J. Logue

March 30, 1970

Memorandum to: Mr. L. M. Saphire

Subject: Interview TC-68 in the IBM Oral History of Computer Technology

I have finally found time to edit the transcript of the interview we recorded on April 2, 1968. After rereading the transcription, I concluded that it would be more productive if I summarized what appear to me to be the key events in computer technology over the past 15 to 20 years. I think you will find this will make a much more coherent story than what would result from editing the transcript of a question and answer session. Since you are interviewing many other people, I will limit my comments to those events in which I was personally involved or in which people that reported to me were involved.

Defense Calculator

I left the academic life of Cornell University as Professor of Electrical Engineering to join what I considered at that time to be a coming field, computers. In May 1951 I joined IBM to work on the Defense Calculator which later became the 701. Two interesting problems arose, that had to be solved to make the machine a success. Naturally, there were many other problems that demanded solutions. As I have said, I will limit my remarks to activities in which I was personally involved.

The first of the problems we were faced with was improving the read-around-ratio of the electrostatic memory. Very briefly, this involved how many times memory addresses surrounding a given address could be interrogated by the electron beam of the cathode ray tube before the information at the address in question would be destroyed. We were striving for, as I recall, a read-around ratio of 350 or more. This meant that the addresses surrounding a particular address could be interrogated a total of 350 times before the information at the given address had to be regenerated or it would be destroyed. By experimentation I was able to show that the read-around-ratio was directly dependent on the stability of the deflection circuits which were in turn dependent on the stability of certain power supplies that powered the memory. After re-designing these power supplies I was able to achieve the required read-around-ratio.

The second problem became known as the indian blanket problem because of the characteristic pattern of ones and zeros displayed on the face of all 36 cathode ray tubes. The problem was intermittent and frustratingly impredictable. The memory would work fine for hours and instantaneously lose its information causing the 701 to come to a halt until the memory was reloaded. This problem persisted, as I recall, for two weeks. One Friday afternoon Jerry Haddad told me he had heard that I thought I knew what was causing the problem and what had to be done to fix it. I told him I felt sure I knew what was wrong but had not been able to try my idea because I could not get the use of the 701. He suggested that we come in Saturday morning and try out my idea. I was able to show Jerry that a noninductively wound feedback resistor only had to change value by .05% suddenly and it would cause the indian blanket problem. The following Monday I replaced the guilty resistors and the problem was cured. Jerry asked me to redesign the deflection circuits to eliminate that type of resistor, which I did.

Point Contact Versus Junction Transistors

After completing my work on the electrostatic memory for the 701 I was placed in charge of a small group of engineers that had been developing transistor switching circuits using point contact transistors. This was about December of 1952. Since I knew nothing about transistors I proceeded to learn all I could and build circuits at the bench. Within one or two weeks, two important problems became obvious to me:

1) The point contact transitor had very serious limitations. Their current gain was respectable only when transformers could be used which limited them to pulse logic. Finally I felt they would always be unreliable because of their construction. ?

2) Minority carrier storage could seriously limit the turn off speed of a transistor.

To correct the first problem, I took Bob Henle and Carter Dorrell off of the Small Accounting Machine model that they had been trying to build for some time using point contact transistors. After one false start I assigned Bob Henle the task of developing a family of logic circuits using junction transistors. By June of 1953 we had such a family of logic circuits working.

I solved the second problem by inventing a circuit that clamped the collector of the transistor out of saturation. This was accomplished within a few weeks after taking over the direction of this circuit development group. I make this comment because we were at that time still working with point contact transistors. Several years later Warnock of Philco addressed the minority carrier storage problem for junction transistors and arrived at a solution that was dependent on some of the claims in my patent when it was properly written.

New Technology Education

In the summer of 1953 Ralph Palmer asked me to structure and direct a course to train 28 engineers in transistors and cores. The course I put together consisted of three weeks of classroom work and six weeks of laboratory work. During the six week period I split the class into six groups and had them lay out on paper six machines using the knowledge they gained during the three weeks of formal classroom work. The course was known as Logue's College of Digital Knowledge.

