Magnetic Cores

by

David R. Brown

Palo Alto

2011

Magnetic Cores

It was midsummer of 1951 when we left California, Sally, Phil, and Ellen flew to Seattle to stay with my folks while I went alone to Massachusetts. I found the Digital Computer Laboratory at the Barta Building much the same, but with more employees. It felt good to be back. I had taken a taxi from Logan Airport directly to the Barta Building and was greeted there by Harris Fahnestock.

I got a room on the top floor of the Graduate House just a few blocks down Massachusetts Avenue from the Barta Building. Besides my work for the Digital Computer Laboratory, I had two personal tasks; (1) find a place for the four of us to live and (2) buy a car. Both tasks were soon accomplished. The house I found was in a new development on a hillside in Arlington, 43 Woodside Lane. I also found a used car, a Chevrolet sedan at an Arlington Chevrolet dealer. I think I paid 700 dollars for it. When Sally, Phil, and Ellen arrived, I was ready. However, we needed to wait a few days for our furniture before moving into the Arlington house, so we spent some time in a hotel near Harvard Square.

43 Woodside Lane

Soon after I arrived, I had a conversation with Jay Forrester about my duties. He told me to read about the design of Whirlwind, because that knowledge would help me in whatever I did. I was given a place to work and documents to read, but I was soon bored. This was not what I wanted. However, I did meet some new colleagues and picked up on what was happening. I remember meeting Dave Israel, Moose Walquist, Bob Wieser, Jack Jacobs, Ed Rich, and others with whom I would later develop close working relationships.

Whirlwind was the fastest computer in the world at that time. It had been designed to work in "real time" as a part of a larger system (an aircraft simulator). It had been sponsored by the Office of Naval Research, but ONR was having difficulty in justifying the increased funds that were being requested by Jay Forrester. Justification came after 1949 with the cold war, when the Soviet Union developed the atomic bomb and had aircraft capable of delivering bombs to cities in the Unites States. George Valley, an MIT professor and chairman of an Air Force committee, the Air Defense Systems Engineering Committee (ADSEC), thought that a digital computer might be capable of processing data from air-defense radars and directing interceptor aircraft to shoot down the Soviet bombers. The idea gained support from others, including Jerry Wiesner. the associate director of the MIT Research Laboratory of Electronics. Wiesner knew about Whirlwind and suggested that the Whirlwind computer could do the job.

Help from MIT was requested in a letter from General Hoyt Vandenberg, Air Force Chief of Staff, to James Killian, President of MIT, in December 1950: "The air defense problem which faces the Air Force is of great importance to the people of this country. The problem is technically complicated and difficult. The Air Force must urgently increase its research and development effort in this area and in this we ask your help. I sincerely hope that you will be able to give this matter serious consideration."

High-ranking individuals in the Defense Department and at MIT, including James Killian, President of MIT, and Julius Stratton, Vice-President, realized the importance of an air-defense system and got behind the idea. One reason MIT was favored was because of MIT's experience with the Radiation Laboratory during WW2. In 1951, about the time I returned to MIT, a contract for an air-defense project was negotiated. MIT would be in control of the project, but the Army, Navy, and Air Force would participate in running it. MIT wanted the project to be located away from its Cambridge campus.

Forrester was ready and rose to the challenge. He had been a participant in many of the negotiations. He may have been waiting on the big contract between MIT and the Air Force before assigning me to my new task.

The project started as Project Lincoln, named after the town near or in which the project was to be located. Several years later, after construction of the new laboratory was underway, it was renamed Lincoln Laboratory, but the actual location turned out to be Lexington.

The Town of Lincoln became an important part of our lives. When we decided to move from our Arlington house to a better place, we were referred to a family in the Town of Lincoln who had a house for rent. They lived in the center of town on Lincoln Road where we met them. They rode with us in our car to the rental house. On the way, the woman in the back seat asked us if we knew anyone who lived in Lincoln. One of us said: "No," then I turned to Sally and asked: "What was the name of Harriet Blackburn's sister?" The woman in the back seat said: "I'm Harriet Blackburn's sister." The house turned out to be the one we had visited about 1947, on which I had nailed siding, when I was a graduate student at MIT.

We were delighted to be able to sign a lease for the house on Conant Road, owned by Jim and Marian Billings. We moved in around May of 1951. The Billings helped us during the move. At the end of the day as we were thanking them for their help, Marian asked: "By the way, what church do you belong to?" I replied: "I'm Unitarian, but Sally is Congregational." Marian said: "You have come to the right town, because we have one church that is both."

