays with energies that could penetrate to electronic devices (10's to 100's of keV). Generating the required electrical pulse used scaled up existing technology, but finding ways to convert the electricity into X-rays efficiently required innovation.

For JPL I analyzed the effects of Jupiter radiation on Voyager spacecraft electronics. The design of the electronic instruments for Voyager had been almost complete when the Pioneer 10 spacecraft passed near Jupiter and measured the radiation levels to be at least ten times more intense than previously expected. All the Voyager designs had to be reviewed for their tolerance to the higher exposure, and tests had to be performed on devices and circuits to verify the analysis. I prepared and delivered a lecture at JPL to familiarize the design engineers with Voyager's specific radiation-effects challenges and solutions. Then I analyzed each of the electronic designs, met with the design engineers, and recommended changes and tests. I was very impressed with the engineers' skill and dedication. In my opinion designers of military equipment did not appear to take radiation exposure seriously enough. All of us hoped that their systems would never encounter the specified radiation environments during operation, because that would imply nuclear war. The Voyager designers knew that their equipment would encounter the Jupiter radiation belts and were eager to avoid the embarrassment that would result if their circuits malfunctioned during the Jupiter fly-by.

One particular conversation was especially memorable. The navigation subsystem used a microcircuit operational amplifier with a very tight specification on its offset voltage (the input voltage difference at which the output is zero). I suggested that a few units be exposed to the specified radiation dosage to determine its effect on offset voltage. The designer blanched, informing me that only three units meeting the specifications remained from a previous space program, and that the vendor was no longer able to produce them. Two of the units were committed to the two Voyager spacecraft; the third was needed for a prototype test article. No way would they sacrifice even one unit to a radiation test! In the subsequent discussion I learned that these special units had been selected from a large manufacturing batch, most of which did not meet the offset voltage specification. I judged that the rate at which the offset voltage would change under irradiation did not depend on its initial value, so recommended that a group of rejects be exposed. The results of that test were then used to specify the thickness of radiation shielding needed for the flight units.

Irradiate and Anneal

Some effects of radiation exposure can be removed by subsequently raising the material's temperature, i.e., annealing the damage. If the anneal is perfect, i.e., restores the material to its original state, an irradiate and anneal cycle can predict the response to a subsequent radiation exposure. However, the measured property may return to its original value after annealing without completely eliminating the microscopic damage. The response to a subsequent irradiation can then be different.

Another major issue was a proposal by JPL radiation test engineers to perform irradiate-and-anneal screening of flight microcircuits. The devices were to be exposed to the specified radiation dosage to measure their response; those that responded acceptably were to be annealed by heating to 160°C and then incorporated into flight hardware. The engineers assumed that the second exposure during flight would replicate the effects of the first. I objected that the 160°C anneal could leave some residual hidden damage that might affect the response to the second exposure. Unfortunately, the device packages would not tolerate a higher temperature anneal, one that would eliminate all damage. I suggested that a few devices be tested before committing to a large-scale program, i.e., perform irradiate-and-anneal and then expose them to the specified radiation again. My fears were confirmed; a fraction of the second exposure restored the damage produced by the full first exposure. NASA saved about a million dollars for testing and a potential electronics failure in space. Instead, irradiation data were used to design local radiation shields when other design solutions were unavailable.

The two Voyager spacecraft were launched in 1977 and encountered the Jupiter radiation environments in 1979. Voyager I was targeted to pass closest to Jupiter, in the most severe radiation environments. If it succumbed to radiation, Voyager II could be programmed more conservatively to meet the most important Jupiter objectives. If it succeeded, Voyager II could be steered into an orbit that would enable it to reach additional planets later in its mission. Voyager I exhibited a few unexpected, but harmless, computer resets that were later attributed to electrostatic discharges, but otherwise survived the encounter intact. Therefore, Voyager II was able to encounter not only Jupiter and Saturn, but also Uranus in 1986 and Neptune in 1989. In June 1981 I was presented with the NASA Public Service Award in recognition of my contribution to the Voyager mission.

I also consulted for Los Alamos Scientific Laboratory, which was a major participant in developing controlled thermonuclear fusion reactors using magnetic confinement of a high-temperature plasma. The material facing the reaction volume, the "first wall", is particularly critical, because it must tolerate extremely high temperatures and intense nuclear radiation. I assisted the Los Alamos personnel in analyzing existing radiation effects data on candidate first-wall materials and predicting effects of the expected exposures in a fusion power reactor.

Thermonuclear Fusion

High temperatures and/or pressure are required to force together positively charged ions, such as the heavy isotopes of hydrogen, deuterium and tritium, but their reaction produces a large energy release. Magnetic fields in a vacuum chamber can be used to contain the ions (plasma) while they are heated. Successful ignition of the plasma produces high-energy particles that impact, and may damage, the container wall.

My DNA consultation inserted me into a controversy on SGEMP simulation between two dominant personalities, Mr. Peter Haas, the DDST at DNA, and Mr. John Darrah, senior advisor on EMP and SGEMP to the Air Force Weapons Laboratory at Kirtland Air Force Base, NM. Pete's approach emphasized the effects of low-energy electrons emitted from the outside of spacecraft by X-ray exposure; John preferred to emphasize the effects of higher-energy electrons transferred between materials on the inside. Of course, both could be important, but significantly different pulsed-power technologies were required to produce relevant X-rays. I tried unsuccessfully to bring them to a common ground, but they were too committed to fighting a turf battle with multi-million dollar stakes for simulator development.

MISSION RESEARCH CORPORATION – PHASE I

After a year and a half as a consultant, I tired of just giving advice and looked forward to participating directly in planning, conducting and analyzing experiments. After considering a few options I chose to form a San Diego office of Mission Research Corporation (MRC). MRC had been founded in 1970 by Dave Sowle (previously Rad Tech's Theory branch manager), Conrad Longmire (an outstanding theoretical physicist from Los Alamos) and Paul Fisher, a theorist from GE Tempo. MRC had an outstanding reputation for government contract research in theoretical physics and I hoped to add experimental physics and electronic engineering capabilities.

The MRC San Diego office expanded gradually. I hired a combination office manager and secretary, a junior scientist and performed some tasks jointly with IRT. I learned that Mr. Jim Raymond, an outstanding electronic device engineer, was interested in leaving Northrop, so we recruited him eagerly. MRC San Diego and Santa Barbara together sold a joint SGEMP experimental/theoretical program. Jim received support on long-term ionization effects in Metal-Oxide-Semiconductor (MOS) devices. We moved to an office with unusually low rent in downtown La Jolla. We hired a number of other physicists and engineers; some of them were disaffected IRT staff.

SGEMP programs up for competition included design support for a proposed major DNA test facility, the Satellite X-Ray Test Facility (SXTF). MRC responded with General Electric Co. as a subcontractor to a request for proposal to serve as technical coordinator of the facility's development. We prepared a proposal in response to DNA's request. DNA sent us a set of questions about our proposal that had to be answered promptly, but I was in St. Louis, MO performing tests at the McDonnell Douglas lightning test facility. Burr Passenheim flew to St. Louis from San Diego and two General Electric representatives flew out from King of Prussia, PA for us to spend a long evening preparing our answers. The next day the procurement was cancelled at a high level in the DoD. That is one of the risks in government contracting: overhead money is spent to prepare some proposals with no return.

