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Interviewee: Jay W. Forrester
Interviewer: Richard R. Mertz
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MERTZ:

Would you like to describe your early childhood and education Mr. Forrester?

FORRESTER:

Yes. My childhood up to the point in where I went to college was on our cattle ranch in the middle of Nebraska, West of Anselmo, Nebraska, so that it was a childhood far out in the country, 20 or 45 miles from where we did even our grocery shopping and as such it was in a pioneer community? Because my father and other people of his generation were the first comers—first private comers—of the land and so these people that I knew as a chi1d were actually the pioneering group in that particular section of the country. As such, one had of course all of the problems of self-sufficiency and this meant building a great deal of one's com equipment. It meant the repair of equipment. It meant initially a home without electricity and so it was an environment, in which there was a great deal of opportunity for innovation and for projects that were important projects more than just hobbies.

MERTZ:

Did you go to school in Anselmo?

FORRESTER:

I started primary school in the third grade after having been taught the equivalent of the first two grades by my mother who had been a school teacher in the vicinity and my father from the time that he went to that country had taught school until two years after I actually started school, so that my first two years are at home under my mother and my third and fourth years were in the one room country school house that my farther taught at; and then I continued in this one-room rura1 schoo1 through the eighth grade and then went to high school in Anselmo where my mother and sister and I lived in town during the school week. She went to secondary school while I was going to high school and then we would go to the ranch on weekends.

MERTZ:

How many brothers and sisters did you have?

FORRESTER:

One sister, Barbara.

MERTZ:

I see. And your parents came to Nebraska from the East?

FORRESTER:

Yes, my father had attended Hastings College in Nebraska and after graduating from college had come up and homesteaded one square mile in what's known as the Nebraska Sandhill Region. The people who initially moved in attempted to farm the land. It's not suitable for farmland. Those who survived and stayed gradually accumulated the land and turned it back into grassland for cattle ranching. That farming phase had about come to an end when I first remember the country. It was still in progress but it was rapidly dying out and giving way t o cattle growing.

MERTZ:

Both of your parents then were school teachers?

FORRESTER:

As a necessity for earning money, I think, rather than what you would call a profession.

MERTZ:

I. was wondering, did your father specialize in any particular field at Hastings?

FORRESTER:

Basically it was a liberal arts college. He worked on the city newspaper. I think of him somewhat as a journalist in those days, but I think the education would be a fairly general liberal arts education including Greek and Latin and the kinds of subjects one would expect under the circumstances.

MERTZ:

When you were in high school were there any particular subjects that you took that you found more interesting or that you did a lot better at than the other subjects?

FORRESTER:

Generally I think I did well in most subjects. Probably anything that, one might call English or writing was more difficult and certainly in college I did less well in. In more recent years it is what I've done the most of. But in the period—secondary school and high school—it ' s fairly clear now that the beginning of a number of trends started in the way of experimentation, interest in electricity. My father always allowed a very substantial amount of time for various kinds of projects, electrical hobbies. Some of these were very basic experiments in electricity. They became turned into an interest in radio that did not go very far. It included the construction of a working wind-driven electric plant which was the first source of electricity on the ranch in which the entire system was made out of second-hand parts, pieces of automobiles, and which worked for a number of years, perhaps 10 years or more before being replaced with a commercial electric plant (wind-driven) which was then in time replaced by the Rural Electric Administration (REA) e1ectric supp1y .

MERTZ:

You built your own crystal set at that time?

FORRESTER:

I came into radio after the crystal days and so it was in the vacuum tube—and this actually was while I was in college. I didn't reach the radio stage until the late thirties.

MERTZ:

When you graduated from high school what choices mere open to you in terms of what you could do with your life?

FORRESTER:

Unlike many people in a similar environment, it was assumed that I cou1d and wou1d go to co1lege so that this opportunity was open. It was assumed I think also it would be certainly local to the extent of being within the state of Nebraska. I don't think there was any consideration of the breadth of choices that people now contemplate when they're planning college. I had received a scholarship to the Agricultural College and was actually embarked on going to it. I was within weeks of attending the University of Nebraska Agricultural College when I decided this wasn't really what I wanted to study. I would rather study engineering and changed to the University of Nebraska School of Engineering. Under the circumstances of the time one had very little information about college. There was very little discussion of it. Really very little preliminary planning and no need to apply because anyone by law had to be accepted. The only problem was staying in.

MERTZ:

Was this state land grant?

FORRESTER:

Yes.

MERTZ:

Did your scholarship transfer over to the Engineering?

FORRESTER:

No, I believe not. On the other hand, tuition as I recall was $35 a semester so that it was not a big issue.

MERTZ:

And them you enrolled in the Engineering School at the University of Nebraska?

FORRESTER:

Yes, and studied electrical engineering there, graduating in 1939.

MERTZ:

Those were sort of the end of the Depression years as such. Do you have any recollection of the impact on you or your schooling which the Depression had? Or was it in terms of limiting possibly where you would consider going? Or were demands placed upon you to help out at home in the summers?

FORRESTER:

Well, it was expected of course that—just assumed that I would do anything I was able to in the way of helping with the work of the ranch. This included weekends while I was in high school. It included all summers. It did include all summers while I was in college. The summer on a cattle ranch is one of the very busy seasons because if it is a year round ranch, as it was at that time, with cattle kept through the winter, the summer is devoted to the cutting of hay and this is an extremely busy period. The transition from horse-drawn mowing machines to tractors occurred during the thirties, so through that period there was hay cutting which ran from late June through ti1 September.

MERTZ:

Did you have any jobs of any kind when you were at the University to supplement your income during) your undergraduate days?

FORRESTER:

Not in any major way. Minor but not substantial to the extent that some people did. I was not earning my way through college while I was going to college. I think this generally was assumed to be part of the summer work and so forth.

MERTZ:

When you graduated in 1939, what then were the options open to you in terms of activities and what ones did you prefer to follow? Among the individuals who had an influence on your life at that time? Who do you recall of those who perhaps exhibited more influence than others on your choices or your
choice?

