From the Farm to Pioneering with Digital Control Computers: An Autobiography

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This is a personal memoir of my involvement from the beginnings of digital computers through the design of the Polaris missile guidance system to the Apollo Guidance Computer that put men on the Moon.

I was seven years old and destined to become a farmer when my father died. His death initiated the first major change in the direction my life was to take.

My father had established himself as a successful farmer with a farm on the banks of the Snake River in eastern Oregon. It was close to heaven, with hills to climb and explore, a river for fishing, a field for hunting, and a friendly shepherd dog for a companion. Exciting farm machinery included a rather archaic steam-driven tractor and a modern tractor with an internal combustion engine (“oil pull”). A paperweight model of an oil pull tractor rests on my fireplace mantel. The tractor dealer gave me this model while my father negotiated the procurement of an oil pull tractor, his first new tractor.

Dad was training me well as a future farmer. I drove his tractors, under his supervision, of course. He also demonstrated for me what it was like to drive at the breakneck speed of 50 miles an hour in an open Model T Ford. Naturally, he did not suggest that I drive that fast. We went fishing in the mountains of eastern Oregon, slept outdoors, and ate food cooked over an open fire, while my mother, safely residing at home, worried about the poisonous snakes that might bite us. I would have followed in my father’s footsteps, but his death drastically changed the future direction of my life.

Since my mother was unable to manage the farm, she moved with my two sisters and me back across the Snake River to Payette, Idaho, where I was born and had lived for the first few years of my life. My mother faced difficult times, with no close relatives to provide support. Her mother and father had died a few years earlier, and her only sister lived on a farm in northern Idaho. The state threatened to put my sisters and me in an orphanage. Facing that pressure, my mother remarried and bought a small place in Payette with one acre of land. That action established the pattern of my life for the next 10 years.

Elementary School

Soon I recognized my future depended on education. Farming was not to be my future, even though I still liked the farmer’s way of life. I could see the difficulty in becoming established as a farmer in the 1930s. It was quite clear that success depended on many uncontrollable factors, such as weather, market conditions, and good land.

As I progressed in school, physics, mathematics, chemistry, manual arts, and sports stimulated my interest in education. Most of the time, sports came first on the list. I struggled with history, English, literature, and the social sciences. Even though I was interested in science, high school never aroused an insatiable desire to learn. However, I realized education was a necessary step in the path to a more-interesting future than one in farming. I wanted to go to college.

We were poor. Therefore, while in high school, I started working summers at a vegetable farm run by a Japanese family. The money I earned bought my clothes, provided for miscellaneous school expenses, and provided a little for a teenager’s amusements.

I received little encouragement in my desire to go to college. My mother’s sister had gone to college and understood the cost. She suggested a trade school as more suitable for my educational purposes. My stepfather had little education. He thought I should quit school and go to work. My mother was my only supporter. She encouraged me with stories about my father’s nephew, a cousin four years older than me, who was a very successful student at the University of Washington.

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After high school graduation, I worked on the Japanese vegetable farm through the summer and fall, then went to Seattle where I could prepare to enter the University of Washington.

The Big City

I arrived at the home of my father's sister, who was the mother of my idolized cousin, on Saturday, 6 December 1941—the day before the bombing of Pearl Harbor. By now, this cousin was in his senior year at the University of Washington and was soon to graduate Phi Beta Kappa.

Seattle quickly assumed a state of war, with a total blackout every night, barrage balloons, and antiaircraft guns everywhere. My plans had to take shape quickly. I needed to find a place to live in Seattle and get a job, since I needed money to start college.

Seattle was a strange place for a bashful kid from the farms of Idaho with "hayseed in his hair" (a common phrase for hicks from the hills). Fortunately, it was easy to find a place to live, but not quite as easy to find a job. Strong unions controlled the job market, and even with the increasing demands for military production, only temporary positions were available. I held many odd jobs, mostly in the building trades. By summer 1942, I had a well-paying job working the graveyard shift (11:00 p.m. to 7:00 a.m.) in a shipyard building destroyers for the Navy.

During the summer, I continued working at the shipyard, registered for the summer session as a part-time student at the university, attended classes in the morning, did homework and slept in the afternoon, and worked at night. I intended to start full time in the fall.