At the termination of this course I recommended to Ralph Palmer that three of the machines be built. He agreed and the three machines built were the Small Accounting Machine, the Data Transceiver, and a transistorized 604. My group supplied the circuit designs for these machines. Since we had been working with Ed Lorenz and his people we had demonstrated that printed circuit cards could effectively be used to package the circuits.

With the exception of wire wrap, current switch circuits, and transistor driven core memories the machines that were built as a result of this course demonstrated all of the concepts contained in the SMS program out of which was built the 7000 series of machines. It must be remembered that this was all accomplished in 1953 and two thirds of 1954.

Blue Sky Activities

In the Spring and Summer of 1954 Ralph Palmer asked me to have my group look at blue sky ideas. These were far out concepts that would hopefully anticipate things to come. We engaged in this activity plus assisting the three machine groups described above. To keep my group productive I was forced to develop ideas that were far out at that time. Several that come to mind are:

1) Semiconductor equivalent of the gas tube counter. The idea I had was to use the double base diode structure in a bar of semiconductor material in such a way as to permit it to have 10 stable states. Input pulses would shift the position of an ON state to an adjacent position and reset the starting position to the OFF state. I assigned a summer student whose name was Rick Dill to this idea and before the summer was over he had demonstrated a device with 4 stable states. Rick went on to get his PhD. and is now working in Research.

2) A one dimensional image disector consisting of a bar of germanium with ohmic contacts at each end and an alloy junction collector at one end near one of the ohmic contacts. In operation, an image of a character was projected onto the germanium bar for a short time. A voltage pulse was applied across the ohmic contacts to sweep out the carriers generated by the image of the character. The carriers were collected by the collector and the collector current was a time function that represented the spacial distribution of light falling on the germanium bar. I had Kenny Sih work on this idea and he demonstrated a working model.

While all of this was quite interesting it was at the same time very frustrating for three reasons:

1) We did not have our own semiconductor laboratory. John Little naturally objected to our using his furnaces for diffusion.

2) It became clear that silicon might have properties that would open up other possibilities. Since there was no silicon technology within IBM at that time there was no equipment that would handle silicon.

3) The work we were doing was very far out and it was difficult to get the type of person that can be productive in that type of environment.

Silicon versus Germanium

In retrospect I must say that while the blue sky work was very frustrating it convinced me that silicon would be the semiconductor of the future. Another point that may or may not be valid is that if we had been able to get the equipment to work with silicon we may have been led to the invention of integrated circuits. We were certainly nibbling at the edges of the concept. It is a fact that in 1955 I tried to get Lloyd Hunter and his people to work on silicon. One of the reasons I gave for my interest in silicon was the possibility that the surface could be oxidized thus forming SiO_2 which could passivate the surface. This concept is very fundamental to integrated circuits as is the idea of performing complex operations within a single piece of semiconductor.

MAC Program

In January of 1955 my group was asked to design transistor circuits for the Modular Accounting Calculator program. We were given three months in which to come up with a circuit manual. At the end of one month Max Paley said he had to have the manual within one month. As I recall we met the schedual with a crude manual. During 1955 we were giving the people on the MAC program circuit designs before the ink was dry on the paper. It must be mentioned that in 1953 and 1954 my circuit group consisted of George Bruce, Ray Emery, Carter Dorrell, Jim Walsh, Bob Henle and myself. I transferred George Bruce to the 604 group in early 1954 because I had had him developing transistor circuits for use with core memories and I wanted that machine to incorporate a core memory into its design. Unfortunately, this did not happen. Sometime in 1954 I transferred Kenny Sih into my group and Rick Dill worked as a summer student.

608
(604, see Henle.

In January of 1955 my group increased from about 6 people to 18 with the 12 new people having no knowledge of transistor circuits. To get a circuit manual out in such short time with new people was very difficult.

In addition, we had only a handful of samples of the Raytheon junction transistors that were to be used in the MAC program. Our semiconductor people could not help us with specifications because they were still working with point contact transistors.

MA

With all the problems I had at that time, I can't understand why I also took on a contract with our military group in Endicott to develop six servo amplifiers using silicon transistors. These amplifiers had very tight design specifications. My reasons at the time were two fold:

1. My conviction was that silicon would be the semi-conductor of the future and we had to get some experience with it to see if that conviction was well founded.

