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Interviewer: Richard R. Mertz
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MERTZ:

This is an interview conducted on 20 January 1971 in Princeton, New Jersey, with Professor Julian Bigelow.

BIGELOW:

I am a graduate of MIT in the Communications Engineering Branch, Course 6A, and my first job on leaving MIT was with the Sperry Corporation, now part of the Sperry Rand Corporation, and later to become a very important outfit in the computing machine field. At this first job, I did not work on anything like computers, but worked in the area of various kinds of instruments, particularly the instruments for recording the condition of railroad track, etc., as detected by a car which traveled over the track and searched for flaws in the track itself and searched for balance in evenness of the track.

I had this position for about two and a half years, and then left to go with International Business Machines Corporation at Endicott, New York, which was then my second job. At that time, IBM was a very mechanically oriented company, and the notion of electronic computing was almost repugnant to the organizational set up.

It was not an easy area to initiate within that company at that time, although a number of the problems which I worked on at IBM had to do with bringing electronic supporting equipment to help in their punched card, primarily electromechanical, machines.

MERTZ:

Do you have a date approximately?

BIGELOW:

That was about the first part of 1939 or the latter part of 1938. I'd have to go look it up to see exactly when. In any event, the IBM Corporation was extremely conservative as seen from the inside at that period, and one of the interesting people I met there was, in fact, Howard Aiken, who at that time, was working on the development of a high speed electromechanical relay type of computer, much along the lines of ideas expressed by Babbage in 1820, approximately. However, my connections with the computing field were not primarily through Aiken. Aiken. Aiken was merely an incidental person who happened to there that I met. I had been thinking about such things for a long time, and I be-came more and more interested in them as time went on. And indeed the questions of reliability and the questions of serviceability and the kinds of things that can go wrong in automatic machinery, were extremely well illustrated and demonstrated by my experiences at IBM. It was indeed an excellent background for one wanting to go into computation of a very much higher order of speed.

I stayed with IBM until the beginning of World War II, at which time, if you remember, there was a draft which went through to bring into the armed services. The first version was supposed to be a nine months draft, after which we would be [?] to go back to civilian life, inasmuch as at that time America was not at war. However, I could see the hand-writing on the wall, and I went back to MIT to get my records and credentials with the intention of becoming an aviation cadet officer in the Navy.

However, when I went back to see my Department head, Professor Wildes, he more or less took me by the arm and said that they needed me there at MIT to work with a well known man, Professor Norbert Wiener. As Professor Wildes put it, nobody at MIT knew whether what Wiener was saying was sensible or not or feasible or not, and they were looking for a man with an engineering and mathematical background who would serve as an interpreter and serve as a colleague for Norbert Wiener.

So instead of going into the services at that time, around 1940, I went back to MIT and became a research associate. I worked for the next three years on a classified military program with Wiener, the purpose of which was to work on computational methods of predicting airplane curved flight in space, from data observed by ground observation instruments. This was indeed a computational problem of both mathematical and engineering interest, and I learned a great deal from working on this and felt a much greater grasp of the problems both mathematically and instrumentally as a result of that three year period of work.

In that period of work, I also invented and built a variety of curved flight tracking computer which was built in our own laboratory by myself and three assistants, and which is described in an MIT Division of Industrial Cooperations memorandum of that time. Beyond this, I worked on and designed a special electrical computer which was intended to do autocorrelations, multiple autocorrelations at high speed, in a step wise, continuous variable digital fashion. This machine was in fact built by the American Machine and Foundry Corporation, though it never was entirely successful, for primarily reasons of mechanical limitations of the design of the equipment, rather than electrical reasons. However, the machine indeed was built and was an automatic lag correlator capable of correlating two time series of a thousand observations each under all lags from zero to 1,000 in about two minutes.

Next, the project in which I was associated with Wiener seemed to be coming to a close, partly because in 1943, it was clear that the War had passed over the hump and that radical new computational equipment would not be possible to be produced and gotten into the field in time to influence the War effort.

I then left MIT and went to work in the Applied Mathematics Panel of the National Defense Research Council, Applied Mathematics Group, at Columbia where I remained for approximately two and a half years, until the War ended in the latter part of 1945.

MERTZ:

At this time, was Professor Murray at all associated with ...?

BIGELOW:

F. J. Murray was a member of the same group that I was with, but a different section of it. There were two sections. One of them was the mathematical section and the other was the statistical research section. I actually was in the statistical research section, but I met F. J. Murray at that time, and he became interested in ... However, his interest in computing per se, I think, really began almost a decade later. Murray was here at the Institute for Advanced Study. He wrote several papers with John Von Neumann, and I think after that period of time in fact, after 1950, was about the time when Murray became interested. However, Murray was at Columbia at the time I was there.