Not long after that, on a Saturday afternoon, Martha DeNormandie called on us at the Conant Road house to invite us to a meeting at the church, the First Parish in Lincoln. Soon we joined the church and were active members for twelve years. I became a Sunday school teacher and eventually a member of the religious-education board. Before we left town in 1963, I had served as a member of a search committee.

I especially remember being an usher in the winter, with snow on the ground. The church steps got the morning sun and were a nice place to stand and greet arrivals. Old Mr. Donaldson would stop and talk with me, telling me about his children and grandchildren, pointing to the houses where they presently lived as we stood on the steps.

The Little Gray House

The population of Lincoln in 1951 was about 2,000. The houses in Lincoln were spread out. The minimum size of a house lot was two acres, but the average was much greater. Our nearest neighbors on Conant Road, the Brownings, were about a quarter mile away. The next residents in the other direction on Conant Road, the Davises, were more distant.

Like most families at that time, we had only one car, so when I drove off to work for the day, Sally was left on her own with Phil and Ellen. She had time to explore and meet nearby residents on foot, with Phil and Ellen in a stroller. They soon found other families with children. (This was the era of the baby boom.) As a result of Sally's explorations, Phil joined an informal playgroup for preschool children. One mother would take the group for a morning, children being delivered and retrieved by a parent (usually a mother). I found that by sharing a ride with a neighbor I was able to leave the car with Sally a few days each week so she could take Phil to the playgroup when it didn't meet at our house. (I think my ride-sharing was more a result of Sally's contacts through playgroup mothers than anything I arranged on my own.)

My commute to the Digital Computer Laboratory at MIT was 14 miles, usually about 30 minutes. The route for my drive to the lab was not fixed, but could change with the weather, road conditions, etc. Most of the time it involved driving right through Harvard Square. I got to know all the back roads of Cambridge. During the winter, we could get a heavy snowfall before it was time to drive home, and that could be an adventure. (If the snow was too deep in the morning, the trip to the lab might be delayed.)

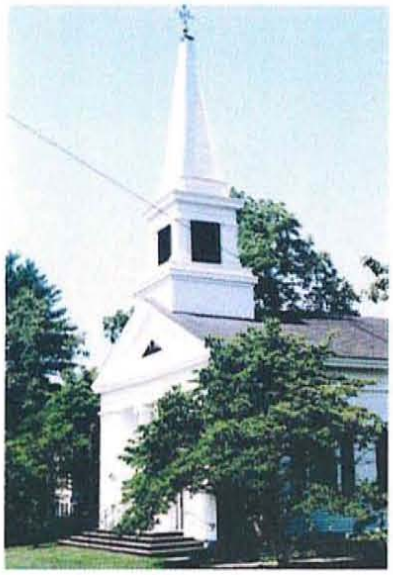
The Brownings, George and Margie, became good friends. They had children about the same ages as ours, as well as siblings and grandparents. All the Brownings lived in a farm house on Conant Road. The road passed within a few feet of their house, both the road and the house having been there since sometime in the 1700's, long before zoning regulations required a set-back for a house. George was the farmer who plowed their farm fields. He also kept a cow, raised chickens, and sold eggs on a regular egg route.

In the fall of 1952, Sally's sixty-four-year-old mother, Lola, joined our family in the Conant Road house. She arrived in Lincoln on a lovely fall afternoon with Uncle Carl and Aunt Cornelia, with whom she had been staying in Great Neck, Long Island. At the time, Sally was pregnant, Phil was four, and Ellen was something over one year old.

Lola soon became a member of our family. She got the best bedroom, at the end of the new wing of the little gray house. She would regularly go to church with us on Sundays. (See the snapshots taken on a Sunday morning as we were preparing to leave for church.)

Ellen and Lola

Phil, Ellen, and Sally



The White Church

We got involved in various aspects of Lincoln society, attending annual and special town meetings every year. In 1954, the town celebrated its bicentennial, with a ball in the school gym that culminated in a grand march. I wore a rented tux. An annual town meeting was held in March or April (after the snow that made road travel difficult had melted but before the ground had thawed) and was eagerly awaited by the citizens. Special town meetings were called as needed. I remember one called to authorize the purchase of water pipe to replace a broken section of pipe.