FORRESTER:

Well again, as with the decision of going to college I think there was much less known and much less thought than perhaps one would expect today. I was encouraged probably by the peop1e in the University to consider graduate study. I don' t think I considered anything else very seriously. I applied to a number of universities, schools for graduate work in electrical engineering, and there were very few to which you could apply—I think 5 or 6 would about exhaust the number that one might reasonably apply to at that time, since there was very little graduate study in engineering available. So the 4 or 5 applications I believe that I—I applied to 4 or 5 places and I think it would be reasonable to say that the choice of MIT was basically for two reasons. First of all, my mother had been a librarian in Springfield, Massachusetts for a brief period after she left college and she knew about MIT and held it in some awe and respect and so we had at least heard of it. Secondly, they made the best financial offer as a research assistant by a substantial margin compared to the others, consisting of tuition and $100 a month, and this seemed to be important. I think those two reasons account for the choice.

MERTZ:

Had you been to MIT or the Boston area prior to this time?

FORRESTER:

No, I had been as far east as Chicago on an engineering field trip but one's horizon from growing up in Nebraska that there are two or three big Eastern cities consisting of Boston, New York, and Chicago. Chicago being an Eastern city.

MERTZ:

At that time was there, an EE Department in Illinois? I believe, that was fair1y active

wasn't there?

FORRESTER:

There could have been. My recollection is that I applied to Cornell, probably to Cal Tech but I'm not sure, and I'm not positive which others.

MERTZ:

Well, then when you arrived on the scene at MIT as a graduate student in electrical engineering, you had not met or known other than by correspondence anyone on the faculty?

FORRESTER:

That's right. I had met no one here. The Electrical Engineering Department a t MIT at that time was a great dea1 smaller than it is now and the kind of education was very different from graduate education in most places today, although this pattern continues I think to be true here at MIT in electrical engineering, but graduate students generally speaking held staff appointments. They were full time research or teaching or teaching assistants, which meant that they only took about a quarter or a third of a regular academic program and they began immediately to work in the research and the educational processes of MIT, and it's that teaching and research which is by far the most effective from of education and I think that in my own development this was a great deal more important than would have been any pattern of merely going to classes.

MERTZ:

You enrolled in the fall of '39?

FORRESTER:

In the fall of '39, yes.

MERTZ:

And do you recall of the professors under whom you studied, the ones who made the greatest impression on you as an undergraduate student?

FORRESTER:

Well, you see again I would stress; as an entering, very junior member of the staff, because the context tended to be in terms of the staff position and so initially this was in the teaching of the electric machinery laboratory under Professor Carl ton Tucker and rather soon after that turned into a year of research under Professor -John Trump in high

voltage engineering, the development of the high voltage electrostatic generator which has now a multiplicity of purposes, one of which at that time was for medical therapy. So I worked with Trump in his laboratory until the second year. I think it was it was in the fall of 1940 when I was adopted or acquired by Professor Gordon Brown for some new work that he was starting.

MERTZ:

Did you spend your summers at MIT or did you return back to Nebraska for the summer?

FORRESTER:

I went back to. Nebraska for the first summer after I came, I believe, and I think that after that I became involved in the pressures of the World War II research program and went back to the ranch them only for vacation rather than for the entire summer. The work under Gordon Brown that I referred to was the beginning of what came to be known as the Servomechanisms Laboratory, the development of feedback devices for the control of gun mounts and radar" antennas, the development of the theory of such devices as well as the design of such, and the design was a real life design in the sense that me were turning out the designs that were actually being put into production and some had the responsibility all the way from theory to actually the fie1d prob1ems in the actual military end application.

MERTZ:

In those days at the outset of the Servomechanisms Laboratory, was there a fairly sizable staff or was it rather modestly staffed?

FORRESTER:

It initially started with four people. Professor Gordon Brown, myself, Albert Hall, and Jack Silvey; and then gradually-evolved from that into an organization of a hundred or mare people.

MERTZ:

Largely under the impetus of the war effort?

FORRESTER:

Yes. This was servomechanisms research as applied to quite, a number of different app1ications.

MERTZ:

And during that time roughly what period of time did that cover? 1941 until?

FORRESTER:

From the fall of 1940 through until the beginning of my work in electronic computers, let us say, in the end of 1944 or early 1945.

MERTZ:

During that period were working on doing continuing graduate work or had you completed some of your graduate work?

FORRESTER:

I had completed most of my formal graduate work early in that period. I did not take the time to convert my thesis from a first draft to a final draft until about three years or more had elapsed so that my Master's degree from MIT in 1945. But during that time I was completely involved in the research design program, the resulting production prob1ems, the field testing, the things that were involved in carrying an idea into actua1 working hard ware.

MERTZ:

Did you notice any shift in the time that you devoted to research and time devoted to teaching during this period? Were you spending more time, perhaps because of the pressure of the war effort in research and less in teaching?

FORRESTER:

All of the time was in research. Actually the teaching was only in the first winter that I was here in the electrical machinery laboratory. The work under John trump would be research and all of the work with the Servomechanisms Laboratory was research and development work. I would say that period under Gordon Brown was the major professional formative period in my career. It's to him and his encouragement and to the way he ran the laboratory that I think one can attribute the succeeding steps.

MERTZ:

Now we get to the threshold of your involvement wit computing machines. One question which put to myself and to others who are more familiar with the problem often, is that it appears that the development in the early stages of digital machines, the more significant developments, took place at facilities where there was already quite sophisticated existing level of development in analog machines. That was certainly the case at MIT and elsewhere. Was it originally conceived in the Navy contract that began the Whirlwind story, that analog computing equipment would be used in the flight simulator? Was this

the idea of adapting what was existing, already existing?

FORRESTER:

Yes, you see my transition into electronic computers came at the end of World War II and it was at a point of decision where I had been thinking that now was the time to leave MIT. I considered various possibilities outside and it was at this point that, in discussions with Gordon Brown who had various possibilities that he thought were in the offing, that I discussed with him what these might be and one in particular attracted my attention and I decided that I would see what I could do with the particular project, which is the one you're referring to—the proposal to build an aircraft simulator. This proposal I had had nothing to do with the history. It had come out of work done in the Special Devices Center of the Navy under Admiral Louis DeFlores during World War II, of which I had been unaware at the time it was going on, in which they had built trainers. The Link trainer was a very well-known phrase at this time. These were much more sophisticated trainers. A number of them at least eventually had been built by Bell Laboratories and they were devices in which a very realistic feeling an appearing airplane cockpit was operated, mechanized by computing devices on the outside so that it would appear to the pilot to be sufficiently realistic that it could be used for pilot training.