One course I took that summer shaped my future far more than I expected at the time. The course was Navy ROTC. I liked the many technical assignments the Navy offered, but the ROTC course concentrated on ceremonial procedures (such as the number of guns fired to salute an admiral when he boarded a ship). I was turned off by these ceremonial procedures.

University and U.S. Army

My enthusiasm built as I started university classes full time. The curriculum for physics majors was exciting. There were many courses in physics, mathematics, and chemistry, but very few required courses in the liberal arts. I had a roommate, a senior in economics, who took this bashful country lad under his wing and provided guidance. However, physics, mathematics, and chemistry were outside his realm of understanding. Academically, I was on my own.

He was in the Army ROTC program and joined the Army Reserves. I followed suit. We were told Reserve personnel would stay in school until they finished their degrees. That guarantee lasted two terms, until March 1943, when all Reserves were called into active duty. My roommate was just one term away from graduation.

After the introductory procedures at Fort Lewis, Washington, I went to Army Air Force basic training and my roommate to Army officer training. It was many years after World War II before we met again. He ran a private telephone business in southern Washington State, and I was at the Massachusetts Institute of Technology's Instrumentation Laboratory (MIT/IL) in Cambridge.

My Army Air Force basic training was five weeks of calisthenics and drill instruction on the beaches and boardwalk of Atlantic City, New Jersey. The closest I got to the rigors of the war was one short march to the rifle range for firing practice.

My next move was to City College in New York City. My days were filled with lectures, testing, military instruction, calisthenics, and some free time to tour the Big Apple.

Eventually we learned that we were waiting for a new Army program to commence at colleges and universities: the Army Specialized Training Program. I was sent to Rutgers University in New Jersey and started the next term to study electrical engineering—an 18-month concentrated program including a broad range of technical subjects in civil, mechanical, and electrical engineering (electrical power machinery, telephony, electromagnetic theory, servomechanisms, vacuum tube circuit theory, etc.) and a few liberal arts courses. Since this was the Army, military and physical training formed part of the daily routine.

Classes filled six days of the week. Study consumed the evenings. Those who were not motivated to study rested on Sunday. In contrast to normal college life, there was little time for socializing.

About 200 men started the course in July 1943. Periodically, those with low academic standing were returned to active military duty, a procedure that provided an incentive for those remaining. After 18 months (January 1944), only 65 of the original group were left in the graduating class. The four students with the highest grade point averages went to a very secretive project someplace in New Mexico (the Manhattan Project). As fifth in the class standings, I missed that assignment and was transferred with the remainder to the Army Signal Corp in Camp Crowder, Missouri.

We repeated basic training and continued school, learning radio repair. By late spring, I and about half of the Rutgers class moved to Fort Monmouth, New Jersey, for a classified course in high-frequency radio and antenna theory. The course proved very interesting. Fort Monmouth, known as the playground of the Army, was the place to spend the summer. Again, I missed a move to the Manhattan Project. In our absence, the balance of the Rutgers class went to New Mexico.

After a few more months, the war was over, and we returned to Camp Crowder to await discharge (March 1946).

Civilian Life and Return to College

After discharge from the Army, I faced a major decision. What should I do next? The University of Washington would not accept courses I took at Rutgers as credit toward a degree in physics. Admissions apparently did not recognize the Army Specialized Training Program courses as college-level work. I did not want to return to my 1943 status as a freshman, so I considered colleges in the East. I had a longtime interest in graduate school at Harvard University or MIT. A small college in Quincy, Massachusetts, Eastern Nazarene College, offered me credit for applicable courses at the University of Washington and Rutgers. This made it possible for me to finish a bachelor's degree in mathematics in two years with enough time the second year to complete a master's degree in physics at Boston University.

My master's thesis was an interesting subject that led directly to my first professional job. A small company, Transducer Corp., sponsored the thesis. Transducer had an Air Force contract to develop an airborne radar simulator for training pilots to operate the APS-10 radar, a radar that sensed and displayed the terrain over which the pilot was flying. The trainer applied an ultrasonic transducer to generate sound transmission in water, which would simulate the radar's antenna pattern. The first step in the antenna design was to analyze the behavior of high-energy ultrasonic waves in water, the subject of my thesis.

After graduation, I had a ready-made job to continue the antenna design. This task drew on the electronic circuit design and electromagnetic antenna theory from the courses at Rutgers as well as the physics of sound transmission and optics from the courses at Boston University.

