From Vacuum Tubes to Very Large Scale Integration: A Personal Memoir

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This article traces one man's journey through the various different forms of electronics and how they were developed and used at IBM. It also illuminates some of the reasons why particular decisions about various technologies were taken and the longer term results of those actions. It started as a personal memoir that was intended, by the former editor of our Biographies department (Eric Weiss) to be part of his usual submission. The article was, however, not finished until after Eric had retired from that position, so it is being used as a feature article instead.

Introduction

Every individual should be fortunate enough to have a mentor. My first mentor was Curt Segler, my scoutmaster. He recognized that I had a natural bent for working with my hands and suggested, because he felt I was college material, that I go to an academic high school rather than a technical school. I am quite certain that this suggestion changed my life.

Early Life

My father, a yardmaster for the Reading Railroad, died in 1935 when I was 15, so my mother and I moved from Philadelphia to Brooklyn, New York, to live with my sister, Eleanor, and her husband, Anthony Mazzan, and my older brother, Earl. Having been in the Boy Scouts in Philadelphia, I became assistant scoutmaster to Segler. As part of my scouting work, I built a demonstration of mineral fluorescence and a model steam engine that drove an electric generator. I wanted to become an electrical power engineer. I cannot define when or how I got this urge, since my parents were not college graduates. At this point, my hobbies tended to electronics, because it was too difficult for me to work with or build generators. I built radio receivers, crystal detectors, and one- and two-tube sets. At Erasmus Hall High School in Brooklyn, I found math, physics, and chemistry so exciting that I did not have to study to get good marks, which was not the case in English, history, and, to a lesser extent, geography.

Cornell University

I entered Cornell University in 1940, planning to become an electrical power engineer. By the end of my freshman year, I had turned to electronics. In each of the following three years, I added three hours per semester in physics and math to my prescribed courses. I now see that I was constructing for myself a course in engineering physics, not then a recognized course of study at Cornell University.

I enjoyed both gymnastics and weight lifting and became quite strong, although I weighed only 165 pounds. To force me to use speed instead of brute strength, the coach of the wrestling team matched me against a 235-pound senior on the heavyweight team. I got him in a scissors grip low around his waist. He lasted for about three minutes. When he gave up, he stood up and then immediately keeled over in a dead faint. The angry wrestling coach told me to give up either weight lifting or wrestling. I gave up wrestling.

Slaughterhouse Summer

During the summer of 1941, my brother-in-law got me a job as a maintenance worker in a cattle slaughterhouse at 40th Street and 11th Avenue in New York City, located next to the construction of the Lincoln Tunnel. Shortly after I joined the company, the meat cutters, but not the maintenance workers, went on strike. That evening at dinner, my brother-in-law, who was a manager in the slaughterhouse, told me the plant superintendent was tending the boilers in the boiler room. The next day, I knocked on the back door of the engine room and was greeted by the plant superintendent. He asked me what I wanted, and I explained that I wanted to work. He then asked me if I was familiar with fires. My answer was, "Sure." After hearing that response, he led me to the boiler room, told me I was the fireman, and promptly left.

When I had said "Sure," I was thinking about campfires. Now, standing in the boiler room with the roar of a furnace next to me, I saw that I was wrong. It became quite obvious that I had to understand how the boilers worked in order to be able to ask intelligent questions. At the end of about an hour, I had a good theoretical knowledge of how the various gadgets in the boiler room worked, and I asked Larry, the chief engineer, to check me out on how to reignite the furnace if the flame went out, the location and operation of the boiler feed pump, and other details. He then explained how to do the job of a fireman.

Since I was very concerned about the disaster that would result if the water boiled out of the boiler and the water tubes burned out, I kept the water level close to the top of the water gauge, constantly adjusting the fuel oil valve to keep the steam pressure at 135 pounds per square inch. To do this during the first week, I was busier than a one-armed paperhanger. Then I realized that I could store more energy in the boiler if I kept the water level in the boiler lower, at one inch above the bottom of the water gauge rather than at the top. This additional energy kept the pressure fairly constant in spite of steam usage. At the end of my 12-hour shift, my pressure recording chart exactly tracked the 135-pound pressure line. I became bored. I went to Larry and got the additional job of engine room mechanic, which I did while at the same time tending the furnace room fires.

At the end of two weeks, I was given a raise to the full fireman's rate, which amounted to $1.00 plus one half cent per hour for an eight-hour day with a multiplier of 1.5 times for overtime and a two times multiplier on Sundays and holidays. My shift was from 8 AM until 8 PM for seven days and then 8 PM to 8 AM for the next seven days. Needless to say, one is not bored when working those hours.

In essence, he said that it was impossible for me to find the solution to a very serious problem in 15 or 20 minutes when two of his best electricians had not been able to solve the problem in more than six weeks.

Cornell as a Sophomore

After returning to Cornell for my sophomore year, one Saturday night I attended the roller skating rink on the campus. I saw a young lady skating with a fellow I did not know. The young lady had been a student at Erasmus Hall High School, but I had never gotten up the courage to meet her even though I was quite attracted to her. I cut in on the young man and introduced myself to Jeanne Neubecker. The fact that I had attended Erasmus enabled me to set up a future date with her. She was a freshman studying to become a veterinarian.

After many discussions over several years, I was able to convince her that being married to me would not prevent her from having a veterinary practice and raising a family. We were married on 31 March 1943. I graduated on 29 February 1944. (I can celebrate my graduation only once every four years.) Jeanne graduated in June 1944, and our son Raymond was born on 8 June 1944. Marilyn was born on 25 March 1950, in Long Island, New York, and Paul was born on 21 December 1954, in Poughkeepsie, New York. Jeanne is retired now from a large- and small-animal practice and has written two books about veterinary medicine.

York Safe and Lock

The following summer before starting my junior year, I got a job at the York Safe and Lock Company in York, Pennsylvania. Although I was hired to be an electrician, I was assigned to shoveling piles of coal around the coal yard. At noon, Bill Ayre, the boss, saved me and turned me over to Scotty, the foreman of the electrical maintenance shop. My new job was to fix electrical problems in the huge production machine tools as an electrician's helper.

When I got my paycheck at the end of the first week, I must have mumbled something under my breath, because Ayre asked me what was wrong. I said that it appeared that my rate of pay was 75 cents per hour. He asked me what rate of pay I had agreed to when I was hired. I told him that there was no agreement, that I thought I would be paid the going rate for an electrician. Ayre said: "I bet you will never take another job without getting an agreement on what your salary will be." I never did.

A short time later, Ayre described a problem they were having with the phone system between the guard headquarters and the eight guard towers. He suggested that I track down the two electricians who were working on the problem, talk with them, and get back to him with my recommendations. I found the electricians in cramped quarters and not at all cooperative about telling me their problem. About a week later, Ayre asked me why I had not reported back to him. I explained, and Ayre told me that the electricians had been working on the problem for six weeks and were now proposing that all the underground wiring for the guard phones be replaced. He emphasized that he wanted me to go back out there and find out what needed to be done.

I located the electricians in the guard headquarters office and got their attention by telling them that Ayre had sent me. They said the trouble was noise on the lines. They connected me with a guard tower and as I chitchatted with the guard, I heard the sound of frying bacon. I immediately concluded that the noise was caused by the carbon granule microphone. To confirm my suspicions, I found that the noise changed as I tipped my head left and right. I told the two electricians that the microphones in the guard phones should be replaced. They laughed until they were gasping for breath. I suggested that they replace the microphone element in the phone in the guard headquarters and do the same in a guard tower and then see if the noise disappeared. They refused to accept my suggestion. I then told them my only option was to report to Ayre.

I explained to Ayre the cause of the problem and how to fix it. He did not take my explanation at all well. In essence, he said that it was impossible for me to find the solution to a very serious problem in 15 or 20 minutes when two of his best electricians had not been able to solve the problem in more than six weeks. Ayre then told me "to go back out and spend some time" and come back with a more definitive answer.

At this point, I made a very stupid error in that I took his statement "to go back out and spend some time" literally. What I did was to spend some time to try to determine additional information for Ayre. Since my motivation was to *spend some time,* I decided to go up on the roof of the plant and take a sunbath. This would give me time to think up additional suggestions. This was the action I took.

After about two hours sunbathing, I returned to the shop and called Ayre on the phone. Using a phone was fortunate, since my face was now somewhat sunburned. I told Ayre that replacing one microphone element in the telephone in the guard headquarters would solve half the problem. Replacing all nine microphone elements in the phones in the guard towers and in the guard headquarters would solve the complete problem. Unfortunately, this answer did not suit him. He asked me, in less than flowery language, if there was any way to prove that I was right. I said, "Yes, of course."