The proposal was to go far beyond the trainer into a device that could foresee the behavior of large aircraft so that some of their stability characteristics could be anticipated before a full scale airplane had actually been built and tested. The presumption was that this could be done along analog computing lines, as had the earlier trainers.

MERTZ:

This was also in the pre-jet era of aircraft, I believe.

FORRESTER:

Yes, pre-jet and where there was much less, I think, of booster contro1s and so forth, where the match between the pilot and the airplane was perhaps more important than it is today. I don't know what today's feeling is.

MERTZ:

And of the possible directions which your research activity could take suggested by Professor Brown, that one had the greatest appeal to you. You might wish to describe then why it was that the extension of the earlier experience with the analog machines seemed to be inapplicable to the kind of problems that you were now faced with.

FORRESTER:

Well, this proposed project in simulators came equipped with a set of equations that had been developed in our Aeronautics Department and we, were simply trying to produce the simulator or the differential analyzer if you like which would handle that body of equations. These equations were very complex and the further one went into the development of a machine to handle them, the more complicated it became and the less likely it began to appear that such a machine would ever1 in fact function. So it came to the point where we felt that this really was not a feasible approach and then the question was one of giving up the whole idea or turning to some other way to accomplish the result, and by this time the program had come under the technical direction of a man name Perry Crawford who had been a student at MIT, who had gone to the Navy to work in the department that DeFlores headed. I think perhaps at about this point he was the Technical Officer. In any case it was I think out of discussions with him and his awareness of the fact that digital, computer work was getting under way and was beginning at various places that I began to consider the possibility that digital computation offered something for this particu1ar prob1em.

Now at about that time the Harvard MARK I machine was nearing completion and the ENIAC computer at the University of Pennsylvania was at or near completion. These two devices were in hardware form by that time, neither of them being at all close to what was needed but both of them basically showing that numerical techniques had a place.

MERTZ:

One of the problems as I understand it was the problem of real time and possible errors... the analog machine?

FORRESTER:

The analog machines would have been so complicated that I think one would never have known whether they were solving the in tended prob1em or so1ving simp1y the interactions of their own idiosyncrasies as the components affected one another. The digital machine gave one a clear control over the logic. You would know what was going to happen based on the logic. At 1east the machine was not going to make unexpected contributions to what happened. The dimensions of the problem in terms of digital computation were still not at all clear because that whole digital computation approach now required evaluation and interpretation. By the time we were well into considering digital techniques there was a plan afoot a proposal for a digital machine that was being carried on at the University of Pennsylvania and I am not entirely sure—never have been—who the major contributors to this were. Eckert and Mauchly were the ones directly involved. It is probable that Von Neumann did a great deal with respect to the logic of it but I'm not entirely sure what the interrelationships were amongst those various people. But from our viewpoint anyway, they were in the process of making proposals for a serial machine that would use mercury column memory. We
Visited with them and this along with the visits to Harvard constituted I think our introduction to thinking about the logic: of numerical computation and we began to

explore the possibility of designing this aircraft simulator along the lines of a seria1 machine.

Immediately very early it became apparent that there was going to be a problem of speed because the one step at a time process that such a machine implies, when spread over this very large number of complex equations, meant a solution interval between successive steps that began to be too long for the task at hand. We pressed the matter into higher speed types of serial storage using cathode ray tubes, higher speed than the mercury line, but even so it did not seem feasible and about one; year spent on the serial machine led us to essentially the same kind of conclusion that we had come to with the analog machine, namely, it didn't look like it would work and we had a crossroads of either a new approach or to discard the task.

MERTZ:

To what extent did the developments up to this point—as I understand it the memory was one of the key stumbling blocks, but so far as the arithmetic and the logic of the machine? Was that worked out fairly early notwithstanding the problem of the memory storage?

FORRESTER:

No, I don't think that you cam say that it was worked out fairly early because the logic has a lot to do with speed. The serial logic I think was pretty much available from the work at the University of Pennsy1vania. But it wasn't adequate to this particular task. So then we turned to the parallel type machine and again there was some parallel type computer thinking going on at the Institute for Advanced Study at Princeton under Von Neumann with Herman Goldstein and Julian Bigelow and the people working there. They were using a very different kind of electronics than we eventually moved into? But they were using a parallel machine. They were using a non-synchronous logic which we didn't feel comfortable with and so we retained the synchronous logic of the serial machine while developing the parallel computer. It would be very difficult to say just where our new contributions to that logic started and where the things were able to acquire left off, but I believe for example things like the high speed carry in the multiplication process was out innovation and that of course made a lot of difference in the speed of operation. So we began them to develop the, parallel synchronous logic, clock timed logic of the machine and if I recall correctly it was in the spring of 1948 that the block diagrams of the Whirlwind computer mere published. Now these mere published under the name of Robert Everett, who made the biggest major contribution, and Francis Swayne who worked with him. I had worked some with Everett on these various ones. But Everett was the person mho put all this together into a cohesive package representing the logic of computer. I mould say that the logic part of it wasn't at all under control until about that time. It had gone through a continuous evolution and I don't think that me mere particularly aware until about that time of the memory being the principal bottleneck because there was a problem every place you turned. There was a problem of vacuum life. There was a problem of just the fundamental logic. There was

problem of all the terminal equipment input and output and none of these existed. One started this whole process from the vacuum tubes and the video circuits that came out of radar at the end of World War II, which was quite a long way from what one needed.

MERTZ:

The development of a clock time, for example in Whirlwind I was fixed. Was this development: based on work with the 5-digit multiplier as a merely experimental device to try out things that were to be incorporated—features that would be ultimately incorporated in the Whirlwind?

FORRESTER:

I may have to check the chronology but I expect we'll find that the 5-digit multiplier came into existence after the purpose of the entire project had begun to change. The aircraft analyzer concept, the task of designing an airplane began to recede in importance and particularly in the circles in which Perry Crawford and I circulated it more and more became apparent that the big job was in what we might call tactical information control or tactical force control and that meant initially the control of the information and the maneuvers of an antisubmarine task force. How can you take all the different pieces of information—the sonar soundings, the radar positions for radar positions from multiple ships which are moving around with respect to each other—and put these all into one frame of reference so that you can see what the total information means in, let us say, a submarine search or any other kind of task.