I was still interested in the pursuit of a PhD in physics, so I took a few courses at MIT and eventually applied to Harvard's Graduate School of Arts and Sciences. Harvard accepted me as a graduate student in the Physics Department in spite of my incoherent undergraduate and graduate school education.

I had to continue working part time while attending Harvard. I was married in 1948 following my graduations from Eastern Nazarene College and Boston University. My wonderful wife held a teaching fellowship, was completing her courses, and was working on her dissertation for a PhD in physics at Boston University. Our first child was born in February 1950. We bought a house. Life was busy and full.

At Harvard, I studied many branches of physics (including electromagnetic theory, quantum mechanics, and low-temperature physics) under famous professors, three of whom were or would become Nobel Laureates (Edward Purcell in 1952, Julian Schwinger in 1965, and Norman Ramsey in 1989). For me, Schwinger would top a list of professors as the best lecturer.

After two years, I was accepted into the doctoral program at Harvard and was offered a teaching fellowship. However, the doctoral program would have taken three to four more years. My wife and I now had two children, and I was tired of school.

An alternative arose. A flyer on the Physics Department's bulletin board advertised an opening at MIT/IL. The lab was advertising for an individual interested in random processes in automatic controls. I had just completed the only courses that were even remotely related to my future at the laboratory: two courses in random processes and Schwinger's course in quantum mechanics.

I could not resist the temptation and applied for the position. My interview was a memorable event, conducted by three PhD mathematicians from MIT. I was unnerved by the interview, but was offered the position. It sounded interesting, so I decided to discontinue my quest for a PhD.

Thirty-Five Years with MIT/IL and Its Successor

After joining MIT/IL in 1952, I started work in the mathematics group. The group supported the laboratory's projects and maintained the computational facilities, which included several different types of analog computers. MIT/IL added a digital computer when the IBM 650 became available in the mid 1950s. During the next two years, I designed equipment and techniques to simulate and analyze random processes in control systems. Unbeknown to me, the senior mathematicians had lost interest in the application of analog computers for analyzing random processes. They were switching to digital techniques. My education had not even mentioned digital computers. Switching circuits, binary arithmetic, and Boolean algebra were foreign to the vocabulary of physicists.

Digital Computers

My introduction to digital computing came with a demonstration at MIT's Whirlwind computer facility. Whirlwind was computing the antenna pattern for a dipole antenna and displaying the result on an oscilloscope. With my experience, I could understand the computer's computational activity.

I learned this monstrous marvel (an electronic digital computer that filled most of the Barta building) was available free of charge to researchers with complicated computational tasks. My wife, while working on her PhD dissertation, had such a problem. She wanted to compute the eigenvalues of matrices. Whirlwind offered relief from performing the tedious operations with a mechanical desk calculator. My wife submitted the elements of a matrix to an individual at the Barta building. He reappeared a few days later with the results, truly a marvel when compared with the hours of labor using the mechanical calculator.

My next experience was at the meeting of the Association for Computing Machinery at the University of Michigan in 1954. I presented a paper on analog computing techniques[1] and listened to papers expounding the wonders of digital computing. This was the first time I had attended a professional meeting and the first paper of my professional career. I had never taken a speech class or made a public address. Most of the attendees were interested in digital computing, so there was little interest in my paper. My knowledge of digital computing technology was limited, hence most papers were beyond my understanding.

As part of the proceedings, the university demonstrated its digital computer. It could play pool. An operator positioned the cue stick. The movements of the stick and balls were displayed on an oscilloscope. It was an impressive demonstration. My knowledge of physics enabled me to understand the complex computations the computer performed.

However, the usefulness of digital computers in MIT/IL's inertial guidance systems was questionable. Their value became apparent when my bosses (J. Halcombe Laning, Jr., and Richard Battin) faced a missile-guidance problem. They had developed a guidance scheme for ballistic missiles.[2] The laboratory used this guidance scheme in a simple inertial guidance system for the U.S. Air Force's Thor missile. Thor was a medium-range ballistic missile, and analog computation was satisfactory, but the Air Force's intercontinental ballistic missiles needed digital computation to realize the required accuracy.