MERTZ:

With whom did you work when you were at Columbia?

BIGELOW:

I didn't work with anybody. I was Associate Director of this Statistical War Research group. The official investigator of the statistical project was Professor Harold Hotelling. However, Professor Hotelling was in evidence only for a short time one day a week, and the group was run by the four Associate Directors, who included a number of people that later became quite well known. One of the Associate Directors was Professor W. A. Wallace, who is now President of the University of Rochester, I believe. A second one is Milton Friedman, who has recently became famous in connection with the monetary control problem and his advice to President Nixon, and is Professor at the University of Chicago. A third one is Professor Jacob Lokowitz, a well known statistician, who I believe, has remained at Columbia, but he was at Cornell and some other places for a while. The fourth one was myself. We were the Associate Directors of this group, and to a large extent, we ran it.

In any event, at the end of the War, Columbia University disbanded its War Research group in a rather precipitous fashion, and some of us, including myself were left high and dry. At that time, John Von Neumann, whom I happened to have met during the War on a visit to my project with Norbert Wiener, the project Norbert Wiener and I had jointly, had come by as a visitor. Von Neumann became very much interested in building a really modern computer, and in fact, communicated with Wiener to find a person who might be a suitable one to head up the actual machine development group for this effort. I'll return to this question from a different standpoint in a minute. Wiener gave him two names, one of which was my own, and in April of 1946. I came down to see Von Neumann at Princeton. He and I had a long talk in his home on Wescott Road, and it was a very enjoyable experience from my standpoint. We agreed that I would take on the job under him, of building this modern computer, and in June of 1946, I came from New York to the Institute for Advanced Study, where I have remained ever since. I was an employee until approximately 1949, at which time my status was changed to that of a member of the faculty of mathematics, which is as a permanent member of the faculty of mathematics.

Now, to come back to the situation at the Institute for Advanced Study. I think it's important to mention that Von Neumann had been involved in many kind of computations during World War II. He was an extremely flexible and competent man in many different branches of scientific activity, ranging from physics through ballistic computation and other quasi engineering areas, all the way down to the actual engineering and design of various kinds of ordnance and physical apparatus for warfare. He was involved in many different kinds of military computations and military advisory functions during World War II.

At this time, he had come to use the early model of Aiken machines, I think the Mark I and possibly the Mark II were in operation in some sense before the end of World War II, but also the ENIAC had been started in the middle of World War II, and there was some computation done on that in the latter part of World War II. That is, some computations had been done around 1945. Among other things, Von Neumann immediately saw the strength and the weaknesses of this kind of effort and saw that it could be pushed much further, and on a much more effective scale by suitable conceptual changes in the machines which then existed, about which I will mention some facts further on.

MERTZ:

At about this time, do you happen to recall whether or not some thought had been give to Von Neumann's going to MIT?

BIGELOW:

Yes. In fact, during that period of time I'd been at the Institute for Advanced Study, when Von Neumann was alive, I'd like to go on record as saying that approximately twice a year, there were very strong positive rumors that Von Neumann was going somewhere and had practically signed up to go there. So that I'm sure that he had considered going to MIT at various times. These rumors, however, one learned to understand, were largely a part of negotiations which a very complicated and effective man engaged in, to be sure that he was doing the right thing at the right place at the right time. To some extent, I think that they were a part of a bargaining way of life which Von Neumann, so to speak, managed with his left little finger, while the other fingers on his hands were doing more effective and important work.

I do know that at various times in the course of the effort at the Institute for Advanced Study, obstructions seemed to come up in the way of what Von Neumann wanted to accomplish. For example, the actual building of the computer here was an unprecedented departure from anything else which had ever been done at the Institute for Advanced Study. In fact, there is nothing vaguely resembling laboratory work here at the Institute, nor has been since that time, and there was nothing of that sort before that time, and there was almost, I would say, a loathing on the part of the ivory tower component of the academic community that anything should take place.

In fact, there were some extremely laughable episodes which took place between the computer group here and the faculty, as the result of such misunderstandings. Not only that, the townspeople of Princeton, at one time, objected to the existence of a computer laboratory here because they thought the computer would make too much noise when it operated, and would bother people nearby. I need say no more about the state of misunderstanding of the effort at that time.