We explored other areas for contract support. We analyzed pulsed electrical damage in semiconductor devices, including solar cells. We addressed the difficult problem of quality assurance for nuclear hardness, i.e., how to eliminate the low-probability device failures that could jeopardize a system composed of thousands of devices. Jim and I provided

Electronics Quality Assurance

An electronic function is performed by a number of devices operating together. The properties of each device type vary somewhat between units and may change with use and age. Quality assurance applies procedures and tests that attempt to ensure that all the devices' properties remain within requirements for proper functioning throughout the equipment life and exposures. It is particularly difficult to deal with "outliers", a very small fraction of devices that may degrade faster than the rest.

advice on radiation hardening for the next major space venture, the Galileo Orbiter and Probe to be sent to Jupiter. I measured pulsed HF and UHF signals emitted by high-explosive detonations. I led a multi-corporate program to develop simulation concepts for SREMP, i.e., the region around a nuclear explosion in which EMP is generated by ionizing radiation. Dr. Joe Chervenak and I finally solved a long-term mystery about the signals induced into coaxial cables by ionizing radiation. We also developed techniques for measuring the properties of ionized air at high electric fields and intense ionization levels with a parallel-plate ionization chamber. These experiments were my first to use a digitizing oscilloscope and real-time computerized data processing. We developed a method to derive the resistivity profile in earth from measurements of the electric field at the surface produced by a long wire carrying alternating current of various frequencies. It was applied at potential MX missile basing locations in Nevada. We also performed experiments on electrical breakdown of soil. Our SGEMP work also led us into the charging and spontaneous discharging of spacecraft dielectrics.

I gradually developed the opinion that DNA, the principal sponsor of my work over the years, was much better at developing technology than at transferring it to the eventual users: electronic design engineers and manufacturers. I became convinced that we needed to develop specification formats (by which nuclear-effects requirements could be described in a legally enforceable manner) and standards (to describe the means by which compliance could be demonstrated) to complement technical reports and handbooks by which design engineers could be educated. DNA managers tended to discount my views as marketing ploys. Dr. Ed Conrad, then the DDST at DNA, invited me to become a temporary government employee to convince his associates. By 1982 all our children were employed or at college, so we were no longer constrained to live in San Diego. In June 1982 I took leave-of-absence from MRC, June and I moved to a tenth-floor apartment in Arlington, VA, and I started serving as an SES-04 civil servant at DNA.

Figure 19. Family in Dec. 1979

DEFENSE NUCLEAR AGENCY

I served as a government employee from June 1982 through July 1983. It was enjoyable, although not nearly as productive as I had hoped. It was nice to work only 40 hours per week for the first time in 25 years, and to experience the culture and ambience of the northeast USA, especially its scenery and sense of history. I studied a little about computers, first by assembling a HeathKit computer. Then I studied the assembly language version of our Apple II+ Applewriter word processor, and modified it so that June could easily exit to type messages for her aide and re-enter.

As a Special Assistant to DDST without a direct programmatic responsibility, I had expected to play a strong role by incorporating technology transfer into long-range planning. I had hoped to work closely with individual program area officers while they developed their plans. I suggested that the SGEMP program would be a good first step, since I was already familiar with the technology and existing program. It didn't happen. The program officers were, "too busy". Instead I was asked to work on the other face of the technology-transfer coin: preventing leakage of critical information to potential adversaries. I assisted in the classification review of documents and in the preparation of revised classification guides. On a more positive note, I helped prepare a video on the effects of nuclear explosions to be used in the training of defense acquisition program managers.

I assumed that an outsider's view would be useful to the DDST and his three assistants, so I offered them frank comments about the organization and its meetings. I was disappointed by the planning process: it seemed focused on doing next year more of what was being done this year. The idea of asking, "Where do we want to be in five years?" and making plans to get there was not accepted by the planners. I also learned to appreciate the control the organization exercises on career civil servants: it is dangerous to venture too far from the conventional path for fear of making a career-ending mistake. Probably, some considered me a loose cannon, someone who could not be controlled because I had no civil-service aspirations.

During my service at DNA a political firestorm was stirred up when some of the staff at the Rand Corporation prepared a briefing alleging that the nuclear effects community (i.e., DNA and its contractors) were overselling the seriousness of the effects of high-altitude electromagnetic pulse (HEMP) produced by exo-atmospheric nuclear explosions. Since the Rand study was sponsored by the Defense Advanced Research Projects Agency (DARPA) it had credibility. Its message was eagerly received by equipment acquisition program managers, who didn't relish devoting scarce resources to HEMP countermeasures. Actually, a good understanding of electromagnetics and frequency analysis was required to appreciate the flaws in their technical argument. I supported the DNA program managers in preparing a technical rebuttal. After some sparring between DNA and Rand, a meeting was held in the office of the Assistant Secretary of Defense for Atomic Energy, Dr. Richard Wagner. Participants included the director of DARPA, Dr. Robert Cooper, the Director of DNA, Deputy Directors and assistants to DDST, including me, and the principal Rand Corp. briefing authors. The agenda called for the Rand authors to present their view first, followed by DNA's presentation. The Rand briefing was presented, but just as the DNA representative stood up Dr. Cooper, the director of DARPA, proclaimed that he had never seen such unprofessional conduct, referring to the DNA personnel. Everyone was stunned. After no one spoke, I said, "I take very seriously being accused of unprofessional conduct, and request an explanation". He simply restated his accusation, after which Dr. Wagner adjourned the meeting. After my government service ended I wrote Dr. Cooper a letter, asking again for his reasoning. He never answered. I was shocked that the director of the lead DoD agency for research and development would condemn an open discussion of technical disagreement, presumably because his agency had sponsored the position that was about to be discredited.

Since Ed Conrad was retiring, applications for the next DDST were sought in early 1983. I applied, just in case my approach might be welcomed. I was found "unqualified", presumably because of my lack of experience in dealing with the Department of Defense bureaucracy. Dr. Marv Atkins took over, so I sent him a memo with suggestions for my activities. After a few weeks I asked for his response; he didn't even remember reading my memo. Since he seemed uninterested in my contributions, I suggested that my DNA service end on July 31. He agreed.

Withholding from my paycheck for retirement produced an interesting sidelight on government bureaucracy. When I first signed on at DNA in June 1982 I was told that retirement deductions would go to the Government Employees Retirement program, but would be refunded at the end of my temporary service. When I was about to leave in July 1983 I was asked to sign a form transferring the amount already withheld to social security. I agreed, but requested that a revised W-2 for 1982 be issued immediately, since I had already satisfied the maximum 1982 deduction as an MRC employee. Nine months later, in April 1984, I still had not received it. During a business trip to Washington, DC I asked a DNA administrative officer for help. He called the finance people at Ft. Monmouth and requested them to issue a corrected 1982 W-2 immediately. I drove there and picked it up in time to file a corrected 1982 return with my 1983 tax return. Naturally, I included the expected 1982 refund in the calculation of my 1983 obligation. A month or two later I received a letter from the IRS threatening me with dire consequences if I did not pay my 1983 tax obligation. I called them and was told that the IRS does not process corrected returns until it completes current returns, so they would not credit me with the excess 1982 tax until later. Meanwhile, they promised to become increasingly aggressive in their demands, but I decided to wait it out. The corrected return was finally processed, but I received no credit for the government's use of my money. A corporation could have been fined severely for being that unresponsive to my request for a corrected W-2.