2. I was also convinced that my group had to be knowledgeable in technologies that would be fundamental to the application of computers to the process control field. I felt that by selecting the proper contracts, my group could learn what was needed for future process control activities.

In late 1955, Max Paley reversed the assignments of George Hawkins and Dick Richards. Previous to this reversal, I had been dealing with George Hawkins who had responsibility for the hardware of the 608 machine. Dick Richards had responsibility for the system design. When their jobs were reversed, Dick Richards took the position that the circuits my group had been designing were no good because they were unreliable due to poor noise tolerance. Needless to say this created a large flap. A number of circuit groups in Poughkeepsie and Endicott proposed 10 and 12 volt circuits as an answer to this apparent problem. I took the position that Dick Richards was all wet. There was so much emotion and so little fact that I pulled my people off the program and assigned them to developing circuits with lower voltage swings than the 5 volts used in the 608. My reason for going to lower voltage was that this was the direction in which to proceed if higher circuit speeds were to be achieved.

To the best of my knowledge, the 608 machines were delivered with the same circuits we designed. I do know that one or two years later Lloyd Hunter accused me of overdesigning the circuits because the 608 machines were too reliable in the field.

STRETCH

In March 1956, I was given responsibility for the hardware design of Stretch. I reported to Steve Dunwell who had overall responsibility for the program. I recall I had my circuit designer's work during the regular two week summer vacation that year. We measured the characteristics of the transistor that our semiconductor people were proposing to use to determine the best operating point for maximum performance. To no one's surprise, it turned out that best performance, in terms of cut-off frequency, would be obtained if the device were operated in the conduction region. I instructed my people to come up with a design for a logic circuit that would keep the transistor in its active region and at the same time perform logic. This was a new concept and it departed significantly from the approach I took in the end of 1952 to keep a transistor out of saturation. Bob Henle gave structure to the concept and Hannon Yourke worked out the details. It involved using complementary symmetry devices, namely PNP and NPN transistors. This was not a new concept for us since we had worked with it earlier.

As a result of this analysis and invention, I posed the following alternative to John Little and Bob Swanson who were to develop the high speed transistor for Stretch:

1. If we were to use one type of transistor then we would require a transistor that had a low saturation drop and come out of saturation fast.

2. If saturation specifications were too difficult to meet, then we would need both an NPN and a PNP transistor with no saturation specification. They decided in favor of providing complementary types of transistors.

Before we had fully developed the current switch circuit and certainly before we had a firm transistor design, I told the systems designers that I would commit to a performance of 20 nanoseconds delay per stage with reasonable fan in and fan out. I had to develop this definition since Gene Amdahl and Steve Dunwell were talking in terms of a ten megahertz clock in the machine but were then being very loose in specifying through how many stages a signal would have to propagate in a machine cycle. I felt that it would be possible in the development time we had available to achieve the performance I had defined. I fully understood, and so stated, that my predictions were based on anticipated invention. I felt we could not build a superior machine if we did not count on inventing our way out of certain problems. It takes a finely tuned intuition to know what is doable and worth doing and what is not doable within a certain time frame, when one is dependent on new technologies and new inventions. It is on this point more than any other that Steve Dunwell and I disagreed.

The SMS Program

In March of 1957, I was asked by Bud Beattie to head up a circuit group to develop the circuits and package for what later became the 7000 series of machine, including Stretch. There was a great deal of motivation for standardization with the intent of keeping the part numbers to a minimum. Ed Wyma and Newton Noell were developing a package called the "Bird Cage". This consisted of placing a circuit, composed of discrete components, on a small phenolic card with sheet metal pins crimped around its edge. These pins were in turn wire wrapped to mating pins mounted in a large board that would support hundreds of the small phenolic cards. In crude concept, this was the forerunner of the SLT module.

During the period from mid-1957 until about late 1958, Ralph Palmer had me reporting into him. He explained to me that Mr. Watson, Jr., was determined to achieve standardization and minimize part numbers. If a low part number count was not achieved, Ralph would be in trouble and one microsecond later I would be in trouble since he had me reporting into him. I got the message.