I'd like to go back, however, to the period of 1946. When I came in June of 1946, I came because, as I said, I had had a conference with Von Neumann at which I agreed to come, but it also became apparent to me, and I learned in fact from Von Neumann himself soon thereafter, that originally he in-tended to have the computer built by Presper Eckert, of Eckert and Mauchly. That the originally preliminary contract negotiations to get support for the computer had in fact planned to do it with people of Eckert's group, including Eckert himself. Subsequently, because the field was then growing with almost explosive rapidity, I suppose, Eckert became difficult to get a hold of, and did not show up at the agreed upon time, which made Von Neumann quite irritated. As negotiations went on and delay went on, Von Neumann came to the conclusion that Eckert was not indeed coming down at all, but was going to try and handle this entire project by some kind of a proxy operation, at which time he started looking around for somebody else, which was how I came into the scheme.

But I would comment that in mid-1946, there was an enormous amount of activity in at least a dozen, or perhaps possibly eight centers, aimed at producing computing machines. To the best of my knowledge, the ones which were going on included Project Whirlwind at MIT, and three different groups in England, one of which was the group trying to build the machine called the ACE. The second was a group, Telecommunications Research Establishment, TRE in England, at Great Marlbourn. The third was a group under Maurice Wilkes at Cambridge University, and indeed there was a small fourth group who were not, I think, seriously engaged in building a full scale machine, but were working on some very small models under A. D. Booth, Donald Booth at Birkbeck College, University of London. Shortly after that, a group at Manchester, under Kilbourne and Williams, in fact, built a small machine out of war surplus parts, and I shall return to that later on, because our first machine used the memory tube which Williams and Kilbourne had developed.

In any event, that's four outfits I guess I have named in England. There was also a machine being built in either Switzerland or Germany, I forget which, I have to look it up. It was called the Zuse, which was a relay machine, a high speed relay machine.

MERTZ:

This was under Konrad Zuse?

BIGELOW:

Yes. German, but I don't know that it was built in Germany. I believe it was built in Zurich. It was a German group, that's right. All of these things were happening at that time. At MIT, there was great activity, which in our thinking down here, was parallel to that of the ACE group in England. I'm outlining this to you not because you don't know a lot about MIT, but I'm trying to give it to you from what our point of view was, which may have some historic interest. The group in England, in fact, consisted of some very competent people. It consisted of Harry Huskey, and A.M. Turing, and Womersley ...

MERTZ:

Was this the ACE group?

BIGELOW:

Yes. It consisted of a fellow named Womersley, who was a very well known applied mathematician, and Fox, who is now, I think, in America, in the Bureau of Standards. I think that's the same Fox who was originally in England. There were two or three other well known mathematicians or applied people of various sorts in the group.

Now the reason that I say that we viewed the English group and the Whirlwind group as being comparable, is that the English group first started to build a pilot model of a very large and elaborate machine, which they had designed on paper. They designed the machine essentially in a box diagram, flow-like arrangement, and the plans for it consisted of iterations of certain modules over and over again. The machine which they actually started to construct, in fact, was a pilot model, so that the main reports one sees in literature, and some of these still are around, are for the construction of the pilot model of the ACE.

Similarly to this, the Whirlwind group generated some very expansive plans for a very large machine, of which the Whirlwind I itself was in fact the pilot model. And their effort was focused on building a pilot model of a main machine, which I think never was in fact finished. So that these are quite interesting comparisons and examples from which one should learn something of historic worth. Indeed, this was the same kind of a situation that Babbage found himself in. He built a pilot model of his main machine. His pilot model worked well, and he in fact, did have physical and mental collapse and financial collapse in trying to execute the main machine of which the pilot model had been developed. So this is a rather interesting aspect of the history of ...

MERTZ:

If I might go back to the time at which you joined the Von Neumann group. The contract, I gather, had already been let to construct the machine?

BIGELOW:

I think that the actual thing hadn't been signed, but that certain agencies had said that they would sponsor it, and it was ready for signature.

MERTZ:

Do you know whether he planned to use any of the personnel from the Moore School, even though Eckert's cooperation was not forthcoming at this time?

BIGELOW:

I have to be quite frank about that. Von Neumann was the kind of man who never overlooked any angle of rendering insurance to the success of his goals. When I first came here, he explained to me that there was a man here, who would arrive shortly, whose name was Simms by the way, who was from the Eckert-Mauchly group, because he had already made some agreement with them, and Simms was going to be sent down in accordance with that agreement. It's my feeling that Von Neumann did not know for sure what he would be getting when he got me, and so he did not completely terminate the verbal agreement or the understanding that the had with Eckert and Mauchly, until something like six months later. Six months later, I think that he began to see that we were going to go ahead and get going in a fairly effective fashion. So that at that time, he severed the connection.