MISSION RESEARCH CORP. – PHASE II

I returned to the MRC payroll on Aug. 1, 1983 but remained in the Washington, DC area for a few months to explore new business opportunities. I observed strictly the restrictions on selling to former government associates. Technically, I had never "sold" contracts to the government, since I had not participated in formal negotiations for contract awards. However, I believe in the spirit of the regulations although they are commonly skirted, which is that you should not use your acquaintance with former government associates to promote contracts. Instead, I explored possibilities with customers other than DNA in medical equipment, utilization of information from earth satellites and high-power electromagnetics.

When I took leave of absence the previous year various MRC associates had assumed responsibility for my remaining contract commitments. Most of them were not eager for me to resume work on those programs, since that might dilute their credits in the company incentive compensation system. So I had to find new areas of contract support.

My major new contract area turned out to be the effects of fast-rising electromagnetic pulses on electronic equipment. A group of civilians working at the Army Missile Command (MICOM) in Huntsville, AL had discovered a pulsed electromagnetic exposure that appeared to be uniquely effective in disabling electronic equipment, including, they claimed, oscilloscopes with which they had tried to record the pulse waveform. During their data presentation I remarked, somewhat impatiently, that I could show them how to make the measurement without damaging the oscilloscopes. A few days later I received a call from an acquaintance at the Army's Harry Diamond Laboratories (HDL) in Adelphi, MD asking me to go to Huntsville with one of his HDL colleagues to make that measurement. I told Dave Sowle, the MRC president, that I needed a charge number for travel and some labor, couldn't tell him for what, but hoped for a sizeable return. He agreed. We made the measurements, helped keep the Army from spending tens of millions on weapon development, and earned about $2 million in contract support to explain the effect and conclude that it was not useful as a weapon.

Joe Chervenak and I resumed high-field ionized-air chemistry experiments. Don Snowden and I continued the work on electrical breakdown of soil, focusing on the role of water filaments in initiating conduction. My previous measurements of RF pulses produced by chemical explosions led to the postulate that they resulted from electrical sparks between charged dust fragments. This led to a number of unsuccessful proposals. Other experiments simulated the tip of the arc formed as nuclear lightning, the lightning-like atmospheric discharges that had occurred during some atmospheric nuclear tests. A major test program was conducted in Pueblo, CO at the Transportation Test Center to measure the electrical and optical characteristics of long arcs in air with currents similar to those inferred for nuclear lightning. We provided instrumentation and analysis for a number of underground nuclear tests to explore the mechanisms of EMP and SREMP generation.

New opportunities to support system development contractors and to provide comments on candidate defense systems was provided by the Strategic Defense Intitiative (SDI) program. I was asked to participate in reviewing LTH-4 programs, which dealt with the effectiveness of intense neutral particle beams for ballistic missile defense. I provided nuclear hardening support to the Hughes Aircraft Ground-Based Interceptor program, which led to the TRW Brilliant Pebbles program. I was eventually replaced on Brilliant Pebbles by another MRC division manager, whose more optimistic but technically unfounded assessments were favored by TRW, the system contractor. The TRW program manager wanted me removed, but threatened to minimize MRC funding if I was allowed to work with TRW's competitor, Ball Aerospace. This was one of the last examples of the problem I encountered repeatedly, others in the company selling our reputation without preserving the quality of our work. It contributed in large measure to my decision to resign in 1991.

Figure 20. Santorini in May 1990

I gradually became disenchanted with my role at MRC. I continued to enjoy the technical challenges, but the requirements to manage and provide revenue to support the San Diego division became increasingly onerous, especially when others in MRC were not dissuaded from trying to compete for my customers. After Dave Sowle retired as president of MRC and was succeeded by Dr. Steve Gutsche the corporate headquarters increased its emphasis on profit and became less responsive to the administrative needs of the divisions. Too many arguments with the corporate administrative support staff in Santa Barbara sapped my enthusiasm. I asked to become a technical advisor, relieved of organizational responsibility but available to assist any MRC project with technical problems. We interviewed various potential replacements to lead the San Diego division. Some would have been excellent leaders, including Dr. Peter Coakley, then at Jaycor, but none of the qualified persons accepted the offer. Apparently, I couldn't become a technical advisor unless I found a replacement manager. Steve Gutsche finally became impatient with my requests and told me to make up my mind, so I resigned as of the end of July

1991 to become a private consultant.

Most of my initial tasks as a consultant were provided by MRC principal investigators. As a consultant I became exactly what I had requested to be as an employee, a technical advisor. In retrospect, I had to resign for two reasons. First, as long as I was an employee Steve wanted me to serve as division manager. Second, the MRC incentive rewards program credited principal investigators with the profit generated by their contracts, but part of the credit could be diverted to other senior staff members whose contribution helped to sell the program. As a result principal investigators were reluctant to involve other senior MRC personnel in their programs for fear of losing part of their credit. If they needed help it was safer to hire an outside subcontractor or consultant.

Eventually, MRC was purchased by a large aerospace corporation, ATK, and a few of the MRC's principals became quite wealthy. Subsequently, the expected synergisms between MRC's capabilities and ATK's aspirations in space hardware failed to develop, and the large-corporation atmosphere was not conducive to the entrepreneurial style required for selling government contracts, so MRC effectively disappeared by 2010.

CONSULTING and VvL,inc.

I hoped to be active as a consultant for about five years, the typical lifetime of government contract areas of research. Previously, as an employee with a company's resources for backup I had developed new lines of work as old ones declined, but I didn't expect that mode to work for a private consultant. I was lucky. Consulting continued for 18 years and covered a variety of topics, on some of which I had no previous experience.

In 1993 the IRS tried to force MRC to hire me as an employee, since my total consulting for MRC on different projects was likely to accumulate more than 1000 hours. The alternative was for me to form a corporation that would provide my services under subcontracts. A subchapter S corporation would simplify tax returns by folding the corporation's income and expenses into our personal tax return. However, our lawyer, after considering my situation, advised me to form a regular C corporation, because it could provide a generous medical-expense reimbursement program for its employees. In this manner our family medical expenses would escape Social Security and Medicare taxes, amounting to 15.3% of these sizeable expenses. The savings would pay for more than the annual corporate legal and accounting costs. Besides, tax-preparation fees would stay in the family because our daughter-in-law, Janice, is a certified tax preparer. So we formed VvL,inc., which remained active until Dec. 2009.

I also established a security-cleared facility with an approved safe and a separate computer with removable hard drives. This allowed me to perform classified work in my office. Also, after a few years the security inspector delivered a STU-II telephone that enabled me to participate in classified phone calls. The price for the secure facility was a certain amount of paperwork and annual inspections. My initial security inspector, Mike Malmgren, was particularly helpful by telling me exactly what I needed to do. When it came to close up VvL,inc. at the end of 2009 I mailed any material requested by my remaining Air Force customer. Then I drove to the Los Angeles area to deposit my classified holdings in a certified incinerator. VvL,inc. was officially dissolved on Dec. 31, 2009.

High-Power Microwave and Fast-Pulse Effects on Electronics

At MRC I had been Principal Investigator of a program investigating for HDL the effects on military electronics of high-power microwave (HPM) exposure. This program followed our successful contract to determine the mechanisms of fast-rise electromagnetic pulse effects on electronic equipment. The Army project officer hoped that I would continue to provide technical support, so MRC promptly issued me a consulting agreement. Dr. Mike Bollen, located in the MRC Washington, DC office, assumed the role of MRC Principal Investigator. Dr. Don Snowden in the San Diego office continued to participate and became my primary technical contact. That program lasted 5½ years and covered high-power microwave effects on artillery fuzes, military radios, infra-red-sensor fuzed armaments and anti-vehicle mines. I assisted with test planning, test fielding, data processing, response analysis and report writing.