I decided to dazzle him with a dramatic proof. I made a setup on a bench with two borrowed phones, a power supply, and a Du Mont 208 oscilloscope borrowed from a secret electronics sector with which to demonstrate waveforms and put on an impressive demonstration that clearly proved that the noise problem followed the microphones borrowed from the guard towers. As Scotty and Ayre listened to the phone connection on the bench and watched the screen on the oscilloscope, I could see that the electricians were watching this whole demonstration from a distance with puzzled expressions. The microphones were changed, and the noise problem disappeared.

I went to Professor Ballard and suggested to him that I had a better way to configure a magnetron and sketched it on the blackboard. He immediately erased my sketch and told me to forget the idea.

Having concluded that I would get no salary increases from the York Safe and Lock Company, I checked with my brother-in-law to see if I could get another job at the slaughterhouse. The answer was an emphatic yes. I asked York directly to be raised to $2.25 an hour, which was what the electricians were paid even though they were not able to solve the problem after spending more than seven or eight weeks on it. I told the York personnel manager I was about to catch a train for New York to get my previous job back and that I would return to York to terminate my employment with the York Safe and Lock Company. He made many promises to try to get me to change my mind, but none of them was satisfactory.

Back at Cornell

At the start of my junior year, I learned from Ray Pohl, a senior in the Electrical Engineering School, about the inordinate amount of time wasted on purely rote assignments required of electrical engineers handed out by the mechanical engineering laboratory. After three or four hours of laboratory work, students had to type their reports, draw the curves in ink, and copy voluminous phrases from designated texts. This required about 40 hours.

I proposed to my class members that we all keep good records of the amount of time that we were forced to spend in preparing these reports. At the end of the first semester of our junior year, two other class members and I presented our case to Professor E.M. Strong, chairman of the faculty committee of the Electrical Engineering School. The rest of the committee was composed of W.C. Ballard and L.A. Burckmyer. Strong said that it was not possible to ask the Mechanical Engineering School to change its course to meet our demands in time for the second half of our junior year, but the lab courses were changed the following year.

During my junior year (1942), I decided it would be better to enlist in the armed forces rather than be drafted. Having discussed it with my wife, I tried to enlist in the Army Air Corps but was rejected because of a hernia and was classified as 4F.

By my senior year, I had concluded that the rotating machinery course would not prepare me for my future, since I saw electronics becoming much more important. I petitioned the Electrical Engineering School to replace the rotating machinery laboratory course and substitute that course with a course in physics. I was refused. The Rotating Machinery Department was headed by Professor Burckmyer, one of the three professors on the Faculty Committee of the Electrical Engineering School.

The courses in electronics were exciting. While listening to Professor Ballard's lecture on a split plate magnetron, I began to sketch an eight-cavity magnetron. After the end of the period, I went to Professor Ballard and suggested to him that I had a better way to configure a magnetron and sketched it on the blackboard. He immediately erased my sketch and told me to forget the idea. This appeared very unusual and improper to me.

Before and during my senior year, I worked to help pay for my education by washing dishes in a fraternity house and assisting in Professor Gartlien's research project. I helped Professors Jim Krumhansle and John Trishka of the Physics Department to develop a magnetic amplifier to replace a Kelvin bridge circuit for the Navy to be used in an ocean depth gauge. The magnetic amplifier would draw much less battery current and be more sensitive.

Teaching at Cornell

I was surprised when, just before graduation, Professor Burckmyer asked me if I had ever given any thought to teaching. He said he would like to offer me a position as instructor in his rotating machinery laboratory. In view of the fact that I had petitioned to be excused from the very course in which Professor Burckmyer was now offering me a teaching assignment, I was taken aback. After talking over the offer with my wife, we decided that I should accept it. There were two benefits to the offer. First, an undergraduate degree gave a limited education in a high-technology field that was moving very rapidly. Second, I could try my hand at teaching, which I felt would be interesting.

When Professor Krumhansle learned that Burckmyer had offered me an instructorship, he suggested that I enroll in his laboratory course and use the building of a cavity magnetron as my semester project. As we were finishing the development of the magnetic amplifier, I noticed that with zero current in the current detector winding of the amplifier, we obtained a significant reading in the output meter. Krumhansle explained that it was due to Barkhausen noise. He explained that magnetic domains in the Permalloy core of the magnetic amplifier changed their magnetization in a random fashion and with a random amplitude. I immediately likened the problem to sand sliding down an inclined plane. If the inclined plane is vibrated, the sand will slide down the plane smoothly and not clump together. My suggestion to Krumhansle was that we apply what I called a shaker voltage, of approximately 20,000 cycles, to be superimposed on the 1,000-cycle driving signal This should cause the magnetic domains to flip at the 20,000-cycle rate.

A week or two later, when I entered the laboratory, I found Krumhansle ecstatic. He had tried coupling a Hewlett-Packard audio signal generator putting out 20,000 cycles in series with the 1,000-cycle driving signal and found that the Barkhausen noise had decreased by a factor of 100. He observed that we had done what Bell Labs had worked on long and hard to achieve, with little success, whereas we had achieved two orders of magnitude improvement with very little effort. It was my understanding that

Cornell had applied for a patent but was turned down, because the AC bias signal used on wire recorders at the time had anticipated our invention. This is the type of disappointment that a researcher or developer can expect once in a while.

My teaching schedule conflicted with Krumhansle's laboratory course. Krumhansle, however, thought the cavity magnetron idea was good enough that he went ahead and constructed one using what looked like a waterwheel, whereas mine consisted of eight circular cavities around a central circular cavity. He hooked up his magnetron and determined that the magnetron worked very well.

His success was short-lived. Professor Gibbs, chairman of the Physics Department, demanded that he remove the magnetron from the vacuum system. He then took it up to his office and put it in his safe. At this point, we learned that cavity magnetrons were considered highly secret by the government because they were used in radar systems. Professor Gibbs did not want the word to get out that Cornell was experimenting with cavity magnetrons, since professors who knew about cavity magnetrons might be suspected of inadvertently letting the information slip out. Now I understood why Professor Ballard had erased my sketch on the blackboard and had told me to forget the idea. We now know that by that time, the Germans had certainly shot down enough airborne radars to have penetrated the secret.

Teaching was interesting, challenging, productive, and gratifying. Teaching, unlike engineering development, gives immediate gratification.

Teaching was interesting, challenging, productive, and gratifying. Teaching, unlike engineering development, gives immediate gratification. One can work for years on a development effort before achieving success. In teaching, however, it is possible to tell during your presentation whether you are coming across to your students. If the students have puzzled expressions or fidget in their seats or do not ask any questions, you know immediately they have not comprehended what you have been telling them. Once in a while, you will have the great pleasure of having a very sharp student ask very penetrating questions or even describe an approach you had not considered. This immediate sense of gratification is what sets teaching apart from research and development work.

While I was teaching, I was also working toward a master's degree in electrical engineering under Professor Ballard as my graduate student advisor. He was a true gentleman and an excellent teacher. He was always willing to try to answer any question I posed to him. My first thesis involved developing an electronic device to generate an analog waveform of almost any shape by deflecting a rectilinear electron beam across a metal template. This involved both an electron beam gun and a vacuum system, which I had to fabricate. I undertook to fabricate it out of steel and glass. My friends in the Physics Department pointed out that it was very difficult to find the vacuum leaks in a glass system and next to impossible to find them in a steel and glass system. After I machined the vacuum diffusion pump plus all of the supporting hardware, I found that I could achieve a vacuum of 10^{-6} mm of mercury in about 10 minutes. There were no leaks.

But I stumbled in constructing the electron beam gun. I tried to make the deflection plates by electroplating copper onto properly machined brass forms covered with wax, which were then coated with graphite to make the surface conducting for the electroplating process. The wax enabled the electroplated copper electrode to be removed from the brass form by melting the wax. But the resulting electroplated electrodes were work-hardened. To anneal them, I had to heat-treat them, which then caused them to distort. This was too much. I discontinued this thesis subject and instead did a mathematical analysis of the ratio detector of a frequency-modulated receiver. It quickly became clear to me that analysis can be much easier than synthesis, because someone else has already done the synthesis.

Because of my machining ability, I was asked to be the faculty advisor for the machine shop of the Electrical Engineering School. In this assignment, I had to give suggestions to 65-year-old tool and die makers who had retired from years of productive work in industry. I was somewhat concerned that my youth, I was then 27, would be a problem. However, we got along very well when they found that I was comfortable in a machine shop and could offer suggestions that worked out to their benefit.