In fact, at that time, there were two or three people borrowed from Eckert and Mauchly, but there were also four people equally competent, or perhaps even better, that we had picked up ourselves. The Eckert-Mauchly people eventually split off and went back to different occupations by themselves.

MERTZ:

Now in terms of personnel, how many were there with him at the time you joined?

BIGELOW:

There was nobody here. He had in fact agreed to take on about three people, but I think none of them were here. However, within the first month that I was here, we had three people here. We had a fellow named Davis, a fellow named Shore, and a fellow named Simms from the Eckert-Mauchly group, within about thirty days or so.

MERTZ:

All three of them were from ...

BIGELOW:

They were at one time and in some sense associated with the Moore School of Electrical Engineering, which is where Eckert got his training. Eckert and Mauchly both had one foot in the Moore School, and one foot in the company that they were trying to start. So these people who came from the Moore School, were in some sense, associated with them, or sent down as their contribution or something of that sort, at a very early stage of the game.

MERTZ:

And then, there was yourself and Von Neumann.

BIGELOW:

At the time I came, none of them were here Simms showed up within a matter of a few weeks. The other too Davis and Shore, came within the matter of a month. Meanwhile, three extremely good people from entirely other sources came here. They were Willis Ware, who is now at the Rand Corporation. He was formerly, I think, the director of their computer efforts there, some of the machine efforts themselves. There was a man named Pomerene, who is now quite well known in IBM. And indeed, I've been talking only about the engineering group. I should also mention that Goldstine was here when I came, and also Arthur Burks.

MERTZ:

I was wondering, were they primarily mathematical?

BIGELOW:

They had a hand in the mathematical and coding problems. In fact, Goldstine's function was to head up the coding groups. As the machine grew, the coding would grow along with it, and our capability would grow along with it.

MERTZ:

To get the chronology relatively correct here, Goldstine and Burks were already here?

BIGELOW:

Goldstine and Burks had been here for approximately six months when I came. Goldstine had some connection with Von Neumann for longer than that, because Goldstine had done computations on the ENIAC under Von Neumann's direction, I believe. So Goldstine was also sort of associated with the ENIAC project, I think, when he was a Lieutenant or Captain in the Army, or something of that sort.

MERTZ:

Yes, I believe he was in ballistics, counterpart to follow the work in the military.

BIGELOW:

Right.

MERTZ:

And was Pomerene already there, or did he show up ...?

BIGELOW:

Pomerene came about a month after I was bare. Pomerene, Ware, and Ralph Slutz were among the first people we hired. I believe that Pomerene and Ware were with a company who had been making transponders. In those times, they were called IFF, which means Interception, Friend or Foe. They were a device which was carried in a military plane, such that when you showed radar on it, it gave you back a coded signal, saying whether that was one of your planes or the enemy's. I used to be able to know the company that had developed this, but Pomerene and Ware had both worked in their development laboratory, so that they had familiarity with pulse and non continuous signal, random high frequency work, but not on computers.

The same is true of Slutz. Slutz, I think, had recently finished his doctoral thesis at [?] Laboratory in physics.

MERTZ:

He came in a little bit after Pomerene?

BIGELOW:

He came, I think, a month or so after Pomerene and Ware came in.

MERTZ:

This was essentially then, those who were primarily interested in the physical organization of the computer?

BIGELOW:

Yes. There were the people who were going to build the actual machine.

MERTZ:

This was in the summer?

BIGELOW:

This was from June of 1946, on through that first summer. By something like July or August, we had most of the team that we did main part of the logic work with, that subsequent fall. In the fall of 1946, we had begun to work out, pretty concretely, what the machine would look like from the standpoint of the central arithematics unit.

There was one other person whose name I should mention, and I'll have to think a moment to think of it--Richard Snyder. Richard Snyder came also, I think in approximately September. He was a little bit older than the rest of the group, and he had also done a lot of experimental work on circuit design, but nothing in connection with digital computers.

Now the first part of the effort was set up, in fact, in the cellar, the boiler room of the Institute for Advanced Study. There was no space for us, and so for the first five or six months, we were crowded into the boiler room with a few work benches we set out. There was not even an office for me to go to and hide, and think about circuit logic, without having people walking all over my desk and crawling all over me and so forth.

However, we persevered, and then I think, just about the first of 1947 or the latter part of 1946, this present computer building had been started, and was available in the early spring of 1947 for occupancy.

MERTZ:

Would you say that the areas of friction from those who were purely theoretically disposed among the Institute faculty, when the project was removed to its present location, that this was reduced somewhat?