> **High-Power Microwave Effects**
> The ability to generate, radiate and focus microwaves at very high intensities opens up the possibility of military uses to affect adversary electronic equipment. Exposure induces currents to flow on the target surface, part of which couple onto the circuit wiring and disturb or damage the electronic devices. The effectiveness depends on microwave frequency, intensity and pulse format.

One of the challenges of HPM research is determining the microwave frequencies for optimum effectiveness, which usually depends sensitively on the target configuration. Testing with high-power exposures at various frequencies is costly, both in the cost of the microwave generators and in the replacement of targets if they are damaged. Low-power exposures can be used if a sensor is available to measure the relative response inside the target assembly, such as the voltage induced at critical circuit nodes. I guided the design of a differential voltage probe: a sensor to measure at frequencies up to 5 GHz the voltage induced by microwave exposure between two nodes in a target electronic assembly. It was used to measure the frequency dependence of the coupling at selected critical circuit nodes during low-level swept-frequency microwave exposure. In typical target equipment we observed variations as large as a factor of 100 in coupling cross section with small changes in frequency. I understand that Mike Bollen and his associates continued to use the probe design long after I was no longer involved.

In 1993 I received a small task in fast-pulse electromagnetics. During the 1980's I had managed the installation at the Army's Harry Diamond Laboratory of a 2-MV fast-rising electric pulse generator developed by Pulsed Sciences Inc. (PSI). Part of MRC's responsibility had been the design and construction of a shaped metal transition to convert the generator's coaxial output in transformer oil into a vertically polarized plane wave in air. In 1993 PSI was awarded a contract to upgrade the 6-MV pulse generator and transition at the ARES simulator at Kirtland Air Force Base, NM. MRC was their subcontractor for the transition design, and I served as their consultant. Don Snowden, Dr. Sheng Tang of MRC and I designed and performed experiments with a scale model of the transition, which included a graded dielectric to convert the transmission-line impedance smoothly from the oil dielectric to air. Our design was then fabricated, installed at ARES, and tested.

Light-Induced Microwave Emission and Infrared Missile Countermeasures

In August 1992 I started for the Air Force what eventually became my longest lasting consulting area: Light-Induced Microwave Emission (LIME). The goal of the LIME program was to discover exposure/target combinations in which the LIME-induced signals would be militarily useful. The most important application was for the defense of aircraft from infrared guided missiles. Since the effect of the light exposure on the infrared detector was much larger than the disturbance produced by LIME, this program evolved into more conventional seeker interference/jamming, albeit with high enough intensities to require less sophisticated pulse modulation and possibly produce permanent damage in the target sensor. I learned a lot about missile-guidance electronics and infrared detectors, including focal-plane arrays (i.e., rectangular detector arrays that provide a target image at each frame time). I completed my last task in this area in 2009, although there were a number of contract gaps during the 17-year course of LIME.

Light-Induced Microwave Emission

Light pulses can produce an electrical response on some materials. The effect is used in carefully selected materials in photo-detectors, but at sufficiently high intensities it can occur inside almost any material or on any surface. Development of lasers enables delivery of intense light pulses to targets at militarily useful ranges. It also provides flexibility to modulate the light intensity (e.g., pulse width and repetition rate) for optimum effectiveness of the induced electrical signals.

Infrared-Guided Missile

Infrared detectors may be used by anti-aircraft missiles to sense the location of the hot engine exhaust from a target aircraft. The missile is steered until the infrared seeker views the target at a constant orientation; that produces an intercept trajectory. Countermeasures for aircraft defense include means to damage the detector or inject false signals into its electronics.

I thoroughly enjoyed working with Dr. Neal Carron at MRC and Dr. Jim Gilbert at Metatech on the LIME program. It challenged us to learn about areas of physics with which we were not previously familiar. What were the mechanisms by which intense light could be converted to electricity in solid matter and at surfaces? This process is well understood for semiconductor photo-detectors, but how about transparent crystals like ZnS or even metallic surfaces? Together we wrote reports that brought together the physical mechanisms and experimental results on solid-state LIME and plasma LIME.

When the LIME program evolved into a study of infrared seeker countermeasures it also encountered intra-Air-Force politics. The historical center for seeker countermeasures was at Wright-Patterson Air Force Base (WPAFB) in Dayton, OH, but the LIME program was managed by the high-power laser experts at Kirtland Air Force Base (KAFB) in Albuquerque, NM. When testing of missile seekers commenced at the LIME contractor, Textron Systems Corp. in Wilmington, MA I recommended that they start by using light pulses from flash lamps instead of lasers to avoid prematurely damaging the valuable test items. The WPAFB personnel ridiculed our approach as naïve, arguing that a focused light spot would produce an unrealistic dependence of response on seeker look angle. We responded that our light spot was deliberately defocused to produce a realistic dependence. This was only the first round in a turf battle that lasted more than ten years.

Working with personnel at Textron turned out to be particularly satisfying. Their program manager, Dr. Dan Trainor had a good understanding of the subject, hired capable scientists and engineers, and encouraged them to innovate. Dr. Chaz von Rosenberg, with a background in fluid mechanics, served as senior scientist, valuable consultant and good friend. Dr. Daisy Chawla, a young physicist with a laser background, participated easily in our discussions and eagerly learned a lot of plasma physics. Her cousin, Dr. Kalpana Chawla, became an astronaut and was killed in the Columbia disaster. Dr. Charlie Pike was a mature laser developer who led the seeker testing. Dr. Richard Slater, an experienced laser designer, performed experiments on solid-state LIME and developed lasers for exposure of seeker assets. They all cooperated with me as if I was a member of the Textron team: we shared in open discussions of the data and their interpretation, and together formulated experiment plans. Overall, Textron personnel provided one of the most pleasant associations of my consulting career. I'll always remember the hike with Chaz in the New Hampshire mountains the Sunday after 9/11/2001, when I was stranded on the east coast by the airline grounding.

More realistic testing of seeker assets used Hardware-in-the-Loop equipment at Eglin Air Force Base, FL. Their Guided Weapons Evaluation Facility (GWEF) provided a computer-controlled simulation of the response of a missile in flight to its seeker's guidance commands while the seeker was exposed to realistic target and interference simulators. Three different seekers were tested, producing almost one hundred data CD's with data. I became the principal analyst to process these data and derive useful conclusions.

By mid 2003 the LIME program moved on from traditional scanned single-detector seekers to systems that used staring focal plane arrays (FPA's) as their infrared sensors. I wrapped up the previous work by preparing a summary of repetitively-pulsed laser effects on conical-scan seekers, including the flash-lamp and laser tests at Textron, laser tests at Air Force Research Laboratory in Albuquerque, and hardware-in-the-loop tests at GWEF. Meanwhile, I supported the FPA test program at Textron and periodically prepared analyses and summaries of FPA test results.

Scanning vs. Staring Seekers

A seeker using a single detector must be scanned over its field of view, usually by a rotating reticle, to locate a target,. A focal-plane array, with many detectors, can locate its target by where on the array the image appears. It also provides more information to evaluate the image for possible countermeasures.

A few tasks for the LIME program remained after 2007, including two test periods at Textron and an outdoor range test at White Sands Missile Range. As usual, I participated and wrote summary reports. My last subcontract task in late 2009 summarized my understanding of FPA response to laser pulse exposures and identified a few remaining mysteries.