There was no way to do this other than just plotting things on chalk boards and the proposal began to evolve that we would think of a digita1 computer as the information center for such an operation. The first reports that were written describing the possibilities were, I think, in the spring of 1948. Going back to the 5-digit multiplier, I think it must have been contemporary with that time and the 5-digit multiplier then is a part of a pattern you will find through the entire Whirlwind program. That is a very heavy emphasis on engineering performance, on reliability, on checking things out as thoroughly as one can as it goes along. So the 5-digit multiplier was a prototype of the arithmetic element of the Whirlwind computer in which logic, the high speed carry—these things were present. But also you must recall there was simply nothing known on the question of random thermal noise errors or the possibility of something making a mistake when it was not in any normal sense of the word malfunctioning or broken. There was simply no knowledge as to whether there might be such a thing as the random mistake without any physical reproducible cause. There was some reason to think the thermal noise and such might possibly produce such impulses.

So the 5-digit multiplier was for many purposes. I was to test vacuum tube life and reliability. It was to test the logic. It was to test the life of components, and in particular it was to test the continuity of error—free operation. So it was instrumented in such a way that individual mistakes, single events could be detected to see their frequency and where

they came from. As I recall we began to find them. We began to find unexplained events and as we began to trace them down we became suspicious of transients on the power lines, of switching transients. We for example went to complete electrical isolation, motors driving our own generators, which made a tremendous increase in the reliability. But it was for purposes of this sort.

MERTZ:

Roughly when did the concept of—was it on this that the concept of marginal checking evolved, or was that on the parts of the larger machines?

FORRESTER:

No, marginal checking came along later and I think I have a very clear recollection of the circumstances for that getting started. The Whirlwind program was a very expensive program for the time. Over a period of 5 or 6 years it spent 4 1/4 million dollars, which is less than the production cost of the machine today but was looked upon as unreasonably expensive at the time. The Navy was short of funds and about once a year we had the recurring investigation to find out if in fact we were being sensible and were we doing proper things and what were we doing with the money. One of these reviews was conducted by I believe his name was Francis Murray.

MERTZ:

Francis J. Murray?

FORRESTER:

Francis J. Murray of Columbia University and he I believe was a mathematician. He turned out to be a man with a great deal of perception as to the problems and so forth. I think it was probably on a Saturday as I. recall but in any case in his quizzing me on what we were doing and its implications he asked me what I was going to do about this very large number of vacuum tubes as they—he asked what we were going to do about this large number of vacuum tubes when some of them gradually began to deteriorate and would get down to the point where their operation was marginal, because at this point any kind of disturbance—a street car going by a bit of vibration or something—would be able to shift them across the boundary from where they would work or they might not work and that these would be very difficult to detect and to find out where the trouble was. They were not outright failures that would be easy to locate and what were we going to do about it. Being under a great deal of pressure and necessity to maintain the support from everyone; I simply answered his question. I said that I thought we could probably design a marginal checking system that would measure the margin between where a tube was functioning and where it would cause failure. In other words, he had propounded the question and the proposition and under the pressure of having to have an answer to a question that in fact had been clearly put, I essentially provided him with what I thought

was a satisfactory answer. It seemed like such a good answer that the next Monday morning we designed it into the machine.

MERTZ:

Who of your colleagues worked mainly on this problem?

FORRESTER:

Well, a number of people did. Norman Taylor was in charge of the electronic design. He was the kind of chief engineer and with various people working for him, he probably carried the main burden of seeing that the design got done. I don't, think I recall who did the literal physical design, but it was mostly the design to do it or the decision what to do and the decision to expend the rather large amount of time and money that was going to be necessary to segregate the machine into the cross
section or coordinates along with the control mechanisms necessary for carrying out the marginal checking process, so that it was, in the face of a budget shortage and all it was a managerial decision of some difficulty to make. But basically once the proposition had been asserted it was not very difficult to carry out.

MERTZ:

At this time had the production of the rectifiers that are numerically the largest single vacuum tube component of the prototype computer—had that development reached a state of reliability which was satisfactory in your judgment to be incorporated at about the time or prior to the development of margina1 checking?

FORRESTER:

You're talking about rec…?

MERTZ:

7AK7.

FORRESTER:

No, these are not—the 7AK7—these are not rectifiers. I think you're speaking of the multigrid, the gate tube. The gate tube, are you? All right, this was the gate tube, the principal logic tube in the machine. I'm not sure exactly the date at which this came into existence but the history was that at the end of World War II almost nothing was known about the life of vacuum tubes out beyond the 500-hour point where people had run tests connected with entertainment uses, as in radio. There we re a few so called high reliability tubes being made but there was little known about them and even there it was mostly a mechanical rigidity kind of reliability that people mere trying to get what was

known about the nature of the failure. The ordinary tube tester simply showed that there was low emission without any idea as to why it occurred. We had undertaken a program to try to find out how to improve the life of vacuum tubes because one needs a very long life, up in the hundreds of hours, if he is going to have a reliable machine with the number of vacuum tubes that we were designing. I have some dim reco1lection of part of the sequence. Nominally tubes failed because they had low emission. On the other hand, I think we knew or our tests showed that if you drove the same low emission tubes into the positive grid region, one could in fact get maybe as much as an ampere out of a tube which nominally was not supposed to be able to give ten mi 11iamperes. So clearly there was something going on that wasn't a pure failure to emit electrons and out of series of tests and contemplation of logic of the situation you know, what could be going on that gave the array of symptoms which we knew about—it began to be clear that something must be going on dynamically within the vacuum tubes. I'm a little hazy about, exactly how this idea began to develop but I think it's one that various people were working on and I think that at one stage I myself began to wonder about, the possibility of something underneath the cathode of the tube.