BIGELOW:

It reduced the tension somewhat. However, it is quite difficult to convey, without sounding ludicrous, the degree to which this became a problem. Among other things, the people who worked here got such ridiculous salaries as five thousand dollars a year. Many of these people, who were good electro-technologists, had, for example, only a bachelor's degree, whereas the Institute itself had visiting members with PhDs from four or five important universities in the world, and who came over here for fellowship stipends which amounted to 2,500 or 3,000. So there were really substantive kinds of jealousy areas from this.

There was that, but there was also the complete lack of comprehension. In fact, in the Institute for Advanced Study, this still prevails today. The whole Institute does not understand what the computer revolution has been, with the exception of a very small number of physicists and others over there who have had direct contact with it, can understand it. The faculty itself is completely reactionary to this sort of thing.

MERTZ:

Well, trying to set up a computer in the boiler room of a rather theoretical...

BIGELOW:

Well, we were not setting up the computer there. We were trying to work on the components.

MERTZ:

I take it you had drawing boards and ...

BIGELOW:

Yes, laboratory facilities, sure.

MERTZ:

This was not the most auspicious place to begin ... equipment ... power...

BIGELOW:

It certainly was not. However, I would comment that the people who objected most would have to go and look for us to know that we were there. Most of them never did. Most of them did not stick their heads in the doorway, so it wasn't that. I was just the principle of the thing that the Institute for Advanced Study is a quiet, remote place, with no contact with the real world, and here were all these busybodies coming in who seemed to know what they were trying to do, whereas we had to think about what we were trying to do.

MERTZ:

I'm sure Von Neumann had a number of initial difficulties in trying to design a machine in this setting, as distinct from, let's say, a department of electrical engineering which has a lot more traditional climate, as it is in MIT.

BIGELOW:

Very likely.

MERTZ:

There literally wasn't anything on which to build here.

BIGELOW:

That's right. We had, in fact, to build our own work benches. We bought lumber and built them. It's hard to remember all the details, but for example, at that time, building materials were under rationing. You could not go out and build yourself a house or a garage or something. If you wanted a few feet of wood, you had to have a certificate for it, much less things like workbenches and hardware and tools.

MERTZ:

Those had to be surplus.

BIGELOW:

We got some stuff from surplus, and we got some stuff by getting the appropriate priorities for it. Directly after World War II, in the period from 1946 to 1950, materials were so scarce that you couldn't build anything without having a lot of red tape and going through a lot of channels to do it. So we obtained whatever lumber we could, and we obtained a bunch of second had hardware and built our own tables. Then we couldn't obtain power supplies, so we obtained surplus components and built our own power supplies, etc. We really worked from the ground up.

However, it did have the advantage that during the course of this sort of thing, which as to some extent routine work, I had some better opportunity to see what the different people in the group were capable of and to get a feeling for how they would work together, and to plow ahead towards important directions we were going to work in. So it served a purpose, thought we had nothing to begin with.

MERTZ:

Then when this building was in condition to be moved into, did the boiler room sort of get abandoned?

BIGELOW:

Yes. We abandoned it and we shifted everything over to this side. There were many laughable things even here. I can hardly resist describing one of them, because it shows the degree to which short sightedness can cause troubles. This building was built in two pieces, and the first piece of money was obtained, and built the present section that you are sitting in, which was sort of an odd shaped piece. The large room at the furthest end from in here, was where our laboratory was. There was a small boiler room, wash room, and things like that, and about a dozen offices.

When that piece was built and livable, it soon became evident that more space would be needed. Meanwhile, in order to run our first version of the machine, of which we had a very large part operating, the main arithmetic system and a lot of the logic and so forth, was operable in 1948. Because of the difficulty of getting satisfactorily regulated voltages to do the job, I decided that we would do something by brut force, namely we'd just get a large number of storage batteries. We built a small battery house, we were about to build a small battery house, when this sequence of errors that I was interested in telling you about, started.

The simplest way to get a steady voltage with essentially a very smooth behavior and good reliability, in case the power lines went off and you could charge it overnight, and be ready next day, would be to have two or three banks of two or three hundred volts of direct current storage batteries. So the question came to me as to where we were going to put them, and we had to build a small house. Now, out in the middle of the back yard, over my objections, they had buried a very large oil tank, which cost perhaps a thousand dollars to stick in the ground, and it was at the logical place where the battery house ought to be. So that I had suggested that we move the oil tank, and this was decided against, voted down. So we didn't move the oil tank. We put the battery house off to one side, and in doing so, we blocked the logical position for a corridor, which would be the extension of the building, for the next part of the building, which would have to be built eventually.