Effects on Electronics of Nuclear Radiation

In 1998 I started another new task, this time for the Defense Threat Reduction Agency (DTRA), the successor to DNA. In light of the nuclear test ban treaty DTRA was responsible for archiving the accumulated knowledge on the effects of nuclear explosions. The technical coordinator for the nuclear-radiation part of the Graybeard program, Ed Conrad, assigned me the task of preparing the guide on transient radiation effects on electronics (TREE). I nominated Jim Raymond, formerly my associate at MRC, and Clay Rogers, formerly of the Sandia Corporation and RDA, to assist. Clay asked Dr. Hap Hughes of the Naval Research Laboratory to assist him with the section on long-term ionization effects on Metal-Oxide-Semiconductor (MOS) devices. I also prepared a summary of the responses of electronic equipment exposed at nuclear tests.

Assisted by Dr. Bill Radasky, founder of Metatech Corp., I prepared a guide to Nuclear Hardness Design and Verification for Electronic Systems.

Figure 21. Our family at my father's memorial, March 1999

1998 was also the beginning of an Air Force program on pulsed ionization effects in microelectronics, especially latchup. We had first observed ionization-induced latchup in 1964 while testing Minuteman II devices at the GA LINAC. Most electronic circuits recover from their transient disturbance shortly after the end of an ionization pulse, but latchup either destroys the susceptible device or renders it inoperative until power is removed and reapplied. In some devices latchup may be produced by a single energetic heavy atom, such as those that are delivered to the earth's atmosphere in cosmic rays. It has continued to haunt designers of equipment potentially exposed to ionization pulses. There are techniques for avoiding latchup, such as using fully dielectrically isolated devices, but they impose other design sacrifices. By 1998 it was prudent to test the recently developed very-large-scale integrated microcircuits for their disturbance thresholds and latchup prevalence. I participated in experiments at the electron linear accelerator located at the Armed Forces Radiobiology Research Institute (AFRRI) in Bethesda, MD and supported data analysis. I remember particularly being at AFRRI in early 1999 when I received a phone call from my daughter, Linda, informing me of my father's death.

Effects of High Altitude Electromagnetic Pulse

In mid 2003 the U.S. Congress tasked a congressional commission to report on the potential effects of a high-altitude nuclear burst on U.S. civilian and military infrastructures. I was assigned to report on the effects of a high-altitude electromagnetic pulse (HEMP) on military aircraft. I reviewed hundreds of reports, talked with key participants in aircraft tests at EMP test facilities, and visited the Boeing Company to learn about their electromagnetic protection techniques, standards and tests. A comparison of the HEMP-induced electrical stresses with those encountered by aircraft exposed to lightning, both nearby and direct strikes, showed that HEMP was more severe at higher frequencies. I worked hard to formulate conclusions and recommendations consistent with all data, avoiding alarmist and whitewash extremes. I wrote a report, but, as far as I know, it is buried somewhere in Washington, DC. That is disappointing.

Figure 22. Caribbean cruise, Aug. 2004

Effects of Ultra-Short Light Pulses

Another new research area opened for me in the middle of 2002: the effects of ultra-short pulses of laser light. Samples of various optical materials were being exposed at the Sandia Corporation in Albuquerque, NM to laser pulses with pulse lengths of 100 femtoseconds (one tenth of one millionth of one millionth of a second). The actual length of the light pulse in space is only about one mil (.001"), i.e., the distance light moves during 100 fsec. I helped plan experiments and analyze results. Toward the end of the project I was asked to revise a report on a model to predict the effects. I revised the model, created a simple computer program to perform prediction calculations, and pointed out the prediction uncertainties.

Our Air Force LIME sponsor also asked Neal Carron and me to apply our knowledge of LIME mechanisms to ultra-short laser pulses. We wrote a report but no sponsor was found for the research.

Propagating Intense Ion Beams

The spring of 2006 provided another very interesting task. I was asked by the Air Force to assist Lockheed-Martin in Palo Alto and Sunnyvale, CA with a program that required a high-current beam of ions to propagate to a tight focus in a vacuum. Since a densely charged beam will expand by electrostatic repulsion, it was planned to create and propagate a beam containing equal densities of negative and positive ions to cancel the space charge. I was asked to work with a young engineer, Mr. Luke Fishback, to conduct a laboratory test of the concept with the available ion sources. I thoroughly enjoyed sharing the experience with Luke. He is very bright, absorbed the relevant knowledge rapidly, and quickly demonstrated his understanding by offering good ideas. We performed the required demonstration in early 2007, just as the program was cancelled for other reasons. Unfortunately for Lockheed-Martin, Luke resigned soon thereafter to develop his own business, monitoring electricity usage in homes to facilitate conservation.

MY CONTRIBUTIONS

My most important contribution during my career was probably the training of junior scientists and engineers. A number of scientists with fresh Ph.D.'s joined my organization and became mature investigators. The first one was Gordon Wikner, who joined GA after graduating from University of California (UC) at Berkeley. He was followed by three who came directly from Purdue University: Jim Naber, Terry Flanagan and Don Trueblood. Roland Leadon came with a fresh Ph.D. from UC San Diego, but he had matured as an aeronautical engineer before returning to graduate school. Eric Wenaas was trained in theoretical surface physics, but quickly learned electromagnetics work on a funded IEMP project. He matured rapidly, became highly competitive and eventually served as president of a contract-research corporation, Jaycor. Burr Passenheim originally worked in our laboratory as a high school summer student. Eventually, he returned with a Ph.D. from UC Berkeley. Don Snowden came to GA with a UC Berkeley Ph.D and transferred into my division later. He became a treasured colleague and friend at both GA and MRC. At MRC we hired Joe Chervenak, with whom I spent many rewarding hours planning and conducting experiments and discussing perplexing data. Together we finally solved the mystery of anomalous signals generated in coaxial cables by radiation exposures. The times we spent performing ionized-gas experiments together were among the most enjoyable of my MRC period.

M.S. degree physicists performed most of the detailed work on our projects, such as assembling experimental equipment, taking experimental data and processing the data, usually under the guidance of a Ph.D. San Diego State University furnished a good crop of them including our early acquisitions of John Harrity, Howard Harper and Charley Mallon. I interviewed Bob Overmyer in Chicago during a business trip. He joined us and became a key leader of programs intermediate between research on materials and engineering for systems, such as the response of electronic devices to radiation exposure. I needed help in theoretical work so we hired an M.S. level theorist, Don Nichols. Our work together on the Range of Recoil Atoms led to an article published in the Seitz and Kohler series of books on Solid State Physics. Bob Poll transferred to GA from Convair and became a leader of projects on systems test, analysis and support.

At MRC I resumed hiring M.S. physicists, some newly graduated, to work on our projects. Dave Fromme arrived to work on our SGEMP program after his tour in the Air Force at its Foreign Technology Division. John Smythe and Art Hart worked on semiconductor devices, mostly with Jim Raymond. Julie Lawrence measured effects of electrical pulses on semiconductor devices, with emphasis on microcircuit latchup. Jim Erler and Eric Beale investigated electrical breakdown in soil. Steve Sherwood became our expert on electromagnetic coupling into conducting enclosures, such as electronic equipment boxes.