A humorous incident took place in a graduate course given by Professor Hans Bethe entitled Classical Mechanics. All of the graduate students in this course except me were enrolled in the Physics Department. There was a very young student in the class who always sat in the front row and never asked any questions of Professor Bethe. We were led to the conclusion that this very young man was probably a genius who had graduated from college at the age of 15 or 16 and was too smart to have to ask questions. One day, Professor Bethe remarked in his German accent, "You will remember in Physics 101 the way we handled this problem was ...." At this point, the young man raised his hand and asked Professor Bethe if this was not Physics 101. Bethe, with his German accent, replied, "Ach no. This is a course in classical mechanics." With that, the young lad grabbed up his books and ran out the door. While very humorous, this gave all of us a great deal of relief.

Brookhaven National Laboratory

In 1949, I completed my graduate work and was granted an MEE degree. Dr. Burrows, now head of the Electrical Engineering School, told me that I was being elevated to the rank of assistant professor of electrical engineering. By this time, my wife had gotten a job with the ASPCA hospital in New York City. My wife and our son Raymond lived with her aunt in Amityville on Long Island. Cornell did not permit an assistant professor to take graduate work leading to a PhD degree. Without a PhD degree, my future as a professor would be seriously limited, and we did not have the funds to permit me to do graduate work at another university. I requested and received a temporary assignment to Brookhaven National Laboratory on Long Island. This enabled our family to get together and enabled me to make the least painful transition to industry without the cultural shock of going from a university atmosphere to industry in just one step.

At Brookhaven, I worked on the radio frequency controls of the Cosmotron, a 2.5-billion electron volt proton accelerator. It was a vacuum chamber loop of four straight sections and four 90-degree quadrants of a toroid with a rectangular cross-section. In the toroidal sections, the beam was deflected by means of a magnetic field. The strength of the magnetic field had to increase as the speed of the protons increased in order to keep them in a circular path. The proton beam was injected into the vacuum chamber at 6 million electron volts and then accelerated to 2.5 billion electron volts by transformer action.

What I found interesting at Brookhaven was that there was a small cadre of engineers and scientists and a large cadre of technicians. Later, at IBM, I found the opposite to be true, the explanation being that engineers could do technicians' work but not vice versa.

As the end of my one-year assignment at Brookhaven approached, I saw that the economy had not picked up, and it was a poor time to go into industry. I got another one-year assignment. During this second year at Brookhaven, the Cosmotron was beginning to take shape. One of the problems was unwanted electric fields in the Van de Graaf generator. To investigate this problem, I was asked to calculate the fields inside the vacuum chamber. My relaxation calculation that made use of the circular symmetry of the structure required many hours on a mechanical calculator. It was this tedious process that probably caused me to conclude that computers would be the wave of the future. Toward the end of my second year at Brookhaven (1951), I decided that now was the time to get into industry and join a company that was committed to developing computers.

I asked him where Poughkeepsie was and who was there. When he told me IBM was there, my response was, "Oh."

I received a very attractive offer from Boeing in Seattle, but while attending a meeting at Columbia University, I went to a drugstore to make a telephone call, where I happened to see Jerry Haddad, a friend from Cornell. He had a scholarship from IBM while attending Cornell. He introduced me to Ralph Palmer and asked me what I was doing. I told him I was giving up teaching at Cornell and planned to get into computing. Palmer asked me to visit him in Poughkeepsie. I asked him where Poughkeepsie was and who was there. When he told me IBM was there, my response was, "Oh." I had heard the rumors about IBM and wanted no part of its kind of paternalism. Haddad asked me if I would visit Poughkeepsie, and I gave him a noncommittal yes.

I told my wife about our meeting, and she wanted to know if I was interested. I said no. This relieved her, because she was looking forward to living in the Seattle area. About a week later, I received a call from Haddad. I was too polite to tell him I was not interested, and so I agreed to go to Poughkeepsie for an interview.

Starting at IBM

Driving up the Taconic Parkway toward Poughkeepsie, my car's fan belt broke. This made me half an hour late for my meeting with Bob Blakely, the personnel manager. I gave Blakely my resume and excused myself to wash my dirty hands. When I returned, I questioned him about IBM's paternalism. He explained that the stories were not true and gave me examples that contradicted the rumors. I told him about Boeing's salary offer and explained that I would take less from a company that was predominately involved in a commercial business, since companies that are involved with military contracts hire and fire according to the government contracts they get. He immediately matched Boeing's offer, and, in fact, when I returned to Long Island, he called to increase the offer. It was obvious that Haddad had put in a good word.

My wife wanted to know why I had changed my mind about joining IBM. My explanation was that Blakely had convinced me that the rumors about IBM were exaggerated. Much more important was the fact that computers would become IBM's lifeblood, whereas Boeing's interest in computers was only as a means with which to design aircraft.

I joined IBM on 28 May 1951. My initial assignment was to provide engineering support for the electrostatic memory of the Defense calculator, which later was announced as the IBM 701. Haddad had hardware responsibility for the machine, and Nat Rochester was in charge of systems. Both reported to Palmer.

Williams Tubes

The electrostatic memory of the IBM 701 machine, the Williams tube, had two serious problems. The first was the read-around ratio. The memory consisted of 72 CRTs. Ones and zeros were stored on the faces of the tubes as minute charges at precise locations in the phosphor coating on the inside surface of the face of the tubes. The tube's electron beam both stored a charge to make a "1" and uncharged a spot to make a "0" with secondary electrons. Unfortunately, a repeatedly interrogated address produced many secondary electrons that could neutralize an adjacent address. The read-around ratio is the number of times a group of immediately adjacent addresses can be interrogated before the address in question loses its correct value because of this effect. The goal, as I recall, was a read-around ratio of several hundred. Initially, we were not even close. By observing the face of the CRT with a magnifying telescope, I saw that the electron beam did not repeatedly hit the same spot all of the time. This led me to believe that the power supplies for the deflection amplifiers were noisy. Further testing showed that I was right. I redesigned the power supplies, and the read-around-ratio specification was met.

The second problem was the "Indian blanket problem." When the computer was running, dashes and dots in horizontal stripes would appear on the face of the tubes, and the machine would crash. It got to the point that this bordered on the supernatural or that there was something fundamentally wrong with the machine that might never be fixed. I concluded that the problem was in one or both of the deflection amplifiers. I explained my idea to Phil Fox, my boss, and other engineers working on the computer but to no avail. However, Haddad talked to me on a Friday afternoon. He had heard that I thought I knew what the problem was and how to fix it. He asked if I could come in the next day so that I could try out my idea on what was causing the problem. I agreed. The two deflection amplifiers were DC amplifiers stabilized by noninductive wire wound around feedback resistors. I had calculated that a ±0.05 percent rapid change in the resistance of these feedback resistors could cause the problem.

To make our Saturday morning experiment, I selected a small carbon resistor that, when placed in parallel with one of the feedback resistors, should create the problem that had been causing serious delays in the program for two weeks. Thus, I was able to demonstrate to Haddad that a 0.05 percent change in the value of the feedback resistor caused the Indian blanket pattern. We tried this experiment many times to convince ourselves that it was repeatable. We were both convinced, and Haddad suggested that on Monday, I remove and replace the resistors in question and determine what there was about their construction that caused the problem.

The resistor element consisted of two resistance wires wound as two counter-rotating helixes on a ceramic bobbin. The ceramic insulating coating on the wires did not adhere, allowing electrical contact between the helixes at points with almost the same electrical potential. This made extremely small but sudden changes in the total electrical resistance of the wire-wound resistor. When the resistors were replaced with properly constructed components and the problem did not recur, the tension that had been created by the Indian blankets slowly dissipated, and things returned to normal.

The social dynamics in a situation like this are interesting. The problem arises suddenly, causing extreme concern. When the cause of the problem is identified and fixed, the concern still remains, because continued proof is required that the fix is correct. This is especially true if several fixes have failed. The conviction that a fix has been achieved may take a long time to be accepted. Then the concern slowly diminishes to the point where there is no longer a concern, the original concern is slowly forgotten, and, indeed, the problem itself is forgotten. Those who do not know the mistakes of history are bound to repeat them.

Transistors

As the IBM 701 neared completion toward the end of 1952, Haddad came to me and asked me to join a small group working on investigating transistors, invented by Bell Labs in 1947. The IBM group consisted of five young engineers and no technicians. It was not obvious to me who managed the group, and Haddad did not tell me if I did. This appeared peculiar, but I was still new with IBM. Although I had had a great deal of circuit design experience with vacuum tubes, I had to learn very quickly about transistors. I had a firm fundamental understanding of solid-state physics, but I was taken aback to learn that the group members had focused their attention on point-contact transistors.