Ralph suggested that I would be a good idea to make a movie, to be shown at the laboratories, that would extoll the virtues of standardization. After giving this some thought, I had a movie made which included a pan shot of a shelf of eight tube pluggable units. When the camera dolly backed off, the viewer saw that it was not a shelf these pluggable units were on, but was in fact the gym floor in the Poughkeepsie IBM Country Club. This sea of pluggable units represented all of the pluggable unit part numbers needed to service the 700 series machines. The movie was obviously too well done because Bud'Beattie saw it and put it under lock and key. Ralph's comment was" If anybody saw that movie, I would get fired." That ended my short career as a movie director.

Ralph decided that a one-sided printed circuit card, with 16 contacts that would hold one circuit, would be an ideal package. He conveyed his idea to Ed Garvey in Endicott and that was how the Standard Modular System -- SMS was born. I invited Ed down to Poughkeepsie to see what Ed Wyma and Newton Noell had been doing with wire wrap and he adopted this idea that had been developed by Bell Labs. My people and I worked with the Endicott people, and jointly set up a release procedure for circuits and cards. All logic circuits were approved by my group before they could be adopted as a standard circuit and published in the standard circuit manual.

During this period I set Bob Domenico up to do circuit simulation on digital computers and to develop and prove out circuit design concepts. We developed an end of life circuit design using a statistical analysis of component parameter drift. Somewhat earlier, I had set up a group under Herman Wolff to develop techniques for measuring the electrical characteristics of transistors so that we would be in a position of being able to predict the performance of a transistor in a circuit prior to building the circuit. The reason I got this work started was twofold:

1. A sound scientific base for circuit design is very important when large production runs of computers using these circuits are involved. It is equally important when the limit of performance is to be attained.

2. I felt that as devices, circuits, and packages get smaller and smaller the day will arrive when it is not practical to model a circuit on the bench.

In mid to late 1958 Ralph Palmer brought Nate Edwards on board to handle standards work in White Plains and asked me to turn over to Phil Stoughton the Product Engineering functions associated with the SMS program and staff Phil's group with competent in my organization. This I did and the transition was very smooth.

Preparing for Batch Fabrication Techniques

During 1957 and 1958 while most of my people were working on the SMS program, I had a small group composed of Gerry Maley, Paul Low and others investigating how machines should be designed so as to minimize the part number problem when many circuits could be fabricated on one substrate. At that time, the technologies that tended to indicate what was coming in the future, with respect to batch fabrication techniques, were cryogenics, electroluminescence/photo-conductors, and to a lesser degree, integrated circuits. I knew that T.I. was experimenting with putting one or more circuits down on a piece of silicon because Willis Adcock of T.I. asked me for and I gave him sample circuit configurations with which they could experiment. Since it was impractical for my group to work with cryogenics, we used electroluminescence/photoconductors as a vehicle with which to explore the impact that batch fabrication capability would have on machine organization.

It was at this time Rex Rice carried the concept from Poughkeepsie to Yorktown. He gave it the name Pattern Logic. Dan Slotnick was in his group for awhile and left IBM to join Westinghouse. Slotnick has continued to explore a branch of this concept ever since. While my group did demonstrate some "gee whiz" embodiments, these never found practical application.

ASDD Formation

In the summer of 1959, I was promoted to Manager of Technical Development. ASDD had just been formed and our first job was to structure a meaningful set of programs. I inherited several programs from Research when they were transferred, with people, to ASDD. I phased some of these programs out and continued to support others that looked worthwhile. Since ASDD's charter was to open up new market and product possibilities for the Corporation, I felt we had to initiate new technology programs with long-termpayoffs.

There were two areas that I felt had significant possibilities for growth, if the proper technologies could be developed to support them:

1. Unconstrained handprinted character recognition.

2. I coined the term Image Processing to describe the other area.

To further develop an idea I had pursued in my spare time during the Summer and Fall of 1956, I got Evon Greenias to join my group in Ossining. I wanted to set up a small but effective character recognition group to develop a practical means for identifying handprinted numerals and characters. The concept of using a flying spot scanner as a curve follower, which I had developed on paper earlier, made a good starting point. After I got Evon set up and the program started, it turned out I had to put my job on the line to keep the program going. My management took the position that ASDD should not be doing this type of work since Endicott had the responsibility for character recognition. My position was that Endicott was not working on unconstrained handprinting recognition and I could envision future product opportunities if IBM had this capability. I was specifically thinking of reading credit slips, stock inventory cards, etc. Subsequent developments have confirmed the convictions I had. The 1287 machine, in a sense, got started back in 1956 with the idea I had for pattern recognition. Certainly Evon's significant contributions to this program made the program a success and contributed towards his being named an IBM Fellow.