Lincoln was engaged in improving its public schools, hiring an outstanding school superintendent, highly-qualified teachers, introducing team teaching, etc. All this was costing more. At the same time, the Air Force was building houses and a school building in the Town of Lincoln for Air Force personnel at Hanscom Field. However, Federal law required that schools for military children, in such circumstances, had to be run by the local government, in this case the Town of Lincoln. The town would be compensated by the federal government according to a formula based on the actual costs of schools in nearby school districts. The Town of Lincoln figured this arrangement would cost the town much of the extra money it was putting into its own schools, if the federal formula applied. The issue became a topic of debate in town and was the subject of at least one town meeting. It was resolved by a motion from a citizen at a town meeting; "Those Air Force kids deserve as good an education as our own kids. I move we send a delegation to Washington to change the law." The motion was passed and the federal law was changed.

I became a member of the Democratic Town Committee, for a time as treasurer. Other members of the committee were Bill Langton and Hank Morgan. Democrats were a minority in Lincoln. I stayed up during several election nights working as an official observer. Sally became more engaged in Lincoln politics, serving as a member of the School Building Needs Committee and as a founding trustee of the Lincoln Land Conservation Trust. The results of her work for the Trust can be seen throughout the town today.

When he was about five years old, Phil would accompany me on walks through the nearby woods where we would find fallen trees or tree limbs to drag home to cut up for firewood. We rarely saw any other people. However, during the weekends in1953 we observed groups of people exploring these same woods. They were families, mostly from the MIT Instrumentation Laboratory, who had joined to build a utopian community. They ultimately purchased some 40 acres right next to where we were living on Conant Road and divided the land into 23 lots. Their new community was called Brown's Wood (no relation). Each family designed (or had designed for it) then built an attractive home. We got to know most, if not all, of the families and our children grew up playing with their children. The Brown's Wood community, at first withdrawn into itself, was gradually assimilated by the Town of Lincoln.

While all of this was happening, the foundations for my work at MIT during the next ten or more years were being laid.

Experiments, called the Bedford tests, had been undertaken with the cooperation of other organizations in New England to demonstrate that Whirlwind could track target aircraft and direct an Air Force fighter to intercept the target. These experiments required the cooperation of

many organizations, including the Whirlwind team and other groups at and around MIT in the fields of radar, communications, flight testing, etc. By March of 1951, the Bedford tests had demonstrated that a computer could direct an interception.

I had arrived in the summer soon after that and had missed the excitement. Jay Forrester attached considerable importance to these Bedford tests as the first significant demonstration of real-time control using a digital computer.

The Bedford tests were followed by tests on a larger scale, the Cape Cod System, with the Whirlwind control center operated by Air Force personnel in the Barta Building.

Jay Forrester was keenly aware that computer memory for digital computers was a problem. He had supported electrostatic storage as a possible solution, and electrostatic-storage tubes were used in the beginning for Whirlwind. However, they proved to be costly and unreliable. During the summer of 1949, Jay took time off from work and, during long walks in the woods, thought about a computer memory based on magnetic cores. He recorded his thoughts in laboratory notebooks and by the end of the summer had described a new form of computer memory, the coincident-current memory.

When Forrester got back to work, he assigned the investigation of his invention to a graduate student in the lab, Bill Papian. Bill would stay with the development of the coincident-current memory for several years. When I first met him he had built at least one, small (2-by-2) ferrite-core memory and successfully demonstrated it. He presented a paper on it at the 1950 IRE National Convention.

The new MIT Faculty Club on the sixth floor of the Sloan Building (that had been a Lever Brothers building) opened and Jay advised members of his staff to become members. Not everyone did. Lunch was nice because one could show up and sit at a round table with others, perhaps meeting them for the first time. I remember lunch with Norbert Wiener. Later, the Faculty Club became a favorite place for Sally and me to go out for dinner.

In the fall of 1951, several of my colleagues decided to improve their ballroom dancing. Sally and I joined the group of five or six couples, including Jay and Sue Forrester, and met regularly in one of our homes with an instructor for dancing lessons.

In September, the Digital Computer Laboratory was established, previously a part of the Servomechanisms Laboratory.

In December 1951, Jay told me that he wanted me to find the best magnetic material for the magnetic cores of his coincident-current memory. I would have whatever resources I needed and I should not be limited in the scope of my investigation. I should consult the best minds in the field for advice and hire whatever personnel were needed.

Some time after that I learned that my new group, the Magnetic-materials Group, would have a laboratory in the Whittemore Building, an old shoe-polish factory on Vassar Street. I would become a Group Leader, one of four or five groups under Jay Forrester and Bob Everett. Other group leaders were Norm Taylor, Bob Wieser, and Steve Dodd (for the vacuum-tube group). Harris Fahnestock and John Proctor, administrators, usually attended the weekly meetings that began in March 1952. The Group Leaders met weekly, typically on a Monday morning. I was appointed the secretary of these meetings. (The minutes can be found on-line in the MIT Whirlwind Archives.) In June 1952, my group moved from the Barta Building to the Whittemore Building, occupying an entire floor.