I spent my first week in the group building transistor circuits on the workbench. I found that minority carriers in the germanium prevented the point-contact transistor from immediately turning off when the input signal was removed. By preventing the collector voltage from approaching the base voltage of the transistor with a diode clamp, the turn-off characteristic of the transistor was improved. At the end of the week, I had a feel for the problems of point-contact transistors, and I asked the group if there were other kinds of transistors available. They told me there were both grown-junction transistors and alloyed-junction transistors and described their problems. I was told they were slow, could only find application in hearing aids, and were highly temperature sensitive and thus not suitable for high-speed digital circuits. The point-contact transistors were faster and more insensitive to temperature changes. In addition, a single point-contact transistor could provide a circuit with two stable states. I realized that Haddad had handed me a hot potato, but I thought it might be possible to turn around this poor situation. Now I had to learn more about junction transistors.

During this period, a hernia operation, performed prior to my joining IBM, had to be redone, and I was away from work for several weeks. By 31 March 1953, I had independently invented what is now called the Schottky clamp circuit. Fig. 1 is a copy of page 64 of my engineering notebook showing the 1N56A diode that clamped the collector voltage to −3 or −1.5 volts with almost equal results. Patent No. 2872594 entitled "Large Signal Transistor Circuits Having Short Fall Time" was issued on 3 February 1959. An engineer at Philco obtained a patent on a similar approach but with a date of invention that was later than mine. The IBM patent department threw my patent into interference with Philco's. IBM won the interference and incorporated Philco's claims into a new patent with a new issue date.

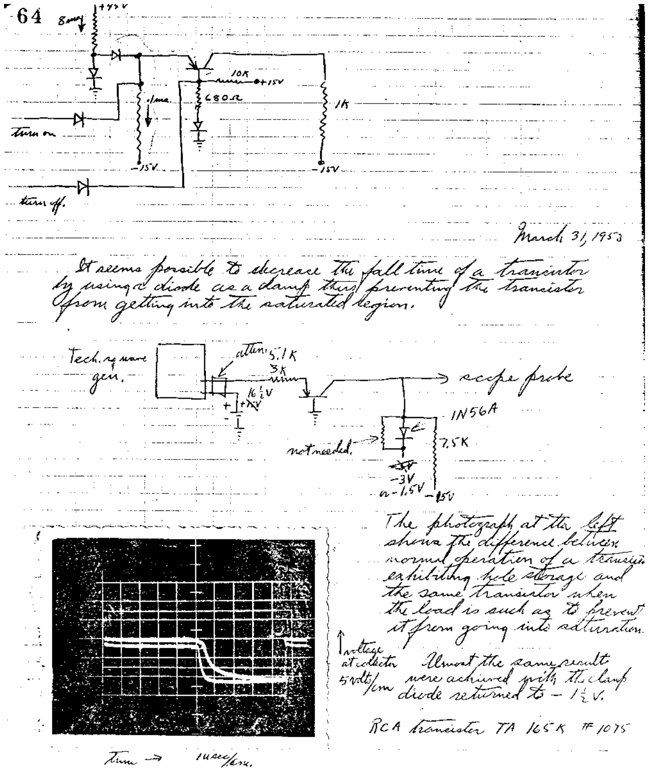

Fig. 1. Engineering notebook showing the concept of the clamp circuit.

We ordered both types of junction transistors. They were available from several suppliers, and we designed circuits around them. We found that we could design around the temperature sensitivity. I saw there was no fundamental reason why the alloy-junction transistors could not be made to have a higher band width. I could see that alloy transistors would be much more reliable than point-contact transistors and could be produced economically. In addition, the junction transistor had significant power gain over the point-contact transistor. I totally changed the direction of the group and focused our circuit design effort entirely on alloy-junction transistors.

The change in direction took many months. There were many obstacles. Arthur L. Samuel and Lloyd Hunter had convinced upper management that point-contact transistors were ideally suited to IBM's needs, because they were fast, a single transistor could provide two stable states, and they were relatively insensitive to temperature changes. To demonstrate to the reader how slow this process of change was, Hunter had asked me to write a chapter in the McGraw-Hill *Handbook of Semiconductor Electronics*. To avoid or minimize a confrontation with Hunter, I split my chapter into two parts, rather than deleting any mention of point-contact transistors. The first part dealt with point-contact transistors and the second part with junction transistors. This enabled me to just delete the point-contact section entirely in a future rewrite.

The transistor issue was never resolved in IBM. It just faded away as it became evident that the rest of industry had adopted junction transistors.

In the summer of 1953, Palmer asked me to set up an education program for 28 engineers selected from both the Poughkeepsie and Endicott laboratories. He wanted the course to teach how to design transistor and core memory circuits. I designed a course of three weeks of classroom work plus six weeks of laboratory work. I divided the 28 engineers into six groups to do a paper design of six machines during the laboratory portion of the exercise. This course became known as "Logue's College of Digital Knowledge," in imitation of that prewar radio band, Kay Kyser's "College of Musical Knowledge." Palmer sat in on the oral examination held at the end of the course to understand what the participants had learned. I proposed to Palmer that three of the six machines be built. He agreed. They were:

1) the Data Transceiver,
2) the Small Accounting Machine, and
3) the Type 604 calculator.

During this building program, I decided that the eight volts we had selected for the power supply was too large. I was not concerned about the reliability of the transistors at this voltage level; rather, I was concerned about the energy stored on stray capacitance. Since energy is a function of $CV^2/2$, by halving the supply voltage, the energy the transistor must supply is reduced by one fourth, and a given transistor can operate at a higher speed. Finally, a computer operating at a lower voltage level would generate less heat. To determine what supply voltage would be reasonable and practical, I first made an educated guess. I remembered that silicon Zener diodes had a zero temperature coefficient when their Zener voltage was approximately five volts. I thought that in the future it might be possible to use a Zener diode in series with a dropping resistor as an inexpensive power supply. Using the same supply voltage for all three experimental machines did not make much sense to me. With this in mind, I decided that the circuits for the transistorized version of the 604 calculator should be designed around a five-volt power supply. Dick Weiss, the team leader of the transistorized 604 experiment, objected strongly. I assured him that I would take complete responsibility. Fig. 2 shows a 1953 printed circuit card that was used in the transistorized version of the IBM 604 calculator. The germanium transistors, housed in a hermetically sealed can, were plugged into sockets for easy removal.

The IBM transistorized 604 was completed in time for the dedication of the IBM 701 building in Poughkeepsie on 7 October 1954. It contained 595 cards such as the one shown in Fig. 2 holding 2,200 transistors instead of the 1,250 vacuum tubes in the commercial 604. It consumed 5 percent of the power and occupied 50 percent of the volume of the commercial 604. It was exhibited all around the United States with few problems. In fact, Hunter criticized my circuit design as being too conservative, because there were so few problems. It must be emphasized that all the circuits and card designs for three experimental transistorized machines were done by my group, consisting of George Bruce, Carter Dorrell, Ray Emery, Bob Henle, Al Lampe, and me.

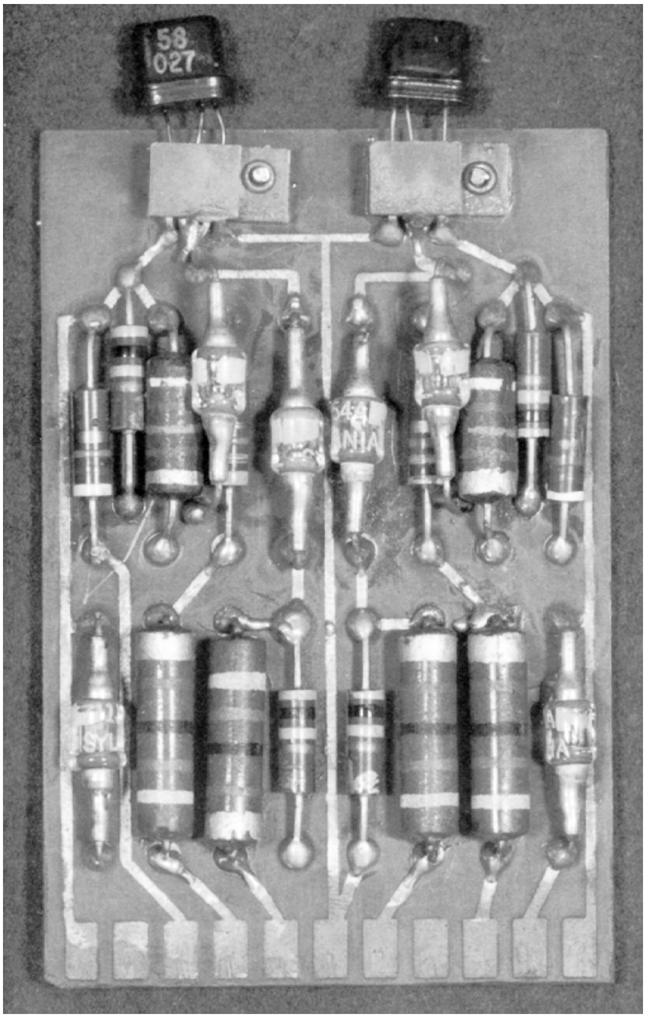

Fig. 2. A 1953 printed circuit card used in the transistorized version of the IBM 604 calculator. It is approximately 2 × 3.25 inches and contains resistors, diodes, and two transistors inserted in holders.