There were many hurdles to overcome. Digital computers filled rooms, were complex, were prone to failure, and were incompatible with inertial measurement instruments. Inertial instruments were analog, therefore an analog computer was a natural marriage. In contrast, data had to be converted back and forth between digital and analog before the instruments and computer could communicate.

The lab had no experience with digital control systems. The challenge provoked my interest. I began taking courses at MIT (switching-circuit theory, sample data systems, and information theory) and began experimenting with digital technology (transistor logic circuits, magnetic core memories, and techniques for converting data between digital and analog equipment). A few engineers joined me.

I suggested pulse torquing a dc motor to convert digital to analog by applying a fixed amplitude of current for a length of time controlled by the computer. To test the idea, I engaged a young graduate from MIT with gyro test experience. He designed the equipment and ran the tests.[3] Pulse torquing could accurately convert digital to analog. MIT/IL staffers extensively applied this idea in the lab's future inertial systems.

An associate designed an optical shaft angle encoder applicable to the acceleration-measuring instruments of an inertial system. I designed logic to drive a two-phase ac motor as a stepping motor. With these techniques, the data conversion between the digital computer and analog instruments in an inertial guidance system looked feasible.

To complete the bag of tricks, I recognized a match between a special computer architecture, Digital Differential Analyzer, and the differential equations in the guidance scheme Laning and Battin developed. The match was made by converting the differential equations to difference equations. A Digital Differential Analyzer adequate to make the computations called for by the difference equations was simple (only 12–17 bit words of memory) and small enough for a ballistic missile's guidance system.

Polaris

The first real chance to apply the ideas came in 1956. The Navy was planning the Polaris system, a submarine-launched ballistic missile, and called on the MIT/IL to develop the missile's inertial guidance system. The Polaris missile was small, so its guidance system had to be much smaller than any of its predecessors.

I inherited the task of selling the proposed digital computer[4] to the Navy. The Polaris missile's guidance computer had to be small, rugged, reliable, and ready in time to meet a very tight production schedule. A debate ensued within the Navy. Should the guidance computer be analog or digital? Important to the debate between analog and digital computing was computational accuracy. At MIT/IL, there was no debate, we were on the side supporting digital computation. The Navy realized that accurate computation would be required, therefore it chose digital. Since the Navy managers could not accept the potential for failure if the digital guidance computer did not meet its goals, they decided to develop an analog computer as a backup. My proposed digital computer had competition.

Since MIT's IL was a research and development organization, the Navy established an industrial support contractor responsible for production. This contractor's engineers joined MIT/IL's staff and expanded the design effort while providing the necessary production engineering.

With the basic computer characteristics defined, I assumed more system-level obligations: specify the missile and fire control interfaces, monitor the computer's electrical and mechanical design, and coordinate the industrial support contractor's efforts.

As my responsibilities expanded, I was promoted to group leader and organized a new MIT/IL group: the Digital Development Group. This new group expanded rapidly. I began to watch over the big picture.

Schedules were important. The Polaris program was complex with many contractors, and the Navy's program managers needed a method to monitor the contractors' schedules. They introduced a new program management technique, Program Evaluation and Review Technique (PERT), and imposed it on all contractors. Engineers at the lowest level would plan each task and estimate the time required to complete it. A computer program integrated all inputs and printed a diagram that provided an easily interpreted progress report for every phase of the program. At MIT/IL, most of us took the position that garbage in yields garbage out. However, we did our best with planning and scheduling. The result was probably helpful at the higher levels of management. At the engineering level, PERT seemed to consume time that could be better spent performing the task.

The project was under extreme pressure. The United States was in a cold war with the Soviet Union, and the Soviet Union had demonstrated superior missile technologies. Navy Captain Frank Harold, chief of the Navy's SP23 branch that was responsible for the Polaris system's fire control and missile guidance, took me to the shipyard where the first Polaris submarine was under construction. He made his point: The Navy was serious. This short visit to the shipyard was far more convincing of the need to meet schedules than the studying of PERT charts. It was clear the guidance computer had to meet its design goals and schedule.

The computer as proposed was feasible and small enough, but could it meet all mission requirements? Would it operate properly for the few minutes of the missile's flight? Could it be produced, maintained, and stored on submarines for the years required of an operational system? Many questions needed answers.