Actually we were beginning to get then someone, I don't remember who was doing the work, but we were beginning to get serene tests that showed under pulses grid conditions one, got a very high current out of these tubes initially which died out exponentially to the low emission condition. This was basically the K curve that you expect, from a resistor capacitor in series with each other and as we began to look into it we discovered that indeed that's exactly what me had, that there was an extremely high capacitance between the nickel of the cathode, and the emitting oxide, formed by an extremely thin oxide layer on the surface of the nickel,. When we discovered the cause of the so called low emission which was not inability to emit but actually a self-biasing condition
In which the actual bias on the tube was being driven down internally, not externally, within…

[NO SPEECH ON TAPE FOR A FEW MOMENTS HERE]

MERTZ:

We were discussing the development of a sort of self-inhibiting….

FORRESTER:

Once having discovered the nature of what was going on, various chemical analyses and so forth showed the composition of what was happening and this was traced back to the intentional inclusion of silicon in the cathode nickel. Silicon was causing the trouble but it was intentionally being included by the manufacturer in order to make the activation of the cathode easier. So then in work
with a man at Sylvania Electric we began to develop some vacuum tubes that could omit the silicon make up for it by more time-consuming and more expensive processing techniques, and in essentially one design step raised vacuum tube, life from maybe 500

hours to 500,000 hours. Then the marginal checking to catch about 9 out of 10 potential failures above that point, giving another factor of 10, so that in terms of failure probability in service? I think me mere getting vacuum tube life up in the region of what you might call five million hours. Now it's not been five million hours but this is in terms of the, one tenth of one per cent failures per thousand hours of operation. So we moved vacuum tubes up to a life that probably has only recently been equaled by semiconductors which are supposed to be far more reliable.

MERTZ:

Now at this point perhaps it might be interesting for you to characterize—we've come up to the threshold of the development of the ferrite memory cores—those contributions which you made to Whirlwind, to the project, which you feel are the most significant ones; and then also your view of the significance, of the machine itse1f.

FORRESTER:

The largest technical, contribution and the one that endures up to the present time was of course the coincident current magnetic core memory. My own contribution I think was equally managerial inspirational, and technical. We had equal good in one or two of these areas; perhaps it was my opportunity to try to provide a balance of each of these. I would say the major—as one looks at it in retrospect now—the major technical contribution that I made was the one in the magnetic core memory.

MERTZ:

Now the machine itself in general, what do you feel were its significant contributions to computer technology?

FORRESTER:

It had a number of things which were I think first in the Whir1wind computer and which have survived machines even the present time. One was the synchronous parallel logic: that we have spoken of, the clock timed parallel logic. Parallel logic had existed, clock timing had existed, but this was a combination that I believe was new although I think one would have to examine somewhat the structure of the Harvard machines that were contemporary that Howard Aiken was making, to make
sure. I think those were serial machines in the magnetic stepping register manifestation. So the synchronous parallel logic, the marginal checking that we have spoken of was another first in the Whirlwind computer. The Whirlwind computer was the first one, I believe, to have a cathode ray tube output. I very early had a cathode ray tube out put because we were short of things like page printers that would work. So we had symbols generated, letters, figures, vectors on an output cathode ray tube display certainly as early, I think as 1949. I k now that these we re operating actua11y in air defense experiments with real life airplanes in the very early part of 1950.

There was typewriter output from the electronic machine. That may not have been the first time that was done. It was the first time—shared machine because there was a multiplicity of cathode ray tube consoles on it in the later stages, say in 1950-1-2-3y in that interval, with each of the operators having access to the machinery each one being able to introduce his; particular part in the man-machine interaction, so that there was multiple display. Time-sharing I think is a fair ward to apply to it because it shared central processor time amongst various people doing different things, responding individually to what they wanted.

MERTZ:

You also mentioned tube reliability which was perhaps—

FORRESTER:

Yes, it certainly was the leader in very high reliable long-lived vacuum tubes and the tubes that we developed were used quite a number of places for the succeeding few years.

MERTZ:

Were there any interesting developments in some of the perhaps ultimately aborted efforts to control the difficulties of an adequate memory for the machine, prior to the coincident current magnetic core, which while perhaps they did not ultimately succeed they represented some, for their development in the state of the art at the time, interesting effort?

FORRESTER:

We put a very large amount of thought, time, effort, and money into the development if a cathode ray tube storage of our own design. At that time the whole design of a computing machine tended to be dominated by what was chosen as the storage; mechanism and so those people that took mercury delay lines had serial machines, those people that took cathode ray tube storage had parallel machines. In the cathode ray tube storage this got its start out of something called the moving target indicator work in radar during World War II, in which it had been discovered that a beam scanning an ordinary cathode ray display tube produced a trail of uneven charge behind it and then if this same line was scanned again one could pick up on a piece of aluminum foil on the outside of the tube, could pick up a signal that represented what had happened in the preceding trace. Radar needed a memory from trace to trace for certain purposes and this method of storage was the first thing that we had tried to use probably with the serial machine ideas but certainly as me moved into the parallel machine we began to concentrate on it and many people stayed with the cathode ray tube pure and simple. It came to be known as the Williams storage tube after a man in Eng1and who wrote severa1 artic1es and probably carried the development of it further earlier than anyone» where a plain cathode

ray tube with a piece of aluminum foil on the outside of the face could be used for storage, was used for storage by a number of projects. But there was no permanence to this storage and one had to continuously rescan and regenerate the image. Otherwise the spray of electrons going on all the time inside the tube wou1d end up ob1iterating the stored information.

Our effort was to avoid this obliteration by constructing a tube, that inherently had stale and self-regenerating storage, and so we produced a tube that had two cathode ray guns. One was the scanning gun which could randomly pick out any point on the tube face and through pulsing of various electrodes could charge positively or negatively that particular-spot and to put it into either of turn conditions, both of which were stable and self-regenerating under the influences of a flood of low energy electrons. So there was a second gun which provided this low energy flood, which would charge up the negative areas (as I recall) and by the excess secondary emission mould also keep positive those areas that were positive. So it was a condition in which the same source of electrons would sustain either of the two stored pieces of information.

Then in the early forms these tubes showed a tendency for the lines of demarcation between the positive and negative areas to creep. In other words, a negative area stayed negative, a positive area stayed positive, but the borderline between them had no inherent reason to stay absolutely stationary. So it would tend to grow or shrink, which ended up obliterating the information. So the next stage in the development of that tube was to lay down a mosaic of very small rectangular conducting patches having the proper secondary emission character with fine insulating lines between. So that this physical demarcation would stop the migration of the boundary. That worked as far as the principle was concerned. However—

MERTZ:

Was that used in the machine prior to the—?