As early as 1952, Forrester and others (including Bob Everett, Norm Taylor, and Bob Wieser) began thinking about a company that could build the computers that would be required for the air-defense system (that eventually became known as the SAGE system, for

semiautomatic ground environment). They looked at the Bell System, IBM, RCA, Raytheon, and Remington Rand and, after a number of exchange visits and negotiations, chose IBM. By the end of 1952, IBM had a contract with MIT to build the SAGE computers. IBM established Project High, its name for the IBM effort. IBM engineers worked closely with the MIT team. Many visited MIT Lincoln Laboratory, as did many of the MIT engineers visited Project High. At east one IBM engineer, Jim Crowe, worked for an extended period in my group. At first, I would take the train between MIT and IBM in Poughkeepsie, but later IBM got a small airplane, with pilots, to fly us between Bedford Field and Poughkeepsie. Sally would sometimes meet me at Bedford Field as the small airplane (an Aero Commander) returned from Poughkeepsie.

Lincoln Laboratory

IBM followed us in magnetic-core development. I introduced them to General Ceramics and encouraged them to get involved, especially in the testing of cores, where we really needed help. Forrester pressed IBM to develop a second source, after General Ceramics, as a source of cores for the air-defense computer. However, IBM was satisfied with the electrostatic-storage tubes that it was making for its commercial computer, the 701, and saw no reason to develop the coincident-current memory. IBM did undertake a core development effort in 1953 in support of Project High. IBM eventually Switched to magnetic-core memory for their mainline commercial computers such as the 701.

Jay held a more-or-less formal tea in his office almost every Friday afternoon, with tea and cookies. Attendees were invited, the leaders of the division. The discussion was not the same as it was at the group-leaders meetings. It was relevant but less formal, with reports of visitors, happenings of general interest, computer developments elsewhere, etc. Minutes were not recorded.

My group probably had ten or twelve members initially. They included two secretaries, three or four technicians, and seven or eight professionals (staff members or graduate students). The group grew to an average staff size of twenty. The room on the top floor of the Whittemore Building was spacious, accommodating six or more lab benches and small offices around the periphery of the room. My office was in a corner overlooking the railroad tracks. In the summer, when the windows were kept open (no air conditioning), I had to dust my office when I came to work in the morning to remove the cinders from the surfaces of my office furnishings. (The freight trains burned coal).

Building my staff was an important part of my job. Some could be hired as MIT staff members (non-academic employees of MIT) whereas others came on as graduate students, like Dudley Buck. I had to go outside the usual pool of engineers, because we needed physicists and chemists in addition to electronics engineers to develop new magnetic materials. MIT, of course, was an ideal place to find the talent we needed. I was told about a chemist (an expert on ceramics) who appeared to be just right for the group. To get him I would have to offer more than I was making, so I asked Jay about it. He said to go ahead, so I made Frank Vinal an offer and he joined the group as a key player.

When I was looking for a suitable physicist, I attended a national meeting of the American Physical Society, probably in New York City. A recent PhD, John Goodenough, was talking on a subject that was relevant. I went to hear his paper and the discussion that followed, and offered him a job at MIT. He accepted and became an outstanding member of our group. While at MIT, he and his wife became "house parents" in one of the MIT dorms. He eventually served as a professor at the University of Texas.

When the search for a suitable magnetic material was just getting started, two materials looked promising, metal-ribbon cores and ferrite cores. The metal-ribbon core had the right magnetic characteristics, but because it was metal, eddy currents slowed its response. Ferrites, being ceramic, were not slowed by eddy currents, but their magnetic characteristics were not quite right for the coincident-current memory. With Dudley Buck and Norm Menyuk, I wrote a paper, "A Comparison of Metals and Ferrites for High-speed Pulse Operation." The paper was accepted by the American Institute of Electrical Engineers for presentation at meeting in New York City. Later, it was judged to be a significant paper and was published in the *Transactions of the AIEE.*

In 1951, a comprehensive book on ferromagnetism by Richard M. Bozorth, a Bell Labs scientist, was published. I arranged to visit him and get his advice. He was very cordial, taking me to lunch at Murray Hill and showing me his laboratory.