To test operation in flight, I initiated a plan for and designed digital equipment to fly in the first few missile test flights.[5] Results from these tests validated the computer's operational capabilities but highlighted a potential problem with the missile's power supplies. There were voltage transients that exceeded the specification and induced failures in the computer. This problem had to be resolved before the first guided flight.

The complexity of digital computers (the large numbers of electronic components and interconnections) was a factor in their inclination to fail frequently. Fortunately, the Polaris computer's design was quite simple, which was a step in the right direction. To realize this advantage, I paid special attention to the components and interconnection technology.

A related issue was a requirement for long-term production of the computer's components. Polaris guidance systems would be in production and service in the fleet for many years. During this life cycle, the computer's components must be procurable from a stable source of supply. However, the semiconductor industry was immature and changed product lines frequently. There were no stable sources for transistors.

To develop and maintain a stable source for transistors, production quantities had to be large enough to be profitable for the semiconductor industry and the transistor's characteristics defined so that the device could be reproduced, that is, maintain process control.

As a step toward solution of the problem, I had selected a single transistor for the guidance computer's logic circuit and prepared the transistor's specifications[6] such that the electrical characteristics would be tenaciously controlled. The Navy imposed the same transistor on the fire control's computer, thus standardizing on one product line and maximizing the production quantities. With the transistor specification as a baseline, the Navy prepared a Naval Ordnance-type specification, established a captive line for procurement, provided acceptance testing, and supplied the transistors to the computer and fire control production lines. The procedure was successful.

By December 1959, the guidance system was ready, and Polaris flew its first guided flight on 7 January 1960: This was a first for an inertial guidance system with a digital computer.

Long before the first guided flight, I started exploring design approaches that would reduce the size, improve interconnection reliability, and improve maintainability. The Navy also became interested. Schedule constraints limited the investigations into alternate approaches for the original design, but as time became available, the Navy welcomed the chance to reduce the size.

A second-generation design repeated the architecture, replaced germanium-junction transistors with silicon-planar transistors, and replaced printed circuit boards with a modular construction employing welded-cordwood modules and wire-wrap interconnections between module connectors.[7] (See Figure 1.)

Figure 1. Polaris guidance system's electronics; computer on top.

The effort to reduce the size of the guidance computer opened the door to developments taking place in the semiconductor industry. During my 1959 visit to the Polaris program's captive line at Texas Instruments, Jack Kilby, the inventor of integrated circuits, demonstrated his experimental circuit, which Texas Instruments announced that spring. Later in the year, Kilby came to MIT/IL, discussed his work in greater detail, and made a proposal to design a logic circuit that would replace the NOR gate in the Polaris computer. My interest was sparked, and the Navy was willing to fund an order for 64 logic gates at $1,000 apiece.

Later, I visited Fairchild Semiconductor. While there, I discussed Fairchild's work on semiconductor circuits with Robert Noyce and his engineers. Noyce, the founder of Fairchild Semiconductor, has been considered the coinventor of integrated circuits. Under his guidance, Fairchild developed production techniques that were necessary to reduce the integrated-circuit idea to practice and produce the first commercially available product in 1961. It was evident that semiconductor circuits were feasible.

Apollo

When MIT/IL received its contract for the Apollo guidance system in 1961, I was ready. The Polaris program was near its end. Operational guidance systems with first-generation computers were in production. Prototype versions of second-generation computers were being produced for evaluation and flight tests. I had a sizable group of designers looking for a new challenge. The Apollo contract came at the right time.

With the success of the Polaris program behind us, we were ready. Also, my group had developed and tested magnetic core transistor logic circuits that seemed ideal for Apollo. Even though functional requirements for the Apollo guidance system's computer were unknown, we knew the computer must operate in a small spacecraft, therefore, the computer's computational characteristics[8] were constrained to minimize size, weight, and power consumption. Therefore, we proposed a computer designed with magnetic core transistor logic circuits, a small coincident-current-core memory (RAM), and a larger magnetic core memory (ROM). Employing this technology, we could design a small low-power computer with acceptable computational characteristics. The computer's design, scheduling, and program planning progressed with core transistor logic circuits for the first year.