In the summer of 1954, Palmer told me to develop new and exciting devices and circuits. I suggested that we should take advantage of what we had learned from the transistorized machines that had been built during Logue's College of Digital Knowledge and use these results to prepare for new products. I could not sway him, so my group set out on a "Blue Sky Program" on our own. I told Fred "Rick" Dill, a summer employee in 1954, to design a semiconductor device with 10 stable states using silicon, which I considered to be the semiconductor of the future. I suggested that he use the principle of the double-base diode, so all semiconductor activity would take place inside the bulk silicon. My goal was integrated circuits (ICs).

By the end of the summer, Dill had demonstrated a silicon device with four stable states. He ran into a problem with John Little, who objected to his use of silicon in Little's furnaces because Little was following the party line that germanium was the IBM semiconductor of choice. IBM's argument was that the carriers in germanium had a higher mobility than did silicon and hence would produce faster transistors. I did not disagree with this fact, but I wanted to do what was needed to build fast machines, whether or not this required the fastest internal devices.

I told Hunter that it did not take much imagination to conclude that it should be possible to oxidize silicon to produce SiO_2, in which case, transistors could be produced that would not require a hermetically sealed enclosure. I also pointed out to him that with silicon's higher energy gap, the transistors could operate at a higher temperature than with germanium. Silicon's lower minority carrier mobility is overcome by its ability to be oxidized so as to form a protective film, and this enables silicon devices to be packaged in a smaller volume than germanium devices. This, in turn, reduces the time delay in transmitting an electrical signal from one transistor to another. Thus, a computer designed around silicon transistors can be much faster than a computer designed around germanium transistors, in spite of the lower mobility of its carriers.

While I have no proof, it would give me a great deal of personal satisfaction if the five-volt power supply standard for ICs that TI established could be traced to our pioneering effort on transistor circuit design in the early 1950s.

At about this time, Willis A. Adcock of Texas Instruments (TI) visited me in Poughkeepsie and asked for drawings of the logic circuits in the transistorized version of the 604 calculator. I sent him the drawings he requested. I did not know that Jack Kilby of TI was inventing the IC. When Hunter's handbook was published in 1956, the circuits that were used in the transistorized version of the 604 calculator became public knowledge.[2] While I have no proof, it would give me a great deal of personal satisfaction if the five-volt power supply standard for ICs that TI established could be traced to our pioneering effort on transistor circuit design in the early 1950s. That, at least, is a standard for which I would be proud to be recognized.

I knew that my small group of engineers could design only a limited number of parts and packages, but I also believed it would be possible to design a small number of transistor circuits that could satisfy the needs of many digital systems. By placing one logic circuit on a printed circuit card, only a few part numbers would be needed to design and service many systems. Palmer was paying close attention to my strategy, and he soon put me in charge of standardization. He made it clear, and stated it in no uncertain terms, that the reason he had me reporting to him was that if he got fired for lack of standardization in IBM, one microsecond later so would I.

During this period, I was in charge of all transistor circuit development. I had challenged my team to measure the performance of the transistors in all regions in which a transistor could be operated. Transistors did best when operated far away from saturation, as I learned in early 1953. When I asked for a switching circuit that operated the transistors away from saturation, Hannon Yourke came up with one. I did not push my earlier invention of the Schottky clamp circuit but challenged my people to come up with a better way, as they did. Dr. Emerson Pugh has reported a different version of this history.[1]

The Movie

Palmer strongly suggested that I put together a movie to depict the benefits of standardization. Bear in mind that I was not suited to be the standardization czar, because I liked to invent my way around problems rather than slavishly adhere to preset standards. Indeed, invention is the antithesis of standardization. However, when I am told to do something that is not totally ridiculous, I will generally find a way to accomplish the task because of my inventive nature.

I assembled a movie-making team and told them what I wanted for the opening scene. Knowing that the large mainframe vacuum tube computers needed approximately 4,000 part numbers, I had 4,000 vacuum tube modules laid out on the floor in a regular pattern and had the camera pan, at a low angle, along the first row of modules, as if they were on a shelf. Then the dolly moved back and took in the whole scene of 4,000 modules while the voice-over pointed out the wastefulness of lack of standardization. I thought it would get people's attention. How right I was. Bud Beattie, the manager of the IBM Poughkeepsie Laboratory, previewed the movie and immediately called Palmer and told him if Tom Watson, Jr., saw this movie, that he, Palmer, would get fired. My career as a movie producer came to a rapid end. The movie was never shown. I did not even get to take a bow let alone do an encore.

Palmer also impressed on me that he wanted no more than 16 printed circuit card part numbers. I said that was impossible, because I had to supply circuits and card part numbers for the 7040, the 7090, and Stretch computers. I guessed that it would take at least 65 part numbers to supply the requirements of all the transistorized machines being designed. The number of part numbers ultimately released was approximately 4,000 for the 7000 series machines. So much for Palmer's and my attempt at standardization.

My mother died in 1958 at the age of 85. She had used her savings to send me to college. By washing dishes, working on research programs, taking summer jobs, and obtaining a scholarship, I was able to reduce the cost to my mother. After I started teaching at Cornell, my wife and I were privileged to be able to send her \$50 per month from my salary of \$900 per semester, and we continued our support for the rest of my mother's life.

Component Division

In January 1964, I joined the Component Division of IBM. I was asked to assemble a team to develop the next-generation technology (NGT) for IBM's machines that were to follow the just-announced System 360 line. I had to direct groups that were outside the Component Division's locations. One was located in Owego, New York, another in the Research Lab in Yorktown, New York, and other groups were around Poughkeepsie.

The group in Owego, headed by Dr. Rico Di Pietro, had been working on ICs. The group in Yorktown, headed by Dr. Hollis Caswell, had been working on superconducting cryogenic devices. I concluded that I had been given all the technical people that upper management did not know how to use effectively. Since my program needed people, this solving management's problem of what to do with these people. First, I tried to use the IC effort in Owego to support the NGT program. Then I tried to determine the practicality of the superconducting program in Yorktown. Finally, I had to see how to package ICs for use in both small computers and mainframes.

There had been no work in IBM directed toward ICs or how to package them. More than two years later, I learned that Dr. John Gibson, president of the division; Dr. Andy Eshenfelder, the lab director of the Fishkill, New York, Lab; Erich Bloch, in charge of development; and Robert Henle, a senior engineer, had told the Corporate Technical Committee (CTC) of IBM in November 1963 that ICs would never be able to compete with discrete semiconductors. Henle stated that discrete devices can be tested one at a time and can be selected to close tolerances. In the industry, this is called "cherry picking." He said that resistors in integrated chips can be made to tolerances only within ±20 percent. Never during my two years' tenure in the Component Division did anyone tell me that the top management of the Component Division had committed themselves to ridiculing ICs. They asked me to define the NGT program. They knew from the moment I stepped in the door that I would be pushing ICs, but they never warned me off them. Shortly after I joined the division, Eshenfelder asked for my plans. When I gave him a fairly detailed description of my program, which included ICs, he asked for more details. I complied with this request and concluded that I was beginning to run into bureaucratic static. I did not know that I was hitting a preconstructed wall of prejudiced ignorance.

The reader might conclude that I was frustrated, and he or she would be correct. My frustration was our inability to make progress at the rate I felt was achievable. Let me recall the Indian blanket problem. It took two weeks for my solution to the problem to percolate up to Haddad, the senior technical manager of the 701 program. Certainly I could have gone to Haddad directly, but that is not my style, particularly in view of the fact that he was instrumental in my joining IBM.

I needed a solution as to how to package ICs. I asked Dr. Jack Riseman what new work he had going on in his small laboratory. He showed me around and introduced me to Dr. Bernie Schwartz, a ceramicist. Schwartz showed me ceramic sheets that looked like thick sheets of paper. He told me:

- it was possible to print patterns on these ceramic sheets by means of a silk screen process,
- the ink could contain very fine particles of a refractory metal such as tungsten or molybdenum, and
- many layers of these printed sheets could be bonded together and sintered into one solid ceramic sheet.