Now before making this objection, before the decision not to move the one thousand dollar oil tank was made, I had had a young mechanical engineer who was working for me, lay out how the building, which was then rather odd in appearance and shape, could be extended into a symmetric, sort of an H structure, with a main entrance and a main archway and a natural flow into the corridor areas, and which made some architectural sense, which the present one does not. However, nobody at the main building was interested in this particular feat of architecture at that time, and the contracting agent said to build it in the cheapest way possible. So we did it in the cheapest way. We put the battery house right where the corridor should be, because a thousand dollars could not be made available for moving the oil tank. Then when we went to build the extension, it turned out that we had to put the extension off to one side, in a grotesque way, so when you come in the front door, you can't see that there isn't any other corridor. So that ninety thousand dollar extension was put on in a crazy position because the one thousand dollar oil tank was never moved. Now, twenty years later, we have a peculiar shaped building with no architectural integrity, and is rather useless to the Institute, excepting that it does have space in it. This is how things very often go.

I won't dwell on that any longer. I was talking about the efforts that were going on in the country. One of the main efforts that was going on, of course, was an effort at MIT, and I compared it with the ACE project which I won't dwell on any longer except to say--and then to turn to some of the other efforts in the country--that the Eckert and Mauchly Company was then about that time, becoming more solidly formed. It was beginning to work on the first UNIVAC machines, which were to be the commercial products which they had developed out of the ENVAC basic ideas, using the supersonic mercury tank delay system for the memory. Their machines did not reach the market until about 1950 or 1951, but certainly, the early planning and primary work was going on.

There were also two or three other places. There was a group who originally were called ERA, Electronic Research Associates, who were out in the Minneapolis - St. Paul area, who began quite early to work on magnetic drums, which as high relevance to computers. Eventually, a group split off from them, and produced a very small digital differential analyzer known as the MADIDDA, which I'm sure you've run across some trace of. Let me think about all the places there were. I'm merely trying to paint the picture that it was obvious at that time, that the machines were going to be built, and that various interpretations of how they would be built were being kicked around.

MERTZ:

I think some ideas for the Mark III, for example, at Harvard, ... We're talking about 1947 now?

BIGELOW:

The period of 1946 to 1947, yes. I think the Mark III was being planned at that time and that was the closest that Aiken could get to a fully electronic machine at that time. Surprisingly, not even then did Aiken fully realize the importance of flexibility of stored program techniques in these machines, though it was becoming apparent.

IBM had also begun a gigantic machine called the SSEC, Stored Sequence Electronic Computer, which I won't bother to discuss, excepting to point out that it had something like 20,000 vacuum tubes and 200,000 magnetic relays in its system. Eventually, it did not do very much serious computing, for various reasons.

MERTZ:

Do you recall ... was this something perhaps under discussion when, as you indicated, Von Neumann always liked to hedge his bets by having alternative course of action available to him, when it seemed doubtful that he was going to be able to use the Moore School as the place to realize the hardware, did he give any consideration to the engineering school here at Princeton?

BIGELOW:

I think there was some thought about it.

MERTZ:

Or to David Sarnoff at RCA?

BIGELOW:

Well, you see, among other things, both of these looked somewhat pessimistic at that time, even quite early in the game. One of the facts of the matter is that you need a great deal of mission orientation, if I can use this cliche from military projects. You need to have some people who really feel that getting this piece of equipment built along these lines, is the most important problem to be solved in the four walls that you're working in. Now in the Department of Electrical Engineering, this tends not to be the case. It tends to be that the case of producing Ph.D. theses, and what not takes some precedence. You need a rather monolithic control of the project. You need dedication. You need people who will work until 1:00 a.m., when you try to do something, and this is only obtained under somewhat special motivation which you might get better if you have your team separate.

Secondly, the Department at the University was really not strong enough, I think, at that time, to have set up an effort of this sort to one side. There have subsequently been excellent electronic people at the University, and it has a very fine and strong department now. At that time, however, it was somehow out of date, and it was mostly a power engineering group. It was much more out of date, for example, than the corresponding departments at MIT, who had emphasized electronics and in fact, had a communications option way back in the early 1930's, which was quite a radical step for them.

I remember when I was at MIT, the electronics and radio work which had been going on was considered rather a suspect and maybe a frivolous thing to be doing that one should really be designing large generators or at least a large are discharge thyratron or something. If one really worked in the communications branch, it was considered a little bit frivolous.