Early in the program, I became heavily involved in determining the computer's operational functions and physical characteristics. The size, weight, shape, and power consumption had to be estimated so that spacecraft design could proceed. It took lots of traveling and many meetings with the spacecraft contractors, NASA engineers, and astronauts to determine the computer's operational functions and define the electrical interfaces with the guidance system and spacecraft. But, the computational requirements continued to be

Figure 2. Eldon Hall and an AGC.

elusive as mission planning and software development progressed.

Very soon it became quite clear to me that the core transistor computer was not a suitable approach for at least two reasons. Its computational speed was limited. Fabrication was difficult, implying the computer would be costly and probably less reliable.

Fortunately, hardware technology (e.g., semiconductors, memories, and multilayer boards) was developed and became available at just the right time. Kilby and Noyce's improvements in semiconductor technology contributed to our progress. Semiconductor integrated circuits designed as computer logic elements became available in the early 1960s.

I felt there was sufficient time to incorporate these new devices into the Apollo computer's design if we moved rapidly, so we bought significant quantities and ran evaluations to demonstrate their advantages. A design change would be dramatic. I became the salesman to convince everybody, including NASA program management, that a change to integrated-circuit logic was feasible and was more desirable than core transistor logic circuits. Fortunately, in December 1962, NASA quickly granted approval to proceed.

There were two very different design challenges posed by the application of a digital computer in the Apollo spacecraft. Both were the result of human involvement. Computers back then were not user-friendly, much less so than today. Imagine the problem in the 1960s for astronauts in space. We had to develop a solution.[9] Reliability was a serious issue: For the first time, an erroneous operation of a digital computer could jeopardize human lives. Designing a computer that astronauts in space could operate was primarily a software problem. Reliable operation was a hardware issue. In those days, computers were not considered reliable, therefore a lot of effort was consumed on both issues.

The Apollo computer would have to operate without error for many days, in contrast to the few minutes required while guiding a ballistic missile. Achieving this reliability was a two-pronged problem. The first issue was technical: how to make the product operate reliably. The second was a managerial issue: how to convince program managers of its reliability. In the final analysis, only successful missions proved the product.

Integrated circuits were a typical example of computer components that needed considerable effort to ensure reliable products.[10] Integrated circuits were used in large numbers, and the production sources were immature. Many in the NASA technical community questioned the wisdom of their application as the logic device in the Apollo Guidance Computer (AGC). (See Figure 2.) NASA personnel knew the risk was high. So, the questionable reliability of integrated circuits fell into both categories of the reliability problem.

I spent much of my time in meetings. There were MIT/IL program planning and design reviews, meetings with NASA's and the spacecraft contractor's engineers, and meetings with technical review teams that NASA organized to discover problems.

I spent hours, days, and weeks conducting the MIT/IL design reviews. These review meetings included MIT/IL designers (both mechanical and electrical), representatives from the industrial support contractor (Raytheon), and representatives from NASA. Some meetings included an astronaut. I went over every detail of the computer's documentation: manufacturing drawings, computer acceptance tests, component procurement procedures, design analyses, etc. For the computer's display unit (the astronaut's interface with the computer), astronaut David Scott reviewed and approved the top assembly drawings to signify the astronauts' acceptance.

Conducting these reviews was a valuable management tool for me. They provided complete visibility into every element of the computer's design, its development schedule, and production activities. Also, the meetings provided contact with all the people involved in the program, from the astronauts who would be flying with the computers to the production workers. It was a task I enjoyed, since it included both technical and managerial responsibilities.

We were in a design review meeting the day President Kennedy was assassinated. That was a dark day, burned into my memory.

PERT raised its ugly head again. NASA program managers needed PERT to monitor all facets of the program, just as the Navy did in the Polaris program. It did not seem to be as much of a nuisance in the Apollo program. Maybe I had become adjusted to its procedures.

The technical meetings with NASA and spacecraft engineers were challenging and interesting. NASA imposed the requirement for common hardware and operating procedures in the Lunar Module and Command Module systems. This requirement complicated the task of defining the computer's functions and associated interfaces necessary to communicate with other spacecraft systems. Different software in each spacecraft would accommodate functional differences, but the computer's hardware interfaces had to be identical.

Another time-consuming activity for me was to respond to NASA-initiated technical review teams. NASA engaged many teams to expose potential problems in the Apollo program. These groups focused on mission success and crew safety. Thus, the guidance computer was an easy target and came under intensive fire.