I asked him if holes could be punched into the green ceramic sheet before firing and filled with the ink so that paths could be formed to make connections between layers. He said that was possible. I realized I would use this process to make IC packages.

Integrated Modules

Early in 1964, we contracted with TI for two sets of nine integrated modules. I asked Bill McCanny, who headed up a small group in my NGT program, to design and build a transistorized version of a card-punch machine using large-scale ICs from TI. I wanted to see what problems we would encounter. Kilby, who headed the TI program, used the standard TI series 53 IC wafers. Each wafer was electrically tested to determine the location of the good circuits, and these were then interconnected to form a complete working IC omitting any bad circuits. The wafers contained more than 100 logic circuits, which we considered to be large-scale integration. The printed circuit card containing nine modules shown in Fig. 3 was the result of this experiment.

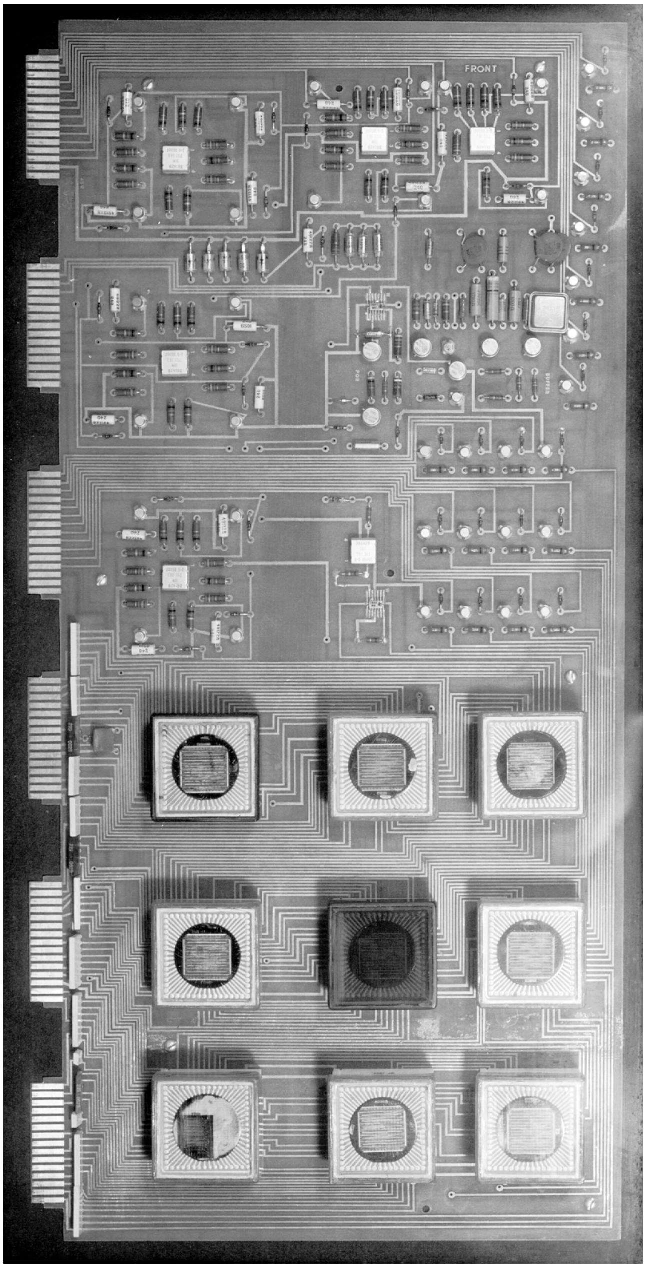

Fig. 3. A 10.5 × 23 inch printed circuit card containing conventional components on one side and nine nodules, each containing a 1.25-inch silicon wafer fabricated by Kilby's group in TI.

McCanny's experiment showed that it was possible to build and debug such a system. It also showed me that we had to have multilayer ceramic packages that held multiple IC chips containing only useful circuits. The wasted silicon area taken up by bad circuits was unacceptable. This convinced me that my concept of the NGT program was headed in the correct direction.

By June of 1964, my NGT program had four phases:

- In the first phase, IC chips with a few circuits would be placed on ceramic chiclets with 16 pins as had been developed for the Solid Logic Technology program. This would get IBM into IC production with the minimum expense and with the maximum benefit.
- In the second phase, I proposed larger chiclets to hold more input/output pins to support more circuits on the IC chips.
- In the third phase, my proposal was aimed at the low end of IBM's products, for which the modules would use ceramic multilayer modules containing many IC chips.
- The fourth phase would be directed toward the high-end computers. These mainframes would make use of multilayer ceramic modules containing many IC chips directed toward high speed and liquid cooling.

When I explained this plan to Haddad, he told me to forget the first three phases and concentrate on the fourth phase. I, like a fool, listened to him.

By the beginning of 1966, IBM wanted a new technology to replace the solid logic technology of the System 360 machines. Haddad assembled a committee to review the available technologies and to report to top management an approach to "save the day." My fourth-phase program was much too aggressive to even begin to meet the needed schedule. However, there was a technology that had been quietly worked on, and it was proposed to Haddad. It was my first-phase program of NGT. Haddad jumped on it. I was told to put together a team to work on this "new technology," which was named MLT. In doing so, I stripped the NGT fourth-phase program almost bare.

Dr. Bill Harding ... dismantled my multilayer ceramics laboratory.... To avoid using multilayer ceramics, the Components Division spent millions of dollars.... This approach did not work.

I decided to test the competence of IBM top management in Armonk, New York. I told Haddad that I had moved most of my people from phase four to phase one. Now did he have a job for me? He made me assistant to Dr. Art Anderson, who reported to Dr. Manny Piore, vice president and chief scientist. Anderson was scheduled to leave his post during the summer of 1966, and when he left, I was given the job of staff director of the CTC, reporting to Piore. My job was to assist in the preparation and evaluation of the technical strategies from the many divisions of IBM and recommend acceptance or rejection to Piore. These strategies covered the technical plans of the particular division for a five- to 10-year period. I also was responsible for structuring the agenda for each CTC meeting. The meetings were held monthly and were attended by the presidents of the IBM divisions. I had a staff of approximately five very senior technical people. Those I remember are George Bland, Bob Myers, Lyle Johnson, Jim Pomerene, and Eugene Shapiro.

At the Component Division, Dr. Bill Harding, who took over what was left of the NGT program, dismantled my multilayer ceramics laboratory and pilot line. Earlier, I had tried to convince Ed Garvey, who was in charge of manufacturing in the Component Division, and Karl Weiss, in the IBM Boeblingen Laboratory, in Germany, of the future necessity of a multilayer ceramics capability within IBM. Although Garvey did not heed my request, Weiss did. To avoid using multilayer ceramics, the Components Division spent millions of dollars trying to develop a module consisting of a ceramic base that supported multiple layers of evaporated aluminum wiring, which were insulated from each other by glass. This approach did not work.

One of the first technical strategies to cross my desk as staff director of the CTC in June or July of 1966 came from Bloch for the Components Division on the subject of computer memories. At that time, IBM was heavily committed to magnetic core memories. The essence of the strategy was:

1) to continue with the M-250 memory for the near term;
2) to depend on thin magnetic film memories for the future; and
3) to "sprinkle" semiconductor memories in computers where needed for registers.

I was appalled, because this completely ignored what was happening in the semiconductor industry. I explained my disagreement with the Memory Strategy to Piore, and he asked me to draft a memo to Frank Cary for his signature. A year later, Bloch was replaced by Ed Davis. As a result of my continuing emphasis on the importance of ICs—as manager of the NGT program, as the staff director of the CTC, and later after I left Armonk—IBM gained a leadership position in semiconductor memories.

In 1969, the U.S. Justice Department slapped IBM with a monopoly suit. I was promptly replaced by George Kennard, and Haddad was transferred to the Poughkeepsie Laboratory. I refused the offer of a job in the IBM Raleigh, North Carolina, Laboratory. I was assigned to Haddad as his technical assistant. It was obvious I was being placed in a parking orbit, outside the sphere of action. Shortly after I joined Haddad, he told me that competitors were causing IBM fits by selling plug-compatible core memory modules. I proposed that IBM quickly change its core memories to semiconductor memories and hide them under the covers of the mainframe. As I had pointed out in 1964 at the start of the NGT program, a semiconductor memory could be packaged like logic and could be placed in very close proximity to logic, thus reducing the time delay between logic and memory. In addition, a semiconductor memory could use the same power supply voltage levels as logic.