I learned that MIT had a long-established laboratory, the Laboratory for Insulation Research, under Professor Arthur von Hippel, that included expertise in ceramics. I found a scientist there, George Economos, who was interested in ferrites and was willing to undertake some research in support of our program. I arranged to have money sent to his lab and he set out to make ferrites for the coincident-current memory. He built a small, electric kiln and proceeded to fire some ferrite samples. When I made overtures to George Economos to draw him into our team under Jay, I got a sharp rebuke from Professor Von Hipple. He explained to me that the scientists in his lab did not work as a team, but each pursued his own research interests, working in private. When one of von Hipple's scientists was ready, he would share his research by way of publication in a technical journal, not before. This came as a shock to me because it contradicted the culture I had gown accustomed to in my brief career.

At that time, I was working with a company in Butler, Pennsylvania, Magnetic Metals, to develop the metal-ribbon (molybdenum-Permalloy) cores. I made several trips to Butler, and ordered hundreds of metal-ribbon cores, but they looked less and less promising as time went on. Before long, the search for a metal-ribbon core was abandoned.

Ferrite cores, on the other hand, steadily improved. The General Ceramics & Steatite Corporation, in Keasby, New Jersey, was very interested in developing ferrite cores. The company was in the business of making chemical stoneware, such as might be used in making beer, but in recent years had been making ferrite parts for radio and TV applications. I worked with Chris Snyder, a General Ceramics VP, and others, including Ernst Albers-Schoenberg, the General Ceramics chemist. He didn't know much about magnetism, but had an uncanny ability to modify the magnetic characteristics of ferrites in the desired direction by changing the chemical composition of the ferrite.

At General Ceramics, ferrites were fired in an old, gas-fired, Dressler tunnel kiln. It must have been twenty feet long and at least twenty years old. Our ferrite cores would travel the length of the kiln on a cart along with all the other jobs at General Ceramics. When I told Jay about this arrangement, he became concerned because of the uncontrolled conditions I described. He asked

me to arrange for a thermocouple to be put on our cart so we could get an accurate reading of the temperature. I never was able to fulfill his request.

I became a frequent visitor at General Ceramics (about once a month). By the end of 1952, General Ceramics had a contract with MIT. I set up test equipment in Keasby and became acquainted with personnel there (Ernst Albers-Schoenberg, Frank Gelbard, Bill Olander, and Abe Katz). I would take the train, "The Owl," out of Boston, get up in Penn station, then take the shuttle to Times Square, and then the subway to Penn Station, where I would get on a train for Metuchen, New Jersey. Chris Snyder would meet me at Metuchen and drive me to the General Ceramics plant. The plant was just beyond the Carborundum plant on the bank of the Raritan River, where the river empties into Raritan Bay.

On the road near the Carborundum plant was a row of identical, small, single-family houses. One morning I was surprised to see newly dug trenches running from the road to each house. I asked Chris: "What was going on?" He replied that water was being provided to the houses. This was 1952!

For my return to Boston, I would sometimes take the "Merchants Limited" that left Grand Central at five o'clock. I could get a seat in a parlor car; enjoy a drink in the club car, and dinner in the diner. If five o' clock was too early, I would the take the overnight train that got me back to South Station in Boston before breakfast.

In the meantime, my group in Cambridge was working. Frank Vinal made plans for a ceramics lab in The Whittemore Building so that we could fabricate our own ferrite cores. When he realized that the group would be moving in a year or so to the new Lincoln Laboratory buildings in Lexington, he hesitated. I went to Jay and asked him if we should go ahead with expensive plumbing in the Whittemore building when we knew we would soon be moving to Lexington. Jay said that what we were doing was important. Get on with it!

Polly was born at Mount Auburn Hospital in Cambridge on December 30, 1952. When Sally returned to Lincoln with the baby, she found Phil and Ellen ill with chicken pox. Polly got a mild case also. We were fortunate to have Lola there to help with all the domestic chores. Everyone recovered without any complications.

Later, Sally became concerned for her mother, who was a wonderful companion for the children, but lacked any outside interests or activities. Sally set out to fix that. Through a friend at church, Florence Hollingsworth, she learned about classes in arts and crafts at Lincoln's DeCordova Museum. She learned that Florence taught silversmithing and talked her mother into signing up for a class. Lola took to it and soon became an enthusiastic silversmith. We still have some of her beautiful pieces; one is a serving dish. I knew Florence's husband, Lowell, at work and at church, an engineer from Stanford who became the director of the nearby Air Force Cambridge Research Laboratory, known locally as AFCRL.

I learned later that Lowell Hollingsworth was a classmate of David Packard and William Hewlett. Hewlett, Packard, and Hollingsworth together published a technical paper on the RC oscillator (in the *Proceedings of the IRE*). Hewlett and Packard later went into business with their first product, an RC audio oscillator. I got to know their oscillator very well during WW2 at the Applied Physics Laboratory of the University of Washington, where we had many of the HP audio oscillators in the lab and I used one almost every day.