Any statistician could prove the guidance computer fell short of the requirements for mission and crew safety. Experience with digital computers and the unknowns, added by the introduction of integrated circuits into the design, fueled the fires. The individuals who made up these review teams were like wolves nipping at our heels. Their motivation did not seem to be pure. At times, their actions seemed to be attempts to bite off a piece of the action.

Responding to the attacks and maintaining an organized design effort required constant vigilance on my part. On a positive note, however, these teams required us to pay attention to every detail, thus contributing—in a devious fashion—to the success of the project. Attacks from these outside experts subsided as computers came off the production line and were operating beyond expectations.

Now a different type of problem arose for me that was more managerial than technical. The design task was phasing down. Many engineers were seeking a new challenge. There were ongoing evaluation tests and problems arising with software and system integration. The only new subject was investigation into computer fault-tolerant techniques. Everybody was still uncertain about the ability of the computer to operate without failure during a mission. However, these tasks were not sufficient to support the group of people that I had during the peak of the design effort. This phasedown was only the tip of the iceberg looming on the horizon.

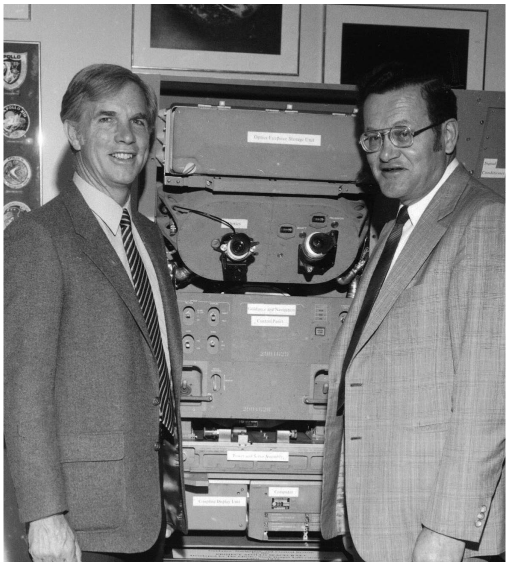

Figure 3. Eldon Hall in 1982 with astronaut David Scott.

On 20 July 1969, two computers designed and programmed at MIT/IL and manufactured at Raytheon were in orbit around the Moon. One was ready to guide the *Lunar Module 7* to a landing. We met the goal President Kennedy had set for the program in 1961.

Over 10 years later, the AGC had found its place in history, having flown every Apollo mission successfully. The computer was to take its place at the Boston Computer Museum. Astronaut Scott and I presented papers[11] during the museum's inaugural celebration. (See Figure 3.)

Leaner Years

Lean years set in as the AGC's design and mission support phased down. In the early years of the Apollo program, I had given up the responsibility for computer design on the Navy's Polaris program. With MIT/IL's continuing efforts on the Navy program, some of my group transferred, but opportunities were limited. I was attempting to hold together a remnant of my group of designers. With very little support for hardware design, I faced laying off staff.

Compounding the problem was the fact that MIT/IL was going through a transition. MIT's students and faculty were protesting the Vietnam War and MIT's role in the development of military weapons. MIT/IL was a prime target, since it was the largest on-campus laboratory supporting military projects. These events led to MIT/IL's divestiture from MIT in 1973. The lab was renamed the Charles Stark Draper Laboratory, Inc., and was established as an inde-

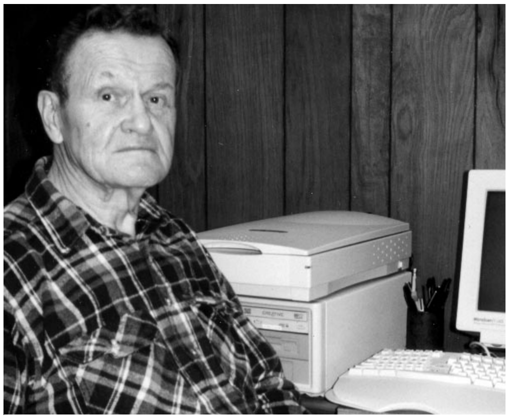

Figure 4. Eldon Hall in 1999.

pendent nonprofit research organization. To function independently of MIT required significant changes in the lab's operating procedures and methods of funding.