FORRESTER:

Yes. This was used for quite some time and was an essential part of the early work that eventually launched the air defense SAGE system and the Lincoln Laboratory. This was used from the first time that Whirlwind operated, which I suppose, speaking in partia1 terms, was 1949 certain1y if not earlier and it was used in the air defense experiments to which Whirlwind came to be dedicated from the beginning of 1950 until August: of 1953. So for more than 3 years this storage tube was the only basis on which the system operated and was the very critical period in which the early development of the rest of the equipment and the ideas and the computer programs and the field air defense exercises were conducted.

MERTZ:

Who were the men who were primarily active in that area of research with you?

FORRESTER:

There were a number of people in various relationships to it at different times. I think I haven't spoken of Mr. Robert R. Everett who was Associate Director of the laboratory, a tremendously important person in the whole activity. He was, as we said, the author of the block diagram reports but he was one of the princ ipal technical people in the laboratory. He had a very substantial contribution to the storage tube work. Patrick Utes was the principal person in the construction of the tubes and the operation of the manufacturing and experimental facility which was a very major undertaking and a very trying one. The tubes were of relatively short life, having both secondary emission surfaces and cathode ray guns in them. They were complicated to make and they were about 31,500 apiece and they had a life probably of not more than 500 or 1,000 hours, so that it was an extremely expensive operation to keep the Whirlwind computer working and it was always on the verge of not having enough of these tubes on which to function. Even when it was functioning very often there would be defective areas in certain tubes and complications of this sort, but Patrick Youtz was involved. I know that Steve Dodd had a very substantial part in the development of the electrostatic: tubes. Quite a number of other people because we worked on them for a very long period from 1948 to 1953, in various stages, at various stages of development under a tremendous amount of pressure all the time and a substantial fraction of the people in the laboratory had their turn at it at one time or another. Those are the ones that I think probably had—played a continuous role.

MERTZ:

In the development of the coincident current magnetic core, who among your colleagues would you say played a more important role than for instance, some of the other?

FORRESTER:

I had done the development of the idea and some of the first experimental work myself and that had continued from the spring of '49 into the fall of '49 intermittently as I could with the rest of my duties. Then it began to be, I thought promising enough that it justified more than the amount of time I was able to put on it myself. Then the next person into the program was William Papian, who was a graduate student at the time, I think probably started it as a thesis project but also as a research assistant so that he had about full time available and the dual objective of the thesis and a research task. He worked on it, I think pretty much alone, through the Winter of 1949-50 until we began to get more experimental results, became, quite sure of the—

MERTZ:

Was he directly under you?

FORRESTER:

Yes, he was. On this it was a project in which he worked directly for me because it had not become either a sure thing or any kind of official project of the laboratory. In fact it was rather necessary to keep the appearance of competition from demoralizing the work on storage tubes which were always in enough trouble to be inherently demoralizing anyway. So it was done without any great fanfare because we were not very sure nor could we upset the rest of what we were doing. In the spring of 1950 we were beginning to shift from metallic cores, metal tapes wound up into the form of cores, which were the materials first started—expensive, fragile—to the use of ferrites, the ceramic type materials. Somewhere in that period in the Spring to Summer of 1950 we began to see enough promise that we began to expand the program and from there on varying peop1e came into it, playing various roles.

MERTZ:

In the state of the art of ceramics materials technology at that time, do you recall any of the people who seemed to be doing some pioneering efforts in paramagnetic ceramic materials, such that it should then be applied to computer technology?

FORRESTER:

Our shift over from the wound metal cores to the ceramics is quite clear in my mind. It came because of a magazine article which was written by a German ceramicist who worked for the General Ceramics Company in New Jersey. He wrote an article in which he discussed the use of ferrites for transformer cores in television and here one would like to have an isolinear material free of losses and therefore a material that did not have an open hysteresis loop. He was discussing the problems of closing or linearizing this material and showed several different kinds of materials, some of which were bad enough from his viewpoint to be interesting to us as a possibility for the kind of magnetic storage that we were working on.

We contacted them and other people and I remember quite clearly that we wrote to the Phillips and Winhoven people in Holland because they had done a lot of the original theoretical work in ferrites. We asked them what could be done and what was known about the theory of ferrites that would perhaps allow us to develop the kind of rectangular hysteresis loop material that we wanted. The answer from them was that enough was known about the theory to know that it could not be done, which was not encouraging. On the other hand, the evidence of existing material came far enough in the direction in which we were interested that it seemed worth exploring the matter. So we went to the ceramicist in New Jersey and asked him to try to go in the opposite direction from the one he had been and to see if he could find something of use. We began to get materials relatively soon that were experimentally interesting and eventually that were, in fact very usable, that worked in the structure of the coincident current storage system. I think it's

interesting to note that far a period of perhaps two years the only source of materials that were usable, were the ones that he made. The yield was perhaps very low, only a few per cent at best but the manufacturing methods were not expensive so even that was quite a satisfactory process. During all of this time we at MIT were trying to discover what really mattered in the manufacture of ferrites, what ere the variables that had to be controlled. We had controlled furnaces we had so1id state physicists, we had e1ectron microscopes, we had all of the resources that we could bring to bear on this. We probably spent a half million dollars or more in trying to understand the nature of these ferrite materials and all during this time for a period of a year or two, our ceramicist using the art that, he could bring to bear, was the only one who was producing usable materials. I never watched him work but I am told by people who did that he would put his hands in a batch of powder and run it through his fingers and say, that feels like square loop material for me, and proceed to fire it with a very usefu1 yield of material. Of course eventually after we had explored the implications of thirty or so different variables and had thoroughly studied the entire process, we were able to find out what did matter –and get contro1 of the process, improve the materials, and get high yields. But for a very substantial period during which we were deciding to use the materials, we were demonstrating that the idea was feasible, and so forth, they came out of the art, not the science.