Sandy Pond from the DeCordova

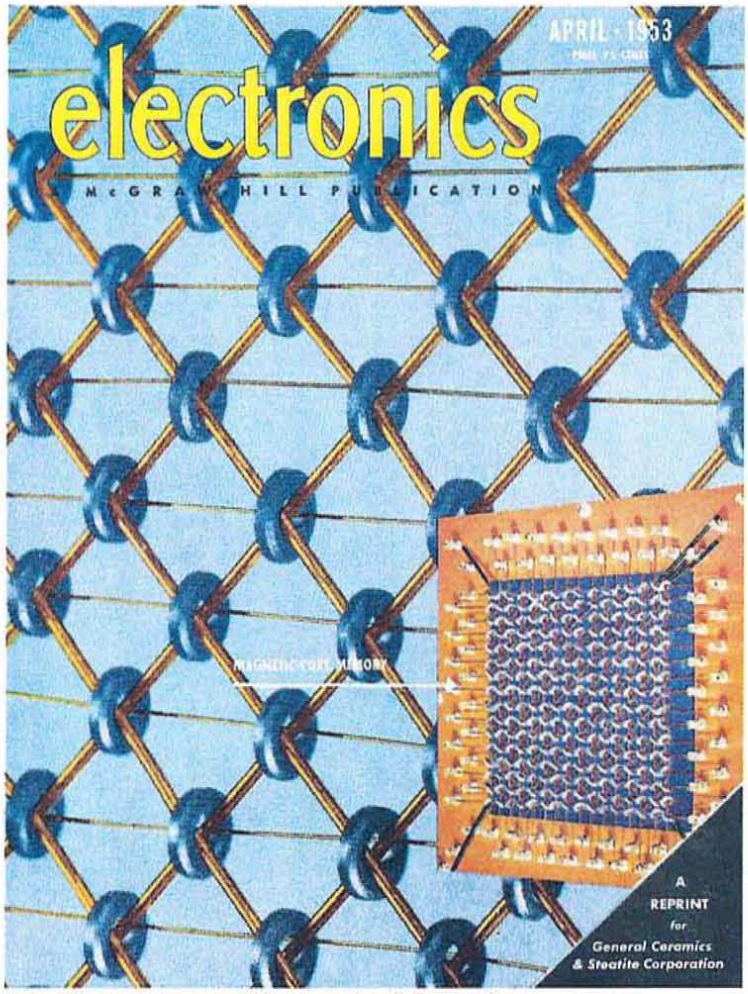
The DeCordova is one of Lincoln's many cultural assets. The town-owned museum, with a 35-acre campus, sits on top of a hill overlooking Sandy Pond and is surrounded by sculpture gardens. It is one of New England's premier art galleries, and also a site for lectures, concerts, and fairs. I took drawing classes there myself. When I was in a life drawing class (with nude models) other students in the class included two famous Lincoln residents, Thomas Adams and John Quincy Adams. I got to be a pretty good sketch artist.

The DeCordova, not far from Lincoln Center on Sandy Pond Road, was a place to go with the family on a Saturday or a Sunday, but I attended classes there on weekday evenings. One year I was involved in designing and operating a booth for a DeCordova fair

Another place for the family to go in Lincoln was the town library. We sometimes went there as a family on a Saturday night. (There wasn't much else to do.) The library was located right at the center of town. Sally's mother eventually took a job there as a volunteer librarian.

The electrostatic storage tubes being used in Whirlwind were proving to be too unreliable for the air-defense application. Whatever form of memory replaced it would require extensive testing before it could be installed in Whirlwind. Plans were laid to build a computer in the Whittemore Building much like Whirlwind, to be called the Memory Test Computer (MTC). It would be used to test the new memory before it was installed in the Whirlwind computer in the Barta Building. By this time, Whirlwind was being used for real-time testing of aircraft interceptions, work that could not be interrupted. Whirlwind could not run air-defense tests and at the same time check out a new memory.

In November 1952 we received some ferrite cores from General Ceramics that looked good, good enough for us to drop any expectation of using metal cores. This was a significant development. Forrester's quest for a satisfactory high-speed memory was close to being realized. A push ensued to get a sufficient quantity of tested, good cores for building the memory for MTC. An urgent effort was supported at General Ceramics and MIT to fabricate and test the thousands of magnetic cores that would be required. Several automatic core testers were designed, built, and operated in my group to handle the large number of cores required. By February 1953 we had finished testing 20,000 cores for MTC. By May of 1953, MTC was operating with 1,024-core arrays of ferrite cores. Later, the MTC memory was expanded to 4,096-core arrays, and moved to the Whirlwind computer in the Barta Building. Whirlwind operated with a core memory for years afterwards.