I have a slight anecdote in that direction, if you're interested in it. One of my classmates was a man named Alexander Rufus Applegarth. I remember the name well because it was an unusual name even when he was a student. Applegarth was as a freshman and as a sophomore at MIT, obviously a genius in radio circuitry. In fact, in his room at the dormitories, there were piles upon piles of amplifiers, circuits, transmitters, receivers, and all kinds of things. His desk consisted of a huge mess of pieces of scrap paper from a pad on which he generated example after example of super heterodyne receivers with different kinds of detection systems, etc., using existing tubes and technology and transformers. If anybody wanted to discuss any circuit, whether they could do it, Rufus was the man we could go and talk to.

[End of Side One, Tape One]

BIGELOW:

It's my impression that he (Applegarth) was the best informed man on actual electronic communications and radio equipment that I've ever seen. That, even at that time, he was so conversant that he was inventing circuits all over the place. It's my impression that he was a misfit in the MIT Engineering Department environment, and he enrolled in a course, which like 6A that I stayed in, eventually was aimed to give him a master's degree. I think before it got finished, he began to see that this was really nonsense as far as he was concerned, and that he had very little more to learn.

It's my impression that he left early. At any event, he ended up as the Chief Engineer and Head of Research at NARCO, which is the National Aeronautical Radio Corporation located down in Pennsylvania. I think he is now one of the wealthiest and most successful of anyone I knew at MIT. I say this because this never would have happened, had MIT recognized fully that the communications aspects of engineering was shortly to be very much more important than all the others put together. And Applegarth was an example of this.

MERTZ:

Yes. Well, apropos of this, though, do you know whether Von Neumann gave much thought to the possibility of using personnel at what was to become the David Sarnoff Research Center? He did have, I believe, contacts with Jan Rajchman.

BIGELOW:

Yes, indeed he did, and very shortly after I came here, we had been going over to Rajchman and Zvorich, who were the key people over there. Rajchman had been working on various kinds of memory storage tubes, one of which was subsequently called the selectron tube. I won't go into the details of this, but it was the idea that you would make an array of electrodes inside a single envelope on which you could, by applying through ingenious combinatorics, different voltages on a small number of electrodes which formed an X and Y coordinate system, pick out the cell you wanted to store in, and operate in it. This effort in the early stages, appeared as if it might be the solution to the memory problem, and so our group made an agreement with RCA, that RCA would in fact build the memory system for the machine. We would build the arithmetic and logical componentry to operate the rest of it, and the two would be married sometime like 1949.

MERTZ:

Was this fairly early on?

BIGELOW:

This was done certainly by 1 January 1947. This was a fairly concrete agreement. Rajchman already had some very small models of this with one or two cells operating. It looked quite strong at that time, that this would be a good solution. It would be fast, convenient, and we could, so to speak, but them right off RCA's shelf. So we gave them some support, and we stayed away from the question of how to build a memory, until we had settled all the other problems we were working with.

However, soon after the agreement, we had been going over to talk to people at RCA. Von Neumann, Goldstine, and I went usually as a group together, though very often, Von Neumann and I went as a team, and sometimes I went alone, to see how they were doing, on approximately monthly visits. Some reasonably short time later, like two or three months, I began to have serious doubts--and I think it's fair to say this is not 20-20 hindsight some years later-- that the Rajchman selectron scheme would ever work in practice. The reason for these doubts was, that quite early in the game, I realized that the number of design parameters, statistical parameters, one had to keep constant in order to make in a single envelope, say, 4,000 memory cells, reliable enough to be treated alike and to be accessed by a common set of coordinate wires on the outside, was going to be an enormously difficult manufacturing control problem.

I had just come from the Statistical War Research group at Columbia. One of the last jobs I worked on was an interesting mathematical problem related to the statistical reliability of rocket propellants, which was done by this applied mathematics group, for the Sunflower Ordnance Works of the Herculese Power Company, and for the Army Air Force and others, because there had been a lot of accidental explosions of rocket propellant units on airplanes in which the explosion would take the wing off a plane. And this would happen in a very rare and erratic fashion. So we had some excellent people in statistics there, including no less than Abraham Wald, who founded sequential analysis while working with our group. Statistical thinking had become a part of my way of thinking about life.

MERTZ:

Was Wald with your group?

BIGELOW:

Wald was with the group, that's right. He worked under us, believe it or not. He was the most gifted statistician, I would say now, on the whole team, but he was working for our people.

MERTZ:

He was also the inventor of the value matrix.