MERTZ:

Do you feel that—one of the questions which is I think of some interest and that is the development of the software aspects of Whirlwind. What influences of the general state of software developments at that time, programming which expression certainly precedes the later one, were brought to bear on both the classified at that time and unclassified activities, applications of Whirlwind, say by the time it was fully operative as a computing machine?

FORRESTER:

I think we'll find as we; look into the history that the contributions were very substantial. For two reasons I feel a little unable to be specific. I'm not too well in touch with the vocabu1ary, the exact definitions of computer software today. I also was paying less attention to this particular area e pecia11y the software that went with scientific uses, at that time than I was paying to other issues. I think that his should be discussed with people like Charles Adams, with perhaps Porter, Helwig—some of the peoples who were directly-involved in that. A lot of this happened after there began to be some divergence between the Air Force air defense system application and the scientific computing application. A great deal of the programming that I had direct contact with and some personal knowledge of continued to be machine language programming on a most sort of fundamental or elementary level of programming all the way up through most of the Sage air defense system work, but various higher level programming explorations and actual executions of programs and processes mere carried on with respect to Whirlwind but I don't personally feel that I can identify those satisfactori1y.

MERTZ:

Do you recall who in the defense side of the software period would be perhaps one of the more, knowledgeable individuals?

FORRESTER:

The computer programming work for the defense side was carried on under the direction of Robert Wieser, who is now in the Defense Department in Washington; and by people like Charles Racket, who is with the Mitre Corporation. A number of people that worked in a very substantial sized group.

MERTZ:

Is there a David Israel?

FORRESTER:

David Israel was a very key person in that; he was working directly under Wieser. At the moment I'm having a hard times calling the roster of that group.

MERTZ:

One other aspect of this—do you recall any of the other individua1s who were active in computer work , both in the development of hardware and software, who spent come time at the MIT facility and then went on elsewhere. There was, if I recall correctly, a program which a number of English scientists had, where, they spent at the University of Illinois—they spent summers and some time here at the MIT, some of whom worked or were trained on Whirlwind or programming or other aspects.

FORRESTER:

I'm not able now to list those people. The Whirlwind program was a source of very widespread information. First of all we had publications program far more extensive than I think any other computer group at the time, an internal program of rather massive documentation but also monthly and quarterly reports that were printed in fairly formal set type form, of which two or three hundred copies were informally distributed so that these were widely available. There are ta1ies in the records of the load of visitors that we sustained but my recollection is that they ran 1,500 to 2,000 people a year who visited the project and they visited for varying lengths of time, so that the personnel output came not only through ideas and short visits and longer visits, but then eventually of course the diffusion of the staff members themselves into various other activities.

MERTZ:

In the history of technology one of the most difficult things to attempt accurately and adequately to assess is the interaction—informal interaction of different groups of people and individuals who are working on related and similar projects. With regard to Whirlwind which groups or individuals would you consider to be the one who outside of their context of MIT, either contributed to or received benefit from the development of Whirlwind?

FORRESTER:

Well, contributing to—let's take that direction first. I've already mentioned the early ideas as they came from the work at the University of Pennsylvania, the early planning for the EDVAC computer r we1 before it was actua1y constructed, the early planning for the machine at the Institute for Advanced Study and its contribution to ideas about binary logic and other matters we've discussed. We had access to the work at Harvard, MARK I, but relatively little of the technology was directly useful but certainly it contributed to the ideas of the nature of computation and logic.

On the more physical side I've mentioned the rather small but extremely important contribution from Sylvania on the long life vacuum tube. The magnetic tape units we used I think came from developments at the Raytheon Company. Eventually rather later in the Whirlwind history, in the fifties rather than the forties, we had magnetic drum units that mere picked up from and I think maybe purchased from, but anyway traceable to the work of the Engineering Research Associates in St. Paul that eventually became apart of the Sperry Rand operation. There was participation that did not leave a lasting impact in some photoelectric: information recorders that: were done by Eastman Kodak Company. These were begun before it was clear that magnetic tape digital recording was going to be successful and they faded away after magma tic tape recording was successful.

MERTZ:

May I ask, was that clone under the aegis of MI—under the Whirlwind project..... or was it separately administered by the Navy as a supportive project?

FORRESTER:

I think it started as a subcontract but the formal paper record, whether it was a direct contract or not I don't know, but clearly it was a piece of the Whirlwind program. I mean it was technica1y under our guidance and our coordination and it was very specifically for the Whirlwind computer. Now going in the other direction, once we began to get the 5-digit multiplier and the computer block diagrams and the work of this sort, the information flow I think really reversed and was mostly outward from the laboratory. There was an early magnetic drum computer done by Engineering Research Associates. I'm not sure whether it was the 1101 or had some other number, but in any case the first

magnetic drum parallel computer, general purpose computer done by Engineering Research Associates, followed rather closely the logic of the Whirlwind machine. It had the magnetic drum memory which was more reliable, more practical in some ways. The logic of the machine was quite substantially I believe the logic of the Whirlwind computer with the drum in place of the electrostatic storage tubes.

Then of course the work that the; International Business Machine Company did on the Sage air defense system, which was designed by the group that I headed after it became a part of the Lincoln Laboratory—this had a very substantial impact on components and design practices that ran through their early-work in computers.

MERTZ:

Do you recall—wel1, there is for example Professor Wilkes who was very much interested in the early developments in programming. I don't know, I assume he did visit the Whirlwind computer and perhaps was informally interested in it or involved in its activities, although not formally. Do you recall any visits of other men to Whirlwind who were very impressed and concerned themselves personally in seeing how Whirlwind was developing an supporting it in the sense, of their interest in its development, from outside of MIT itself in the academic wor1d ?

FORRESTER:

I have a hard time, you know, picking any particular person out. Wilkes we knew. He came and went. I think he had more contact with our programming people than anyone else. I have the impression that his computer, his physical computer was without a great deal of technology transfer in either direction, although I could perhaps be wrong. I've mentioned and perhaps more in the programming area. But actually we had such a very heavy stream of visitors, only some of whom I ever even met myself. If you have anyone specifically in mind I might-

MERTZ:

I do recall that perhaps at least once if not several times Von Neumann visited Whirlwind. Of course he was I'm sure quite interested in its development. Do you recall ever meeting him on any occasion when he did visit the facility?