***Electronics* Magazine**

In April 1953, *Electronics* magazine ran a story about the magnetic-core memory with a color photo of an experimental memory plane on the cover. The feature article was "Ferrites Speed Digital Computers," by David R. Brown and Ernst Albers-Schoenberg.

By November 1953, my group was making good cores of its own design. A pilot plant for fabrication of memory cores began operation in October 1954 and qualified as a second source in December. My assigned objective had been met.

Other makers of magnetic cores were supported by my group, including RCA Victor in Camden, New Jersey. I visited the RCA Victor plant in Camden several times and set up test equipment there. The RCA folks were slow to develop a satisfactory magnetic material, but I think we eventually tested some cores from RCA that were acceptable. Also, the Laboratory for Insulation Research at MIT, with George Economos, was able to fabricate acceptable cores.

Around July of 1954, we in Division 6 of Lincoln Laboratory moved from the Barta Building to the new Lincoln Laboratory Buildings in Lexington. Whirlwind stayed in Cambridge and continued to operate, at that time as a part of the Cape Cod System, but also as a computer resource for MIT.

The new laboratory was just a few miles from our house on Conant Road in Lincoln, but too far to walk. Sometimes Sally would drive me to work or pick me up at the end of the day.

Later in 1954, my duties at Lincoln Laboratory were expanded. Jay Forrester defined a new set of tasks for his division of Lincoln Lab, Division 6. Group 63, the Magnetic Materials group, became Group 63, Magnetic Materials and Advance Development. My group would have two principal activities: (1) basic research on magnetic materials for digital-computer applications and (2) the development of new components, circuits, logical design techniques, and system-design concepts to improve the reliability of digital computers for AN/FSQ-7-type applications.

The transistor, invented at Bell Labs in 1948, had the potential for replacing the troublesome vacuum tube as a digital-computer component. Finding or developing a suitable transistor for digital-computer applications became one of my major goals. Around this time, Lincoln Lab received an invitation from the Philco Corporation, an electronics company in Philadelphia, to send a delegation to Philco and learn about their work. Several of us at Lincoln accepted the invitation to visit, including Torben Meisling from my new group (the magnetic materials and advance development group). Bob Rediker, from another division of Lincoln Lab, also was with us for the visit to Philadelphia.

We spent several days in Philadelphia and learned about a number of programs at Philco, including a computer project in which Jim Angell was involved. (He later became a professor of electrical engineering at Stanford, where he also served for many years as the university carillonneur)

During one of our meetings with our hosts at Philco, a manager there, Carlo Bocciarelli, introduced two of the young engineers working for him, George Messenger and Bob Noyce. Bob Noyce spoke about the surface-barrier transistor, a new development at Philco. I was very impressed. This transistor seemed to be very close to what I was looking for. Eventually, I negotiated a contract with Philco to develop the surface barrier transistor for use in digital computers. The subcontract spanned a period of several years during which I made repeated visits to Philadelphia. Carlo Bocciarelli also visited me at Lincoln Lab.

Bob Noyce left Philco in 1956 to join the Shockley Semiconductor Laboratory in Mountain View, California. He moved to Fairchild Semiconductor in 1957, then to Intel in 1968 where he led that company to become the world's major semiconductor manufacturer. He became known as "The Mayor of Silicon Valley." I met him a few times in California, once while working for the annual MIT Alumni Fund. He died in 1990.

During my early conversations with Bob Noyce, I learned about his vision to integrate all of the electronic components for a digital circuit onto a small, silicon chip, i.e., an integrated circuit. I remember advocating the integrated circuit with my colleagues at Lincoln Laboratory. Years later, integrated circuits and microchips became the standard for the industry.

Around this time, my group organized to design and build a new digital computer using surface-barrier transistors. I created several sections in the group: (1) logical design, (2) circuit development, and (3) computer fabrication. Wes Clark led the logical design and Ken Olsen was put in charge of the fabrication of the computer. (My boss, Jack Jacobs objected to my appointment of Ken Olsen, but my decision prevailed.) We debated over a name for the new computer. I favored "Lexington Minuteman." Jay correctly vetoed that name and recommended something less stodgy. Eventually, we settled on "TX-1." When we got to the budget for the project, we found that it had to be scaled back. The name became TX-0.