The image processing group that I established under Bob LeHane unfortunately did not really get off the ground. My idea was that while we had gotten fairly sophisticated at processing data we had done very little toward processing images. It was my feeling that a lot of man hours were spent in drafting and doing styling design in the auto industry. The reason this effort got no place was due to a lack of a focus. I later found out that at about the same time Elmer Sharp who was in DP was negotiating with General Motors for a Graphic Expression Machine - GEM. This was exactly in line with the concept I had in mind in connection with image processing. As it happened the GEM program reported into me when I returned to Poughkeepsie.

There were at least two other technologies that I anticipated but did not achieve success with. One was hydraulic logic. I tried to get Pat Panassidi to stop thinking in terms of pistons and valves as components with which to assemble a hydraulic system but instead think in terms of jets of air that can be deflected by other jets of air. Unfortunately, Pat did not have the background to develop this concept and I did not take the time to do so.

I had Jack Horton investigating the direct generation of microwave energy in a single block of semiconductor. We were using silicon and a magnetic field and were attempting to understand and improve on a mechanism, as I recall, RCA first discovered. As we now know, the concept was sound but it took Gunn and a different semiconductor to implement it.

As can be seen from this sample, the technical development work in ASDD, at that time, was quite broad, highly speculative, but quite anticipatory of future developments.

Back to Poughkeepsie

At the end of 1960 I was promoted to Manager of Technical Development of the Poughkeepsie Laboratory. In this position, I had responsibility for memory development and product engineering, machine technology, advanced technology, tape devices development including Hyper and Tractor, communications products development, and the GEM program. During this period a number of significant products were announced or delivered as a result of the efforts of these groups. Some that come to mind are: the 7302A air cooled memory, Hyper, Tractor, an incremental tape drive, and GEM. Some of these programs required my getting involved in technical details in order to make the programs a success. Hyper and GEM are notable examples.

Hyper was a tape drive system that was far superior to anything available at the time with the possible exception of Tractor. It took me several months to convince Jim Weidenhammer that the drive could not meet its start-stop specs. When he made the correct experiments and was convinced I was right, I suggested a capstan was needed that would absorb the energy stored in the tape. This is the fix that was adopted by Jim and Ray Barbeau. I was instrumental in getting Jim to adopt the phase encoding method of recording on the tape. In order to reduce the cost of the drive, I spent a month going over the design and was able to eliminate $1,000.00 of product cost without impacting the reliability of the drive. Last but not least, I had to get personally involved in a tape quality problem that held up a product test.

The GEM program was another difficult technical program. It was further complicated by SDD having subcontracted the job to FSD Kingston. In the Summer of 1961, I called an audit on the program and found my worst fears to be correct. I took over the program from FSD and put Ernest Newman in charge. To staff the group, I assigned many of the people in the communications area and the incremental tape activity to the program. It was a difficult technical program because it involved exposing microfilm from a CRT and developing, fixing, and drying the film in about 10 seconds. The film could then be viewed in a projector built into the machine. The machine also had a CRT display console with a light pencil. The system was checked out in Kingston prior to shipment to General Motors. A system checkout in Kingston had not been planned in the original schedule. This caused a further slip in delivery date. The machine was shipped early in 1963 and has been doing useful work in G. M.

The 91 Program

In January 1963 Bob Evans and Carl Reynolds asked me to join the systems areas as manager of Advanced Systems Development. I had responsibility for the Stretch machines in the field and made sure the customers that had these machines were happy. A group led by Dick May that was doing programming for NSA reported into me plus other programmers such as John Carter who was improving the performance of a FORTRAN IV for Stretch. Bob Meade had a small group of very competent engineers designing the 608 that later became the 91. This group worked closely with Bob Henle's group in CD who were developing ACP.