NASA's interests at the lab shifted to paper studies at a much lower level than the support my group received during the Apollo program. There were planning activities for NASA's space shuttle program, a few tasks to complete the Apollo program,[12] and a NASA-funded effort to design a fault-tolerant computer that led into studies of fault-tolerant computer configurations for the shuttle's avionics.

Studies and design of fault-tolerant computer configurations continued for years, with a few contracts from commercial organizations. One of considerable interest was an application of microprocessors for electronic engine control.

Various studies occupied my time, including a long list of computer-related technology: fault-tolerant computer configurations, the impact of lightning on aircraft and missile systems, nuclear hardening for the space shuttle electronics, applications of a microprocessor to a U.S. Army Teletype Writer, evaluation of the Army's proposed Military Computer Family, evaluation of memory technology for the Navy,[13] evaluation of the Viking spacecraft's guidance computer, and evaluation of the shuttle's engine control computer. In many of these studies, I became the outside expert critiquing the activities of the designers.

Of these studies, I found the Army Teletype Writer the most interesting. In 1976, we were awarded a small contract to add intelligence to a standard Army Teletype. Electronics with a Motorola 6800 microprocessor were added for control. Semiconductor RAM and ROM provided memory for program and data storage. The finished device had a few word-processing features similar to modern electronic typewriters and could store, print, and send messages to matching equipment at remote sites.

Retirement

Near the end of my professional experience at the Draper lab, I got involved in an effort to bring transmission line theory into the design of computer interconnection technology. It was a return to my educational and professional roots of 40 years earlier. More-modern computer engineers had little educational background in and even less interest in transmission line theory, but as the speed of integrated circuits increased, the interconnections between these circuits functioned in a way similar to transmission lines. I could bring my knowledge to bear on this modern problem.

After I retired, this study of interconnection technology[14] continued, and I became a consultant. The effort provided a phasedown from my professional life and made a welcome transition into retirement.

Next I decided to write my recollection of the Draper lab's history in digital computing and development of the AGC. Since the Apollo lunar landing missions were the first time human lives were intrinsically interlocked with the proper operation of a digital computer's hardware and software, a history of the AGC's development seemed important to me. My awareness that many books and news media reports of this history were significantly different from my memory of the events provided additional motivation for this effort.

A prominent example of these differences is the way the media reports the occurrence of computer restarts, an indication of computer failures, during the Apollo 11 landing. According to the media, the computer was failing, and the astronauts took over to land the spacecraft manually. The correct explanation is complicated, but the fact is the computer was functioning just as designed and continued to control the spacecraft to touchdown. Just before touchdown, the astronaut sent steering commands to the computer to move the touchdown point away from a field of boulders.

I can cite another example from the histories of semiconductor developments and applications in the mid 1950s and 1960s. Most histories imply that the electronics industry enthusiastically welcomed transistor and integrated-circuit developments. However, the reverse is more historically correct. It took government-sponsored programs like Polaris and Apollo to provide the semiconductor industry with support and motivation.

My role in the Polaris and Apollo projects provided me with the visibility and memories necessary to write this history. Fortunately, the lab maintained extensive archives of reports and copies of viewgraphs that we had used as presentation materials. With these archives and the files I saved, I augmented my memories. However, my biggest problem while writing my book was organizing all the material to make it readable. A word processor running on what could be called a descendent of the Apollo computers made the effort possible. I could sit at my desk with a computer at my fingertips that was at least 1,000 times more powerful and user-friendly than the Apollo computer and write, assemble, and rearrange the material until the message was readable. (See Figure 4.)

After six years of research and writing and many publishers' rejections, the American Institute of Aeronautics and Astronautics accepted my manuscript for publication.[15]

In 1998, my wife, Grace, and I celebrated our 50th anniversary with our four children and seven grandchildren. Grace, after assisting our children with science projects and teaching physics as an adjunct professor while they grew up, pursued her first academic interest, literature. She earned an MA in English and wrote a critique of Shakespeare's *The Tempest.* Her book, *The Tempest as Mystery Play,* was published in 1999. As part of my retirement activities, I have returned to farming, gardening, and yard work. I have no regrets for having left farming when I was young. The farmer's life has many advantages, but my first academic love is still physics. Farming could not provide the excitement of my scientific endeavors and certainly could not have matched the marvel of the Apollo lunar landing project.

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