Haddad set up a committee to study the issue with me as the secretary. After at least a month of meetings, IBM launched a top-priority effort to develop a semiconductor memory capability. I learned an important lesson from this. It is necessary in any large organization to build up a consensus that supports your proposal before taking your proposal to higher management. "Too soon old, too late smart."

In 1971, IBM made me an IBM Fellow. This permitted me to pursue any activity that I chose with the help of about five people. I chose to investigate programmable logic arrays (PLAs). I had felt for a long time that the master slice approach of the Components Division would have a limited lifetime. Furthermore, Hal

Fleisher had been developing the theory of PLA, and we could jointly advance the concept. Our programs need not conflict, since I was pursuing the practical application, whereas Fleisher had been concentrating on the theoretical development.

In the early part of the 1970s, I joined Bob Evans in a trip to IBM Germany to see Weiss's progress in developing multilayer ceramics. On his return, Evans demanded that the Component Division adopt Weiss's work. Bloch had to further develop and manufacture the multilayer ceramic package that was subsequently used in IBM's mainframe computers in the 1980s. By then, IBM found, to its great satisfaction, that the multilayer ceramic multichip module could be used to build mainframes to produce a multibillion-dollar revenue stream.

I suggested to Evans that Schwartz be given an award for the work he had performed by setting up a multilayer ceramic pilot line. Evans told me that such a suggestion had been proposed before and was rejected because one major invention had not been patented by Schwartz. I built a case to support such an award by showing that all the elements of the present approach were anticipated in the work done in the NGT program. As a result, Schwartz received, as I recall, a $35,000 patent award from IBM.

We showed that the revenue after the third year would be approximately $50 million, and the product would be profitable in the fourth year. Papes's response was that $50 million would be lost in the round-off error of the revenue of the division.

Supporting an FET Program

We began supporting Bill Gianopolus's effort in the IBM Kingston, New York, Laboratory to produce a custom-designed large-scale IC chip using NPN and PNP field effect transistors (FETs) in a dynamic logic configuration. The Component Division had been developing a master slice approach to FET chips. The fundamental problem was that an FET design that must conserve silicon area cannot drive many other circuits. In the master slice approach, a circuit may have to drive another cell some distance away, but the stray capacitance of the connection conductor cannot be known until the complete chip is designed. As a result, each cell must be designed to handle the worst-case conditions, which leads to cells that

- are slow,
- draw too much current, and
- create too much heat.

The Component Division saw Gianopolus's program as a competitor to its program and publicly deprecated it.

In supporting Gianopolus, I had placed my reputation on the line. While I consistently disagreed with the Component Division and the forerunner of the Research Division, I almost always was found to be correct. Before Gianopolus had a successful working chip, I decided that a good offense makes the best defense and attacked the Component Division's FET program. In talking to the working members of the FET program, I had learned that it had a serious technical problem. Their circuits would latch up, that is, after they fired, they would continue to conduct current after the initiating signal was removed. I put my findings in a memo to a number of people, including Haddad. Haddad called a meeting to which I was invited. When I walked in, Haddad pointed his finger at me as if to fire a gun and yelled "Bang!" The tenor of the meeting did not improve. It took six or eight months before the FET program was terminated. The semiconductor device approach was correct once the latch-up problem was fixed. The master slice approach was all wrong. The demise of this CMOS FET program in the Component Division was later found to be a serious setback for IBM. They threw the baby out with the bathwater.

It was another example of scientists not understanding total practical application. A technical organization must contain a balanced representation of all the skills that are needed for the program to be successful. I feel a great deal of my success can be attributed to the fact that I created for myself at Cornell an engineering–physics set of courses. In addition, I am well-versed in what is required to make parts and manufacture a product. In short, I am practical.

The Glass Cockpit

As an IBM Fellow, I supported the development of the "glass cockpit." Mike Fader proposed to replace D'Arsonval needle movements in the instrument panels of commercial aircraft with CRT display screens. He knew I was an avid flier and that I owned a twin-engine Aero Commander 680. I thought that this idea might be a way to get IBM to support a custom chip design effort based on FET technology, which I was convinced would be needed for future large mainframes. I knew I could not do this by just making the proposal in my yearly IBM Fellow report or by going to management. I had to supply a business opportunity that would lead the way.

My group set about to develop a model for demonstration in my aircraft. A gas panel display was driven by a small computer with input from an attitude gyro. We set up focus groups for chief pilots of commercial corporations to whom the concept was presented in detail. We did a market study to determine the revenue and manufacturing costs. We made a great presentation to Ted Papes, president of the IBM System Products Division. We showed that the revenue after the third year would be approximately $50 million, and the product would be profitable in the fourth year. Papes's response was that $50 million would be lost in the round-off error of the revenue of the division. This product proposal and my effort to get IBM into large-scale integration with the FET technology, to protect IBM's future business, would not fly. No pun intended.

Communications Division

After five years as an IBM Fellow, I got back into a management line job by joining the Communication Division headed by Allan Krowe. The Communication Division, among other things, was responsible for terminals. The division found it difficult to get the components it required. A representative from division headquarters in Harrison, New York, gave us a pep talk saying that we should persuade the Component Division and our brothers in Endicott to work harder to turn out more products for our manufacturing plants. I contradicted him, saying that we should realize that in good economic times, the mainframes, because of their high profit, were given priority for components we needed as a division. During poor economic times, we could get the parts we needed, but then the market for our products was poor. I said the speaker reminded me of Neville Chamberlain running around Europe with his umbrella trying to appease Adolf Hitler. If we wanted to solve our problems, we should seek vendors outside of IBM. Then, at least, we could get parts for our products when the economy was booming.

The next thing I knew I became the division's technology manager. I negotiated a contract with Matsushita in Japan for a controller for a gas display panel for terminals based on the PLA technology that I had developed as an IBM Fellow. In negotiating a contract with Matsushita, I learned that Matsushita expected to have to provide us with IBM's unique connection to a substrate in the form of lead balls, but I told the Matsushita engineers to use the wire bonding with which they were quite familiar. Furthermore, since a terminal is turned off and on many times, the resultant thermal cycling would cause fatigue failures of the lead ball connections. I invited five of the Matsushita engineers to Kingston to learn about PLA design. At the end of one week, Fugimoto San, the Matsushita executive with whom I had negotiated during my week in Japan, reduced the estimated cost of \$3.6 million for the contract by \$1.2 million, bringing the total cost to \$2.4 million. The results of Matsushita's efforts were quite satisfactory.

My recommendation that the program be continued was based on the fact that the program had been so badly mismanaged that it was difficult to determine if the basic problem was mismanagement or the technology.

Josephson Program

In 1981, I headed a technical audit of IBM's Josephson program by a group of approximately 15 engineers and scientists. We met for several weeks. The Josephson program had people in East Fishkill, New York; in Yorktown, New York; and in Ruschlikon near Zurich and got some financial support from the National Security Agency (NSA). Only three of us recommended that the program be continued. I was one. My recommendation that the program be continued was based on the fact that the program had been so badly mismanaged that it was difficult to determine if the basic problem was mismanagement or the technology.

The program had developed a siege mentality. The program members felt that every other group in the Yorktown Research Laboratory was against them, so they hid all of their technical problems. In addition, the members of the Josephson group had very low morale. That portion of the group in East Fishkill was not getting the facts needed to build a pilot line. The Zurich group had begun to evidence a general dissatisfaction with the technical direction of the program. At first, I did not talk to the workers, but as we were about to conclude the audit, I began to get input from the lower levels. At this point, I circumvented the upper-level management and talked directly to lower-level managers and workers. I learned that significant portions of the technical data presented to us were wrong. I called Ralph Gomory, who was in charge of all IBM Research., and told him that the audit team report was not worth the paper on which it was written. He told me he would take care of it. He offered me the job as manager of the Josephson program.

I later learned that he had made the offer to several others before me, so I suspect that I was the only one insane enough to take it. I said that I would accept his offer, but he must understand that I had only one chance in 10 to be successful and that it would take two years to determine if the program would fly. He told me that I would report to Jim McGroddy. I said that McGroddy had stated publicly that if he got the chance, he would terminate the Josephson program. Thus, while my chance of success would have been very small, with McGroddy as my boss, it was zero. Gomery said that he would take care of McGroddy.

Later, McGroddy asked me why I had taken the job. I said that the program had been so badly mismanaged that I thought I had a 10 percent chance of being able to make it work. He asked me very directly to keep him informed of any problems with the program as soon as I learned of them. I assured him that I would. True to Gomory's word, I did not have any trouble with McGroddy. In fact, we got along very well.