A basic problem with the 608 program was that the systems effort in my shop did not match the components effort in CD. I did not agree with the late delivery schedule for the machine since I felt competition would force an earlier schedule and if this did not happen then the component approach would be obsolete by the time the machine was delivered. Toward the end of 1963 the situation deteriorated rapidly when the technical approach that CD had been taking was challenged by other engineers in SDD. CD had designed their circuits around a tunnel diode which is a fast device but which, when placed at the end of a transmission line, caused a "ping pong" problem.

In retrospect, I would say my position at the time was somewhat ambiguous. On the one hand I had pushed very hard to get more funds into the systems effort to accelerate the program. On the other hand, I permitted CD to delay too long the dropping of the tunnel diode approach. In support of my position to let CD continue to explore the tunnel diode I felt at that time that there would be no great difficulty to revert to resistive loads if the tunnel diode approach did not buy anything. The rapidity with which the resistive load approach was implemented, once the program was properly supported, attests to the correctness of my feeling.

By the time the program was being given the support it required the emotional and political problems had gotten out of hand. As an example, I found one of my recently acquired managers had made an organizational change without complying to my written request to review all such changes with me prior to their implementation. This created two personnel problems which is what I wanted to avoid by my request to review proposed organizational changes. Since I found myself in the position of not being able to control problems that I had anticipated, I asked Max Paley to relieve me of responsibility for the 608 program. The request was granted and I was transferred to CD.

Integrated Circuits

When I joined CD in January of 1964, I was very surprised to learn that there was no work directed at developing integrated circuits. Furthermore, there was an antagonism toward integrated circuits. They were considered a threat to the SLT program rather than complementary to it. With the exception of Leon Atwood, all of the people in the Advanced Technology group that reported to me were negative toward integrated circuits. I later learned that Bob Henle made a presentation to the R&D Board in September 1963 in which he took the position that integrated circuits could never compete with a hybrid technology. All of this was very strange to me since these people had always been at the forefront of technical development. I found my conviction about integrated circuits was as welcome as a case of the bubonic plague. Dr. Pietenpol, to whom I reported, asked me as soon as I came on board to draw up a five year plan for Ad Tech. Naturally, I gave heavy emphasis to developing integrated circuits. My five year plan was not well received. I found myself in the position of having to lay out a plan for the next generation technology NGT and my management and my people completely negative to integrated circuits. Bob Doxtator and then John Haanstra were asked to give an assessment of the integrated circuit technology and its impact on SLT. The results of these task forces are well known and I won't belabor them here.

The problem I faced was converting my people to the idea that integrated circuits was the way to go and defining a packaging system that would enhance their capabilities. By August of 1964, I had defined a four phase program that would enable IBM to ship machines with integrated circuits by 1969 or 1970. Phase one was essentially, the MST program as it stands today. Phase two was to further exploit the SLT package but with a tighter grid spacing. Phase three was what later was known as the NGT program. It consisted of integrated circuit chips mounted on a multilayer ceramic module and the module mounted to a board through an areal connection of pins on the board and solder buckets on the module. The board was to be wired either through the use of the printed circuit technology or by discrete wires that were imbeded in a low melting point alloy such as Wood's metal. Phase three was aimed at cost/performance with Phase four directed toward high performance.

In addition to the above, I had another effort called MINT. This was a benchmark program aimed at investigating the advantages and disadvantages of large scale integration LSI for small low cost machines by using the ECHO keypunch as a test vehicle. This program was directed by Bill McAnney and there was a total of about eight people in his group. We had a contract with T.I. to supply two sets of nine modules with about 120 circuits on one wafer on each module. A model of the ECHO keypunch was completed and working by April of 1966. It consisted of nine modules each containing 120 circuits on one wafer and the nine modules were mounted on a printed circuit board along with some special circuits.

During the early part of 1964 T.I. people described to us their status with integrated circuits. I asked them about monolithic memories and they indicated that they could indeed build many memory cells on a chip. I talked to Bob Meade about the use of monolithic registers in the 91 and conveyed to him the information that T.I. had given me. This interaction started what later became the SP-95 memory.

At the same time, I had Fred Buelow and Leon Atwood in Fred's group set to work investigating the advantages of monolithic memories in configurations other than shift registers. Leon's work led to the medium speed local store, MSLS, which was used in the Mod 44.