Once we embarked at GA on programs requiring radiation effects response analysis, testing and hardening of electronic equipment, we needed experienced electronics engineers. Leo Cotter, Bob Imhoff and Don Burkhart transferred from the GA Electronics Division. At that time Convair Division of General Dynamics was undergoing cutbacks, so we were able to transfer Henry Kay, Bob Keyser, Hal Winter, Fred Gatti, Max Donaldson and Marion Rose. Others came from elsewhere, including Sheldon Jurist from North American Rockwell. At first I worked closely with Leo Cotter, but later depended on him and Marion Rose to provide most of the detailed engineering supervision.

I've never been able to match my first hire for a versatile laboratory technician, Ray Denson. During his interview he impressed me as a personable, bright young man. His current employment as a truck driver at a lumberyard was not a particularly useful background for a science technician. His cabinet-making hobby suggested that he had manual skills, so we offered him a job. What a find! We taught him soldering, including silver soldering for vacuum equipment, and he quickly surpassed his teacher. When we required complicated mechanical assemblies he either constructed them himself or prepared drawings for the machine shop. One day the internal parts of our Collins helium liquefier were arrayed on a table for cleaning, but our trained operator had to be hospitalized for surgery. We needed liquid helium for an experiment, so Ray studied the manual, reassembled the liquefier and produced liquid helium the next day. The regular operator feared of losing his job to Ray, but we wouldn't consider wasting Ray on such a routine task!

Our programs at underground nuclear tests required extensive signal conditioning and data recording equipment, so we hired a retired Navy electronics mate, Johnny Johnston, to coordinate the fielding activities. He worked with the engineers and technicians to construct, check out and operate complex instrumentation assemblies.

I was proud that MRC hired my son, Kenny, when he graduated from ITT Tech as an

Electronic Technician with an Associate degree. I recused myself from any decisions related to his hiring, performance review or salary, but I thoroughly enjoyed working with him on projects with electronics instrumentation challenges. We performed experiments together at radiation facilities in San Leandro, CA and Adelphi, MD. He also provided leadership to keep the MRC laboratory in good order in spite of the bad habits of others, including me.

My personal technical contributions include designing experiments, somewhat improving theoretical understanding, and initiating discussions with my colleagues about apparent mysteries in experimental results. I remember well meeting with a small group in a motel room during an IEEE conference in College Park, PA. John Harrity and Charley Mallon had measured the short-term annealing of displacement damage in silicon using the GA Accelerator Pulsed Fast Assembly, which delivered a neutron pulse with a duration of 7 microseconds. The damage immediately after the pulse appeared to be as much as 50 times more severe than the permanent damage. Other investigators had measured the damage at 1 millisecond to be only 2 to 3 times more than the permanent damage. We considered various explanations, including the possibility that the experimental results had been misinterpreted. Together we wandered into the answer: the observed annealing was caused by a shrinking cloud of space charge around the cluster of defects produced by an atom recoiling from a neutron collision. Roland Leadon subsequently developed a computer model that reproduced the results. His paper won the outstanding paper award at the next IEEE radiation effects conference.

Another contribution that pleased me was a revised model of ionization-induced conduction in insulators. The early TREE workers fit their data to a model developed for photoconductivity in insulators, i.e., the conductivity produced during steady-state illumination with light. This model produced a nonlinear dependence of photoconductivity on light intensity. I noted that there was a fundamental difference between photoconductivity and ionization-induced conductivity: light generated free electrons one at a time, but ionizing radiation generated them along the track of the incident particle. As long as ionization tracks did not overlap the response had to be linear, i.e., the effect of one track cannot depend on the presence of other tracks with which it doesn't interact. At high accumulated exposure the response could be nonlinear in accumulated exposure, i.e., the response can depend on the degree to which the material has been modified by previous exposure, but must still be proportional to the instantaneous intensity. Don Nichols and I presented a paper with our analysis at the 1966 IEEE conference held at Stanford University. John Harrity and Terry Flanagan subsequently performed LINAC experiments with various radiation pulse widths that verified our prediction. Most previous experiments were consistent with either model because they used radiation sources with a fixed pulse width, i.e., changing the intensity automatically changed the accumulated exposure.

I've always emphasized correlation of radiation effects with irradiation type. We understand well the primary collision between an irradiating particle and a target atom. The complexity of the processes that follow the primary collision, e.g., thermally stimulated motion of the products, precludes reliably predicting the eventual effects. However, we can correlate the effects of different irradiation exposures on the same target material by comparing the products of the primary collisions. For example, as a first approximation we assume that equal amounts of ionization energy densities produce equal ionization effects. If necessary, we make adjustments for different local ionization densities along incident particle tracks. A similar procedure is applied to the primary atom recoil energies for displacement effects: first using the total non-ionizing recoil energy, then adjusting for differences in recoil energy distributions.

Most of the nuclear electromagnetic pulse (EMP) community considered me an outsider, because I approached from the view of electronic equipment response. Most of the active members of the EMP community are experts in electromagnetics calculations, so they favor high-quality electrical shielding to provide protection of electrical equipment. My background with electronics led me to advocate sharing the protection between electrical shielding and electronic-equipment tolerance, in a manner similar to conventional protection from lightning and other electrical interference. There are also disagreements about EMP testing: many EMP experts advocate very large and expensive high-power test equipment for threat-level exposures of large systems; for the most part I advocate testing separately the shielding effectiveness at low electromagnetic exposures, and equipment tolerance with current pulses, reserving high-level full-system exposures as a rarely performed check on the methodology. My purpose in becoming a temporary civil servant in Washington, DC was to promote at DNA the development of specification formats and standards for nuclear effects testing, including EMP tests. I argued that this approach paralleled the methods currently used for other types of interference. The counter-argument was that equipment encountered other types of interference during normal operation; nuclear interference only occurred during nuclear war, at which time it would be too late to discover a defect in the methodology. I failed to convince government nuclear-effects managers, although costs have severely limited the use of full-system tests. I'm convinced that a carefully designed discipline with separate equipment and shielding tests would have produced more reliable systems. Hopefully, tolerance to an actual EMP will never be tested.

A measurement in scientific and engineering tests is usually a competition between signal and noise, between frequency range and noise, and between signal magnitude and degree to which the measured node is disturbed. I developed and applied techniques to make reliable measurements for nuclear and electromagnetic exposure tests. During the early experiments on ionization-induced conduction in insulators we reduced interfering signals by using a guarded voltage electrode in between a pair of target samples with ground electrodes on the outside of the samples. Later we minimized radiation-induced interference in pressure sensors to be used at an underground nuclear test by applying this balanced-pair technique.

Pulsed irradiation sources, including LINAC, generate serious electrical interference in their vicinity. Ordinary coaxial cables are excellent for transmitting wide-band signals, but pick up high-frequency interference from currents induced onto their shields. We used configurations with multiple coaxial cables inside a solid metal conduit to suppress noise. Termination treatments forced high-frequency interference currents to flow on the outside of the solid conduit, not on the cable shields. For moveable cables required at off-site test facilities we brought with us a flexible bellows-type solid-metal conduit. Its purchase was costly, but it saved us many hours of noise hunting during experiment checkout.

A peculiar type of signal interference, first observed at the AURORA flash X-ray facility in Adelphi, MD, was dubbed "great white whale" by another investigator to describe its shape on an oscilloscope. Even though the AURORA electrical and radiation pulse width is only a fraction of a microsecond the interfering signal increases slowly and lasts for milliseconds. Joe

Chervenak and I finally explained it as the result of diffusion outward through the generator's metal structure of the sub-microsecond current pulse. Similar signals also interfered with strain-gage measurements during underground nuclear tests. They are particularly insidious, because the large source current and low source impedance enables the induced voltages to appear between nearby places that are obviously connected by metal, i.e., all grounds are no longer equal.