The new computer, TX-0, although built on a reduced budget, was a success. It became a popular educational research tool and was later turned over to the engineering school for use by students.

Associated with this expansion of my duties, a small research group located in Cambridge was created, the Building 10 Research Group. Bill Linville, a professor of electrical engineering, and I shared responsibilities for the leadership of the group of ten or so graduate students. (Building 10 was at the center of the MIT building complex, the building under the big dome. It housed the headquarters of the electrical engineering department.)

In late summer of 1954 we were hit by Hurricane Carol. I was at Lincoln Lab that day. We lost power all around and a batch of memory cores that was being fired in our pilot plant. One of the administrators at Lincoln Lab was concerned that he couldn't get power to an aircraft-warning light on one of the towers by the Lab.

When I drove home that day, I had to go around fallen trees and across private property in order to get home. Fortunately, our house was unharmed except for the loss of power. Our well had an electric pump, so we were without water for a few days, until I was able to borrow a gasoline engine from Bob Wieser's lawn mower. I could run his gas engine for a few minutes at a time to fill our water tank. Repair crews from as far away as Ohio worked in our neighborhood and after ten days the power was restored.

Ann was born 9 June 1955 at Mount Auburn Hospital in Cambridge. I was allowed to be with Sally before she was taken into the delivery room, a new experience for me. It was a warm summer evening when I drove back to Lincoln to tell Lola and the children all about it: "It's a girl!"

Earlier that year I developed the idea that we should all go camping. It was the only kind of vacation that we could afford. Neither Sally nor I had ever done any camping, so we had to learn. During the winter months, I studied catalogs and "how-to" articles about camping. We ordered an umbrella tent, sleeping bags, camp stove, ice chest, etc., from Morsan Tents in New York. Neighbors, the Stritters (I ride-shared to Cambridge with Ed Stritter), told us about a campsite in North Conway, New Hampshire, that would be good for novice campers, so that was where we went. Friends of the Stritters, Meg and Les Brown, had property in North Conway, with a swimming pond, a small campground, and a shop called the House of Color. It was ideal for us.

Ann was two months old on our first camping expedition. When Ann was a few days old, Sally came down with mastitis. Sally had to switch Ann from breast milk to bottles. At first, we thought we had to cancel our camping plans. However, we figured we could do it, sterilizing bottles on the camp stove. We all managed the expedition just fine, and at the end were no longer novices. Camping became our regular summer vacation and we returned to North Conway each year for several years—until we discovered Cape Cod and Martha's Vineyard.

Polly, Sally, Ann, Ellen, and Phil

Sally, Ann, Polly, Phil, and Ellen

Our growing family began to feel the need for a larger place to live. We enjoyed living in the little gray house on Conant Road, but realized that we needed a larger place. We investigated many alternatives, including building a house in Lincoln. Grandpa Browning, our next-door neighbor had plenty of land but was not interested in selling. However, our neighbor in the other direction down Conant Road, Grandpa Davis, had divided his land into building lots and was willing to sell. We paid $4,000 for a two-acre lot. It was on an old, unpaved, abandoned road, Old County Road. The Town Selectmen were reluctant to approve the lot as a residential site, but our attorney, Harlan Newton, found that the road had been on the books since the 1600's and we met all the legal requirements. The lot was on the edge of a 200-acre swamp, but it included a small spring that Grandpa Davis told us about. When we built our house on the lot, we had a shallow well build on the site of the spring. It supplied us with all the water we needed.

We talked with Lucy and Gene Rappaport, who lived down Conant Road in one of Grandpa Davis's farmhouses, about our search for a place to live. Gene was an engineer and Lucy had just graduated from Harvard with a degree in architecture. Lucy was interested in designing a house for us. Since it would be her first house she would do it for a reduced fee. We agreed. We couldn't afford a general contractor, but Sally volunteered to serve as the general contractor if I would be her assistant. I agreed to that and we were lucky to find an excellent lead carpenter. We couldn't have done the job without him. I think our budget for the construction of the house, with six bedrooms and three baths, was $20,000. We forgot to include the millwork in our budget and consequently were $2,000 short of the money we needed to complete the construction. We moved into the house in 1958 before it was entirely finished. Gene and Lucy bought the lot next to our lot and built a nice house for themselves and their children.

The new house included a suite for Lola on the lower level. It had its own plumbing and heating system.

My job at MIT changed about this time. Division 6 of Lincoln Laboratory became the Mitre Corporation. Although the name of my employer changed, nothing else did change— for awhile.