In the Fall of 1964, Bob Henle decided on monolithic memories as his direction of effort for his IBM Fellow program. Upon learing this, I told Fred Buelow to discontinue further work on memories since to do so would place his group in competition with Bob Henle and create unnecessary duplication. I felt very strongly that monolithic memories would be very important in the future and in fact, informed Bob Evans of this. I happened to see Bob Evans at the Poughkeepsie Airport at this time, and told him that I felt monolithic memories would obsolete thin film memories. I also told him that as a responsible manager, I was having my people discontinue the work I had gotten started on monolithic memories. My explanation was that Bob Henle was a competent engineer and I could not justify a duplicate effort even though we had started work in February or March of 1969 before Henle became an IBM Fellow.

During 1964 and 1965, my group worked on developing the capability to do the NGT Program. It must be remembered that the Ad Tech effort in CD had been aimed at ACP which later became ASLT. There had been no effort on integrated circuits prior to the NGT Program. The ACP Program did pioneer the multilayer board technology, and a very small effort under Bernie Schwartz was exploring complex ceramic structures. To a very large degree, therefore, the NGT Program had to pick a direction and then set about developing support tools as well as the technologies to be used without the advantage of being able to draw on previous efforts.

The direction chosen was mentioned earlier and I would say the concept is still sound. Let me outline it again:

1. Phase one of NGT became the MST Program.

2. Phase two of NGT was not done in the NGT Program but was explored by Kurt Trampel's group in Poughkeepsie and they call it NEXT.

3. Many ideas and technologies of phases three and four of NGT were adopted by NLT-HP and the SNS Programs:

   a) Multilayer ceramics
   b) Capillary Refill
   c) Multi-chip module
   d) Areal connections between module and board
   e) Solder bucket connections
   f) Heaters imbedded in module
   g) Discrete wires on a board imbedded in a low melting point conductor.

Many support tools were developed in parallel with technology development. I had a printed circuit generator modified to improve its precision so that it could be used to generate masks two hundred times actual size. These masks were then photo reduced for use on a step and repeat camera. I had a group under Gary Gied develop and build integrated circuit chip testers. There were other people developing design automation techniques for mask layout and developing test techniques for testing complex integrated circuits. All this activity, while peripheral to the main effort, was needed for a successful program.

While the people in the semiconductor shop under Dave DeWitt, Don Seraphim, and Ernie Van Derveer developed the integrated circuit technology -- Rico DePietro who was in Owego, supplied our first integrated circuit chips. These consisted of 3 emitter coupled logic circuits per chip and the circuit delay was 5 nano-seconds. Jerry West's group in Endicott mounted the chips on multilayer ceramic modules made by Bernie Schwartz's group in Fish-kill. The modules were designed and checked out by Brian Malbec and his people.

One of the major problems we faced in 1964 and 1965 was getting experienced people on the program. The cryogenic effort reported to me in early 1964 and was headed by Holly Caswell. I gave Holly the challenge, which he accepted, of completing a working model of an eight by eight by eight memory by July 1964 or if unsuccessful, terminating the program. They were not successful, and I terminated the cryogenic program. The people in this group were given choices of joining my other groups or joining the semiconductor groups and most of them joined my other groups. These were experienced people and they were badly needed. There was a great risk involved in terminating the cryogenic program since GE, T.I. and RCA still continued their work which might have been successful.

The people from the cryogenic program enabled me to staff a packaging group, a test equipment group, and contribute to the staffing of a multi-layer ceramic effort. By the end of 1964 the basic technology work was well underway. The semiconductor and module work was still being done for the program on a subcontract basis. The multilayer ceramic group was inadequately staffed and I therefore had to depend on the subcontract module group. This was very unsatisfactory because the subcontract group ould only think in terms of variations on SLT. The semiconductor sub-ontract efforts was somewhat better, but not much. As an example of hat the NGT program was faced with, Ed Davis had Casper Barscia do n analysis which showed that the maximum level of integration desirable, om an economic viewpoint, was 5 circuits per chip. This was in ebruary and March of 1955. Ed Davis publicized his findings without hecking with me. I found myself in the position of targeting my cost rformance program toward 10 to 20 circuits per chip and my MINT ogram toward 120 circuits per chip. It must be remembered that I as depending on Ed Davis and his people for my semiconductor and odule designs.