Testing for effects of high-power microwave exposures on electronic equipment sometimes requires measuring the voltage induced between two circuit nodes by microwave exposure. The dependence of the induced voltage on exposure frequency is particularly important, because it determines the relative effectiveness for producing electronics response of different exposure waveforms. We developed a miniature device to rectify the microwave signal near the desired circuit nodes while maintaining high enough impedance to prevent unacceptable loading of the circuit. The rectified signal was carried by wires carefully routed to avoid disturbing the equipment response to a pre-amplifier and lock-in amplifier/meter. The microwave exposure was modulated at a convenient frequency, such as 1 kHz, which was also connected to the reference input of the lock-in amplifier. The meter only recorded that portion of its input signal that was in phase with the modulation, thereby rejecting extraneous interference. This technique measured microwave coupling cross sections varying by factors of a million or more.

My contributions to the LIME program included applying knowledge of FPA structures to data analysis. Images of FPA outputs recorded before and after pulsed laser exposures showed changes, some local near the exposures site, others more extensive, some only for one image frame, others permanent. We needed to interpret the changes in terms of actual damage, such as craters in the detector material, rupture of conductors in the underlying MOS processing chip, and shorts between the conductors and the silicon substrate. I studied literature and visited the Raytheon/Santa Barbara FPA vendor to learn about their FPA architectures. My colleagues and I managed to interpret most manifestations, including some produced by image-correction software, but a few remained mysterious.

I've tried to consistently tell my customer the truth as I believe it, even when he/she didn't like it. Some customers responded favorably. Pete Haas used to deliberately contradict his contractors' representatives to gauge their response. If you presented a reasonable argument for your position and discussed the issue with him logically you gained his respect. If you retreated, you lost it. Some customers didn't want to hear my "truths"; I soon lost them as customers. Just as well; I didn't want to be there when reality eventually caught up with them, although, I admit, some fared remarkably well.

HONORS and AWARDS

At NMMI I was awarded a Longines watch and a handful of medals. The watch was for winning a public speaking contest. One of the medals was for citizenship, presumably because I became a naturalized citizen of the U.S.A. during my study in Roswell. It's probably hard for my family to believe that I won the "Neatest Cadet" medal in 1945. Actually it was for excelling in weekly competitions in the rifle manual of arms that also required a neat uniform and brightly shined shoes. I also won a rifle sharpshooting competition. That came in handy during my Army basic training at Ft. Jackson, SC, when getting my company's highest score on the rifle range earned me a 48-hour pass on the weekend my wife arrived in Columbia, SC.

I was honored to be accepted at Cal Tech, both as an undergraduate and graduate student. During my third and fourth years in graduate school I received National Science Fellowships. My Ph.D. degree was awarded magna cum laude.

During my Army service my supervisor tried to get me promoted from private first class for my technical contributions to the Nuclear Weapons Test Division. The company commander responded in effect, "Not until he has more time in grade". Instead, I eventually received the Army Commendation Ribbon with Metal Pendant for "meritorious service".

Although I was educated as a physicist and participated in the American Physical Society, most of my technical publications and professional society activities were with the Institute of Electrical and Electronic Engineers (IEEE). I presented the first paper on transient radiation effects at one of the last meetings of the American Institute of Electrical Engineers (AIEE) before it merged with the Institute of Radio Engineers (IRE) to form the IEEE. I was the Papers Chairman for the IEEE annual Nuclear and Space Radiation Effects Conference held at Stanford University in 1966. I was elected a fellow of the IEEE in 1977, "for contributions to the understanding of radiation effects and to the application of this knowledge to improve the survivability of military and space systems." In July 1986 I organized a short course on Radiation Effects Testing for the annual IEEE radiation effects conference held at Providence, RI. I received the 1988 IEEE Nuclear and Plasma Sciences Society annual Merit Award, "for contributions to the understanding of radiation and electromagnetic effects on electronics relevant to military and space systems hardening."

On June 2, 1981 I was presented with the NASA Public Service Award, "for outstanding contributions in providing technical expertise to the Voyager Project in the area of radiation effects in electronic parts, materials and circuits".

The Defense Nuclear Agency recognized my government service in July 1983 by giving me its colorful plaque.

The Hardened Electronics and Radiation Technology (HEART) conference was formed during the 1980's to provide a forum for classified and export-controlled publications comparable to the IEEE Nuclear and Space Radiation Effects Conference. A few years later the conference committee established an annual Peter Haas award to memorialize the material contributions to our nation's defense by this former DDST at the Defense Nuclear Agency. I was honored to receive the Peter Haas award in 1993, "for contributions to the development and application of nuclear and electromagnetic susceptibility for hardening and qualifying electronics in survivable military systems."

The Summa Foundation elected me an EMP fellow in 2004, "for contributions to the basic theory of and insight into the mechanisms of EMP and SGEMP.

LESSONS LEARNED and NOT LEARNED

My most important lesson is, "Choose an occupation you enjoy". In spite of difficult times I've thoroughly enjoyed the challenges and accomplishments of my career as an applied physicist. Success as an individual investigator and proposal writer led to responsibility for a growing organization, Gulf Rad Tech, which succeeded but fell victim to Gulf managers' greed. So I started over at MRC to build a small group to perform interesting work. A detour as a civil servant through Washington, DC was interesting but not very productive. Then I built another small program. Eventually, I looked to retirement as an opportunity to continue what I enjoyed the most and to minimize everything else. To me, the technical work was the most fun, but most of it was being performed by people I was supervising. The responsibility to provide funding and administrative support was becoming onerous. Fortunately, by becoming a consultant I was able to continue the fun work for the last 18 years.

Much of my success in developing funded programs was the result of investing in unique capabilities, such as the ability to measure and record small signals with broad frequency content. The first example was the amplifier and recording equipment we assembled for experiments at LINAC. They enabled us to measure and record smaller signals from higher-impedance sources than the capabilities of our competitors. At MRC we specialized in making sophisticated measurements near high-power pulsed radiation sources, i.e., electrically noisy environments. We bought a solid-metal screen box and cable conduit to interconnect experiment assemblies with recording equipment. We brought them with us to off-site test facilities and installed them with specific rules for grounding and shielding to make measurements with negligible interference. Our multi-million dollar program to investigate the effects of fast-rising electromagnetic pulses on electronics was earned by a $10,000 MRC investment in measuring the shape of the radiated pulse at the MICOM test facility. After observing the sensor response the customer asked us to record the background signal. It was a flat line on the oscilloscope. Soon thereafter we received a sizeable contract.

During the late days of Gulf Rad Tech, before its acquisition by Intelcom, we decided to invest in a new capability: an ultra-high-vacuum deposition chamber with diagnostic instruments. We planned to use it to prepare and study pure thin films and carefully treated surfaces. The films could be prepared by various techniques, including evaporation and molecular beam epitaxy. The components were purchased and the chamber was constructed, but the acquisition of Rad Tech by Intelcom diverted funds that would have demonstrated its usefulness and marketed its services. Years later the chamber was scrapped. A huge industry now uses molecular beam epitaxy to manufacture special electronic devices, such as lasers. We chose a promising course, but were denied the opportunity to pursue it.