FORRESTER:

Oh, I knew him relatively well. I visited him a number of times at the Institute for Advanced Study. He was up a number of times. In some sense of the word I think we felt in the later stages that we were somewhat, shall we say, competitors in the computer field because we held sharply different views as to the technology, the synchronous logic versus the parallel logic. We had sharply different views as to the importance of reliability. We had very different views as to—

MERTZ:

By reliability you mean marginal checking?

FORRESTER:

Marginal checking, the importance of testing out things, the importance of spending money for developing reliable vacuum tubes 1ike the 5-digit mu1ti p1ier. We insisted on sort of solidifying the ground as far as components and testing and so forth. The tendency at the 'Institute far Advanced Study was more just go ahead and build a machine and I think both of these views mere probably right in the context of what was being attempted. They mere interested in concepts plus the scientific use of a machine which if it mould run for modest amounts of time mould do a great deal of good and reliability was not as important as it is if you're going to control airplanes and move into the real time control. Von Neumann was of course a very major figure. He was older; he was much more a person of reputation than I was at that time. He was also a very helpful person, very willing to discuss things and in my own estimation one of these very rare people who can discuss any subject, even subjects that apparently he has not had any occasion to concern himself with before and quickly in the first 15 minutes come to the heart of the matter and where the ske1etons are buried and what really matters. This is a technique, a perception that one occasionally sees. It is not based on prior knowledge but based on horn to sort of push other people, into the fundamentals of the subject and so in that sense he was a most interesting person to talk to.

MERTZ:

In the history of computer technology at the period of things, preliminary papers done by Goldstein, Von Neumann, Birkes, there is some controversy historically about the—who was the first simulator in the sense of the machine logic and some believe that Professor Mauchly at Pennsylvania in fact was—preceded in the publication, at least in making the field generally aware of those publications of the Institute. I gather from you said that the original conceptualization, at least for the machine logic and for the stimulation was to some extent from the Institute rather than from Mauchly's work.

FORRESTER:

I don't feel—no, I don't want to take a position either way on that because I simply do not know how the concept of general purpose machine operating from its own internal memory really got started. I don't think that we claim any major contribution to that. I think that idea was clearly formed as of the time that we talked to the people about the EDVAC machine. Now I'm distinguishing the Harvard MARK I machine from the EDVAC on this particular point because the MARK I machine, like the Babbage machine, was tap controlled in which the control orders were physically separate from the

numbers on which the machine was operating. Now the use of a common memory in which the machine computes on its own instructions--and you see in modern general purpose machines a great deal of computation is computation on its own instructions and indexing of these instructions and the sorting and rearranging of these instructions. The idea of the machine that operates on orders over which it has control is a concept that I have never traced nor been able to trace the source of.

MERTZ:

Insofar as the awareness at the time of Whirlwind my impression is that the work of Von Neumann was that which was known in terms of the logic. As to whether it was first or not, that it was this work which Whirlwind took cognizance of.

FORRESTER:

I think it probable that we went first—I can't be sure—my first visit to the University of Pennsylvania would have been a visit to the ENIAC Computer, which is not a general purpose computer, although it is vacuum tube. At the moment I'm not clear whether at that point information about the EDVAC computer concepts were available or not. But I rather believe that we obtained the EDVAC computer serial logic kinds of concepts first from Eckert and Mauchly at the University of Pennsylvania, but I don't feel that this tells us anything about where the fundamental ideas came from because it is my understanding that Von Neumann was a consultant to that group at the time and I simply don't know.

MERTZ:

Thank you. Now you remained in charge of the Division Six of the Lincoln Laboratory till 19-

FORRESTER:

Till 1956 when I came to the Management School here.

MERTZ:

I see. What were some of the motives which moved you on into another chapter of your life?

FORRESTER:

I think at that time I had perhaps begun to sense, perhaps not as clearly as I do now, but I had begun to sense that my real interest was in pioneering new technologies or new areas and that, the computer, the electronic computer part, was simply one of these. I don't think I ever looked upon myself as wedded permanently to computers. As a matter of fact, I think you'll see from what we've ail ready said that the computer was a means to an

end. I was trying to first of all develop an aircraft ana1yzer, secondly, military contact information center system and the equipment was always incidental to the purpose. It was necessary but it was for a purpose, I think one of the reasons for leaving Lincoln Laboratory was the way to accomplish that purpose, there didn't seem to be any comparable new horizon opening up that held anywhere near the kind of challenge or promise. Also the entire history of the computer laboratory and especially the Sage air defense system was a history of managing technology as much as it was of creating the physica1 aspects of technology, and these things would not have been done, would never have been accomplished without certain contributions to the management, the courage to do it, the negotiations, the gathering, and the coordination of the efforts of the Air Defense Command. Their materiel command, the International Business Machines Company, the Western Electric and AT&T systems in the early stage, and an extremely active and insistent part on how the structure would be set up that eventually took over in the form of Western Electric, managing under a particular form of Air Force office in New York that made it possible to carry out the system, so that the managerial side without perhaps being very consciously aware of it had been an extremely fundamental part of everything that had been done. Furthermore it progressively made me realize that the successes in technology and the failures are usually managerial successes and failures rather than technical successes and failures.

So you didn't consider it the major break or discontinuity that—most people assume in going from the Lincoln Laboratory down here. I was deciding that it was time to do something else and again it was a series of somewhat happenstance things. A very casual conversation in the hall of Lincoln Laboratory with Jim Killian, MIT, led to his comment that the Management School was here. It had been formed. It was expanding and then for the better part of a year before I left Lincoln Laboratory I. worked with Eli Shapiro who was Associate Dean of the Management School, now at the Harvard Business School, and with Edward Bowles who had been a Professor of Electrical Engineering—at that time was the Consulting Professor was the official title—in the Management School but actually working outside of MIT. But he was fairly close to Eli Shapiro and between the two of us—between the threes of us—we made plans, wrote and rewrote a proposal to the Ford Foundation for funds that eventually led to my coming here and starting what came to be called the field of industrial dynamics and is now broadening out into work that I've recently done in urban dynamics and the possibility of further expansion into the broad fields of dynamics in economic systems, ecological systems, and social dynamics in a broad sense.

[End of interview]