I was forced to take the position that I had to have my own fully staffed module effort if the NGT program was ever to develop and demonstrate the route I felt we had to take. In the Summer of 1965, we redistributed the manpower in Ed Davis' group, and I was able to set up a multilayer ceramic pilot-line. We were then able to make progress and turn out multilayer, multichip modules.

In December of 1965, Jerry Haddad headed a task force to review the NGT ~~program~~ and decide what technology would be used to support the next family of machines. The task force recommended that the corporation go with what was first proposed as phase one of NGT. At this time, it was called SLX. It later became MLT and then was called MST which is its present name.

In January of 1966, I placed Jim Walsh in charge of the MLT program and staffed his group with people from the NGT program. By June of 1966 it became clear the NGT program could not survive in the environment in Fishkill and I joined Jerry Haddad's staff in Armonk.

Staff Director of the CTC

After assisting Jerry Haddad for about six months, I was promoted to Staff Director of the Corporate Technical Committee and reported to Mannie Piore. This was January 1967. At this time the Memory Strategy was submitted for review by the CTC. I felt the strategy put improper emphasis on ferrite cores and thin magnetic films and inadequate emphasis on monolithic memories. To prove my contention, I went back to Bob Henle to see where he stood after a little over two years study of monolithic memories. By chance, he had just written a memo to file with no carbon copy list in which he identified advantages of monolithic memories. I used this to support my position that the Memory Strategy was wrong. Mannie Piore concurred and we made it an issue with DP Group. It took DP Group one year to study the situation and conclude that the CTC was correct.

During 1967 I drafted a procedures manual for the CTC. It defined the content of the strategy documents and the procedures that were to be followed for their approval. It also became clear that the CTC could not limit itself to addressing only technical issues as it was charted to do. I felt the CTC had to understand the product direction and the relationship of the technologies to this direction. SDD was very emphatic in wanting the CTC to stay out anything smacking of a product issue. During 1967, SDD also adopted the concept of written strategies. Some of these strategies were uniquely SDD's, others were initially CTC strategies that SDD incorporated as part of their family of strategies. This created a certain amount of confusion and tended to water down the effectiveness of the CTC strategies because of the dilution caused by the SDD strategies.

It had a beneficial effect with respect to the CTC being exposed to SDD's product strategy. SDD was obviously more interested in a product strategy than a technical strategy. They therefore spent more effort on defining the product strategy. At the same time, they did not want the CTC to formally review the product strategy. After several meetings with SDD people, I got them to agree to write their strategies in two parts; a technical strategy and a product strategy. The CTC could review the technical strategy with the benefit of having a product strategy to review in parallel. It was agreed that the CTC would not raise issues with the product strategies. This arrangement worked fairly well.

By my staff and myself reviewing the strategies and having a first hand familiarity with the technical work in the laboratory that supported the strategy, I was able to inform Dr. Piore of the weaknesses in the strategy and issues that should be raised that may not have been raised by the CTC members. I felt, and Dr. Piore agreed, that one main measure of my success in the job of Staff Director of the CTC was how infrequently the Corporation would experience technological surprises. There were no technological surprises during my three years in this assignment. Monolithic memories could have been a disasterous surprise but as I described earlier, this was very effectively avoided.

The above briefly but accurately summarizes the highlights of my 19 years with IBM and in the computer field. I would say my major early contributions involved pioneering in solid state logic circuit development which permitted IBM to switch from tubes to transistors without a major disruption. While I personally contributed in terms of patents, my main contribution was in selecting the right people, training them, leading them down the right technical paths, and challenging them to do more and better professional work. As can be seen, there have been many times when I have selected, at a very early date, the right technology that ultimately survived. Conversely, I have been instrumental in terminating technologies that were wrong to pursue further.

During the last ten years, my contributions have been at a somewhat higher management level. In addition, my assignments have been more varied. They have involved peripheral equipment, systems, and to a much lesser degree programmer supervision. My last three years were spent looking after the technical health of the Corporation.

I hope my next fifteen years will be as interesting as my last nineteen years. Lastly, I hope this will be of use to you, Larry.

J. C. Logue

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JCL:mc:cpl