Another fruitful approach was building capability gradually while accomplishing contract commitments. A Principal Investigator is usually tempted to satisfy the contract statement of work with minimum effort, but a little extra work can usually open up new opportunities. Automating data acquisition at GA is an example. At first it was fraught with difficulties, especially in mixing digital and analog electronics. I spent many nighttime hours trying to suppress interference between relays and digital circuits. My colleagues advised me to return to our tried-and-true manual techniques. But we eventually solved the problems, thereby increasing productivity in both quality and quantity. Our rewards for automation came in later years when we competed successfully for contracts to test large quantities of electronics parts. The profits from those test programs supported the research to develop new contract areas.

Data recording and processing became increasingly automated during my career. In the late 1960's at GA we invested in one of the first digitizing oscilloscopes, a Lecroy unit using multiple sample-and-hold circuits. We connected its output to a magnetic tape recorder, which prepared tapes in a form readable by the mainframe IBM computer. In 1983, while employed by MRC, I first used a Tektronix 7912 digitizing oscilloscope and a Tektronix workstation with 48 KB of RAM to record data from experiments with ionized gases. The memory limit was awkward and forced us to remove comments from the source code before compiling the BASIC routine. In the mid-1980's we still had to use 1-GHz Tektronix 7104 oscilloscopes with Polaroid cameras to record the fastest data, so we developed a semiautomatic trace follower to digitize the photos. An operator placed a computer cursor at the beginning of the trace on an image of the oscilloscope photo. The computer then followed the trace to its end, ignoring graticule lines, although the operator could interrupt if the computer lost track.

Using computers to process experimental data also evolved during my career. The available computers evolved from a mainframe machine programmed in machine language to a desktop computer that replaced slide rules, calculators, graphic artists and secretaries. The advent in the 1960's of the scientific programming language FORTRAN was an enormous advance, but programs were still executed on large mainframe computers with punched-deck inputs. I wrote or revised some programs, but we hired data-entry specialists to punch the cards and print the outputs. By the time I joined MRC multi-processing mainframe computers were in vogue, so the MRC/San Diego office used a Tektronix terminal to connect to the corporation's VAX computer in Santa Barbara. By the 1980's desktop computers were prevalent and BASIC became my favorite computer language, first IBM BASIC, then Microsoft BASIC and finally Microsoft Visual BASIC. BASIC seemed easier to debug and eventually acquired most of the capabilities of FORTRAN. We defined standard formats for experimental data and gradually added subroutines for data manipulation as we needed them, e.g., scaling, combining, correcting, Fourier transforming, etc. When I finished consulting in 2009 I prepared a CD with the latest version of my programs, together with their description, and provided copies to some of my associates.

During the 1995 Army High-Power Microwave program many data were recorded in FM format on magnetic tape. I programmed computer software to digitize the tape data during playback at reduced speed, using National Instruments NIDAQ subroutines. Eventually multichannel digitizing oscilloscopes came into routine use, but different models produced different output file formats, so I gradually accumulated a library of subroutines to convert each format to my standard data structure for further processing.

Word processing, spreadsheet and scientific plotting programs also became useful work tools, enabling me to prepare technical reports without the aid of graphic artists or secretaries.. Spreadsheets were particularly useful for corporate administrative tasks, such as time sheets, billing and classified material control. I think that MRC paid a high price for specially developed software; I avoided the cost and unreliability of custom software by adapting the tasks to the capabilities of commercial spreadsheets.

Much of my productivity as a consultant during the last 18 years was provided by my data processing codes. I gradually improved them by adding new capabilities as needed. They started with simple manipulations of time-history records, such as scaling the amplitude, shifting the time scale, combining two or more records and displaying or printing the results. Then we added frequency analysis, such as timing of zero crossings and Fourier Transforms. Similar operations were performed on the frequency transforms. Analysis of missile seeker experiments required a new set of manipulations, such as deriving the pitch and yaw components of a command by resolving the sigma-dot signal with a reference signal. Simple codes to calculate the effects of commands on missile fly-out trajectories followed. Focal plane array tests produced snapshot images of the array's output before and after exposures. The images had to be compared and changes interpreted.

Planning and executing tests to check equipment performance is an important part of all system development. Realism is sought, but not achievable at reasonable cost for many nuclear effects. Consider testing the effects on a spacecraft of the X-ray pulse from a nuclear explosion. The exposure must be in a vacuum chamber, but how good a vacuum and how large a chamber are necessary? Should the spacecraft also be bathed in the natural trapped radiation? How about simulating solar radiation and the target for earth-pointing sensors? Which of the variety of specified X-ray spectra should be used? In which of the huge number of possible states should the electronic equipment be when the pulse is delivered? Answering those questions and others in preparation for a test is extremely difficult. In my opinion we must answer them by considering the character of the design, together with the tests and analyses performed during development, and asking, "What are the uncertainties that could jeopardize the system meeting its requirements?" This is not a popular approach. As a rebuttal some of my colleagues invoked unknown-unknowns, which obviously cannot be anticipated by my approach. My answer is that system test experience reveals mostly unexpected manifestations of known effects. In many cases the surprises are due to the uniqueness of the test environment as it differs from the operational scenarios for which the equipment was designed.

It is even more difficult to design suitable system tests for highly political programs. I heard an Air Force colonel, the manager for the Brilliant Pebbles missile defense program, announce forcefully that, "There will be no failures in our flight tests". My unspoken comment was, "Therefore, you will learn nothing." A test designed to preclude failure must be designed to stress only that which is known assuredly, rather than those features that are most uncertain. Even though I didn't voice my opinion, the TRW project manager probably sensed my attitude and soon thereafter demanded that MRC replace me as our subcontract principal investigator.

A prime lesson I learned at Cal Tech, and tried to share with my colleagues, was to continually ask yourself whether your understanding of something makes sense. Is it consistent with all you know, or are there hints of contradictions? Be especially critical of experimental results that confirm your favorite theory! Don't brush under the rug the stray fact that casts doubt. During my early days at GA I developed a simple theory and some preliminary experiments appeared to be completely in agreement with it. Ecstatically, I submitted an abstract for the next meeting of the American Physical Society. Then I performed additional experiments to refine and extend the results. The agreement between theory and experiment completely fell apart. Something was changing and I couldn't even reproduce the initial results. It took months of hard work to figure out what was happening; meanwhile I had to withdraw the abstract. It is tempting, but potential dangerous, to accept uncritically conclusions you like, whether in physics, engineering, politics or life.

Scientists and engineers also must ask each other the same question, "Does it make sense". I remember how refreshing it was during my first days at GA when someone challenged one of my comments. I had not realized that I had become an unquestioned expert at my Army assignment, and that I was becoming careless by jumping to conclusions. I remember attending a technical conference in Albuquerque and after one of the talks asking the speaker a question that could have exposed a flaw in his conclusions. He gave a satisfactory answer, and I assumed the issue was closed. Another attendee, a scientist working at the Air Force Research Laboratory, commented later to a friend of mine that it had been rude of me to ask such a question. I was shocked to hear about his comment. I felt that the speaker had strengthened his position by providing a satisfactory answer. If he had not, it would have been important for the audience to understand the weakness of his conclusions. Such questions are essential in technical dialog.

Figure 23. Canada cruise, May 2010

I'm used to speaking frankly and as accurately as I can to my respected colleagues, and most of them usually respond in kind, but this habit sometimes causes me serious trouble. Some others infer that if I said, "#$*&@" I must really mean, "#$*&@!!!!" and take offense. I still have a lot to learn about diplomacy, but I doubt that I'll get very far in that direction.