First, I addressed the morale problem. Then I had to convince the rest of the groups in IBM Research that the Josephson program would welcome their input and that there would no longer be a siege mentality. The members of the Josephson program were handpicked scientists from the top universities of the United States and foreign countries. There was no way that I could run a group such as this without leaning very heavily on their knowledge and experience. I spent three months getting the Josephson people to understand their strengths and weaknesses and to have them define the type of organization that was needed. I set up six committees to define the programs in key technical areas. This let me determine who the natural leaders were and the details and schedules in the key technical areas.

At the end of three months, we had a team of competent leaders who had defined what had to be done and on what schedule. In addition, everybody knew exactly what had to be done and what role they each had. I was extremely impressed with the lack of politicking that one would usually find in such an organization.

The program required a drastic change in direction. It had previously been structured to build a supercomputer based on superconductivity and Josephson devices and deliver it to NSA. This goal was totally unattainable with a group of about 125 spread through three separate locations on two continents. I cut the goals down considerably, but I had to demonstrate that superconductivity was a technology that was superior to silicon and gallium arsenide. The performance of a Josephson computer had to be at least an order of magnitude better than its competition at a cost that would be slightly more than a machine designed around either of these two competing semiconductors. While the previous management had done a clever job of defining a package that could be used at the temperature of liquid helium, they had not considered that a new technology must be measured on a cost/performance basis compared to other technologies.

By the Labor Day weekend of 1983, I had to decide whether or not to sign the contract with NSA for another year. We had made significant technical progress during the previous 25 months. I had told Gomory that after two years, I would tell him whether the program would fly. I had the results of a technical audit of the program that I had asked Pugh to head.

The key points of the audit report were that with minimum dimensions of 0.1 micron, the Josephson technology might be twice as fast as the competing technologies. Furthermore, the Josephson memory cell would take up more area than the competing technologies. This might force the memory for a Josephson machine to have to be operated at the temperature of liquid nitrogen with silicon as the semiconductor material. Finally, the transmission line delay between the memory and the CPU would be unacceptable for a general-purpose machine.

As thousands of people around the world were engaged in improving the silicon technology, the minimum feature size for the silicon technology was decreasing. It was clear that the 125 members of the Josephson program could never compete with this in improving the Josephson technology. A proper return on investment could not be achieved with the Josephson technology. I told John Armstrong, who had earlier replaced McGroddy, that I recommended the termination of the program, and we both informed Gomory. The decision was subsequently reviewed by Haddad, Evans, Louis Branscomb, and other members of the top management in IBM and NSA. Then I had the sad assignment of reassigning the people in the program.

One internationally known metallurgist thanked me for what he had learned on the program. He told me that before I took over, he had investigated whatever subject caught his attention. When I came on board, he found that he was asked to target his attention to subjects that were important to the overall program. I thanked him very sincerely, but it caused me to think about the structure of the program when I joined it. What I found was that many of the 50 PhDs on the team were more interested in work that would lead to an individual publication than in working as a team to advance the program. I think my management approach was instrumental toward turning a bunch of individuals into a team. Perhaps the metallurgist had attempted to express this same thought in a different way.

While I was reassigning my people, Armstrong asked me what my interests would be for a new assignment. I said that I did not want to think about a new assignment until I had my people properly placed. Later, when I told Armstrong that I had all of my people placed and was now ready for a new assignment, there was dead silence. I felt this was a bit unusual and did not know what to make of it.

In 1982, Gomory had told me that I was no longer eligible for stock options. By coupling Armstrong's silence with this pronouncement, I realized that any future outstanding accomplishments would no longer be appreciated by IBM. At that time, Dr. Harry Kroeger, whom I had interviewed for a job in the Josephson program before I decided it should be terminated, told me that his current firm, Microelectronics & Computer Technology Corporation of Austin, Texas, was looking for a vice president. I contacted the firm, was invited to Austin, and was interviewed by Bobby Inman. The salary offer was impressive, and the bonus plan was extremely attractive. My wife agreed that I should take the job.

In my exuberance, I mentioned my new job to Evans, who was very negative about my taking the job. I said that Inman had answered my concern about a conflict of interest on my part by explaining that an IBM executive told him that while IBM did not plan to join in a business relationship with Microelectronics & Computer Technology Corporation, that IBM did wish the firm well in its new venture. Evans talked to Gomory about my decision, and Gomory asked me if I would talk to John Akers. I had a productive discussion with Akers, who said he would recommend to the board that I be awarded a restricted stock option if I stayed. As a result, I agreed to remain with IBM.

Packaging Research

Since I had accomplished very significant results in developing digital circuits and digital circuit packages over my years in IBM, I assembled a research effort directed toward packaging. I was appointed director of packaging technology in IBM Research. This seemed fitting in view of my past accomplishments:

1) redirected IBM's effort toward junction rather than point-contact transistors,
2) invented the Schottky clamp circuit,
3) recognized the benefits of complementary symmetry in circuit designs,
4) defined the standard modular system concept,
5) established a five-volt signal-swing standard within IBM,
6) established the first effort on silicon in IBM,
7) guided the invention of the current-switch circuit,
8) established an IC effort in the Component Division,
9) defined a four-phase NGT program that was very closely followed in IBM,
10) initiated the MST program as the first phase of NGT,
11) anticipated the need for and pushed custom circuit design,
12) terminated a thin film magnetic memory program,
13) led IBM into the adoption of semiconductor memories,
14) terminated two superconductivity programs, and
15) managed a number of successful machine programs.

Why did I list programs I terminated in a list of accomplishments? When I was staff director of the CTC, I got to thinking about my role and concluded I was doing a good job of nurturing IBM's technology garden, but I was not doing anything about weeding it. I expressed this thought to an important staff person in Armonk. He said the risk is too great to terminate a program, since a competitor may make the technology work and then you will be remembered for having killed it. I did not agree, so I told Piore of my thought. He asked if I had a program in mind. I knew Piore had a warm spot for the thin film magnetic memory program in Burlington, Vermont. With trepidation, I said yes, the thin film memory program in Burlington. Piore did not bat an eye, but he asked how would I go about doing it. I said since Pugh is very supportive of magnetics, I would have him head up a task force to review the program. Piore agreed.

The task force came back with the recommendation that the program should continue. However, Bill Simpkins, the manager of the program, having been questioned at length by the task force, recommended that the program be terminated. The magnetic thin film program was terminated.

At one time, I took a census of the IBM Fellows and found that approximately 14 percent of them had worked in one or more groups of which I had been the manager. I consider it a significant indication of the fact that I had, in an indirect way, been their mentor. It is my hope that I have in some small way, through my guidance of engineers and scientists who have reported to me, repaid Segler, my Brooklyn scoutmaster, for the kindness and guidance he had given me.

I retired from IBM in July of 1986 at the age of 65 after 35 years. Working for IBM was an exciting and sometimes frustrating experience. It is comforting to realize that I had made my mark in two very important and fast-moving technologies and industries.

References

[1] C.J. Bashe, L.R. Johnson, J.H. Palmer, and E.W. Pugh, *IBM's Early Computers*. Cambridge, Mass.: MIT Press, 1986. See chapter 10, section 5, entitled "Current-Switch Circuits."

[2] L.P. Hunter; *Handbook of Semiconductor Electronics*. New York: McGraw-Hill, 1956. See Section 15, pp. 29-67. In particular, Figs. 15-50 and 15-51.

Editor's Note

At my request, one of Logue's IBM managers reviewed this memoir and made these slightly edited comments.

> Joe was greatly appreciated and admired by the IBM management, although he was never given the credit he deserved. In many cases, nontechnical considerations were the reasons that decisions were made that seemed contrary to his technical recommendations. There is no question that he was one of the most technically perceptive minds in the last half of the 20th century. His insights and powers of analysis were of inestimable help during the glory years of IBM and the starting years of the electronic computer. He should be held in high regard by students of this historic era.

Joseph C. Logue is chairman of the board of Lorex Industries, Inc. Ray Logue, his son, is president of Lorex, which does contract development involving high technology for a number of large corporations. The senior Logue assists his son on technical problems when needed. Logue feels his greatest accomplishment has been successfully managing engineering and scientific groups engaged in high technology and machine development. He had the good fortune of being able to anticipate technological trends so that the groups under his command were always in the forefront of the technologies being developed. In addition, he is very pleased with the number of technical leaders he has had the good fortune to identify and develop. The preceding autobiography gives the details of his career. Logue is a member of the National Academy of Engineering of the U.S., a Fellow of the IEEE, and a Fellow of the American Association for the Advancement of Science.

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