BIGELOW:

That's right. He was the inventor of a lot of things. He was also a collaborator with Von Neumann in an interesting mathematical group in Vienna, quite early, that was done under the direction of Karl Menger, the famous mathematician, for a while at Notre Dame, and this was called the Vienna Kreis, Vienna Circle, Viennese group of people, and John Von Neumann was in that. Abraham Wald, Karl Menger, the mathematician named Neubling, and Godel, who is a famous logician here at the Institute for Advanced Study, were all together for one year at Lichtenstein, yes. I can give you exactly the people, because I have a copy of the Admissa reports that they put out. Anyhow, Wald was that caliber of man, and he was at Columbia as a consultant to our statistical group.

So to come back to the anecdote that I was relating, I had been so immersed in the statistical aspects of war work, that thinking statistically and become part of my repertoire. I had done a number of kinds if simple computations, which showed that if Rajchman was getting four or five defective cells from a small sample of his memory system, that if one extended the size of the sample, then the probability of finishing one glass envelope containing 4,096 of these, of which all worked successfully, was indeed astronomically small.

One would have to change the statistics of reliability of the thing, or change its design, or partition it much smaller. Rajchman, though one of the most gifted people I have ever known, had a blind spot. He had an inability to understand this, to accept it or to believe it, as some of us pointed this out. Von Neumann, of course, understood it instantly, because his mind was faster than anybody's, and he saw things more clearly than most people of that era.

In any event, I became somewhat skeptical of the success of the selectron tube, and indeed, by the summer of--let me try and be careful now --by the summer of 1947, we had built and essentially solved the problem of building the arithmetic part of the machine, inasmuch as we had a set-up in which there were essentially ten digits of the adder-subtracter-multiplier-diver unit operating. The whole thing was to be forty, and this ten digit array was a breadboard. By the summer of 1947, it was clear that this was a successful piece of componentry, and we turned more seriously to the question of how to realize a connection with a memory system.

I'll have to check dates more exactly for this manuscript. I have to get them from the top of my head now. In any event, we then heard also of the work of F.C. Williams at

Manchester, who had, as I say, built from war surplus radar parts, a very small serial type computer. Ours at the Institute was to be parallel. We heard that he had it running in Manchester, with two memory tubes of a special storage type, which would depend upon only standard cathode ray tube components to make them work.

By the way, one of the reasons our group was successful, and we felt we got a big jump on others, was that we set up certain limited objectives within the group, namely that we would not produce any new elementary components. We would not try and produce a memory tube, we would not try and produce special logical circuit elements, but we would try and use the ones which were available for standard communications purposes. We chose, so to speak, vacuum tubes which were in mass production, and very common types, so that we could hope to get reliable components, and not have to go into component research. Whereas, Forrester's group spent an enormous amount. In fact, they spent something like 25 million on storage tubes alone, which is nearly ten times our total project.

MERTZ:

Was there much thought given to the initial electrostatic storage tube that was being developed at MIT?

BIGELOW:

Yes. We went and studied it, and I also, as I was about to say, went to England, and saw the Williams tube working over there. We concluded that this technique would work satisfactorily. We felt that Forrester and company were doing interesting work, but not really what they were supposed to be doing. Namely, they were developing fundamental new components which eventually probably would be very good, but meanwhile, they were not really pressing the computing machine area, which they were supposed to be doing, on the basis that we were.

MERTZ:

Was this in 1947, when you went on this trip?

BIGELOW:

I'll have to check the date. It's possible that I may be off, but I know it was in the summer. It was either in the summer of 1947 or the summer of 1948. I can check this by some records I have.

MERTZ:

One of the questions I have is, did you by any chance, produce a written report of your experience, or of your evaluation of what you saw at the facilities you visited at

Manchester?

BIGELOW:

I probably did, but whether it still exists or not, I'd have to have some time to look. Unfortunately, when Von Neumann died, there was a huge cleanout of his office. He had an enormous system of files, and he had a very efficient full-time secretary. The whole side of his office was covered with file cabinets. There was something like 15 of them, four drawers high, 15 wide, the most enormous bank of files you ever saw. His secretary, who for a long time was Mrs. Delsasso, used to keep separate files on these things, but also on many other activities he was in.

When Von Neumann died, his files stood around for quite a while. The classified ones were taken out and destroyed. The unclassified ones sat there, and nobody seemed to know what to do with them or want to do much with them, and an enormous amount of material was thrown out. At one time, I went over and ransacked for some certain things that I might be interested in, and I do have someplace, a smallish amount of material which I felt worth saving, which I can sometime look into for you. I don't know whether or not he actually kept my diary. I now I made a diary on this trip.

I visited Manchester, I visited Boothe in Birkbeck College, and I visited Wilkes at Cambridge. I took notes on each of these and brought them back. Wilkes himself had started a machine at that time, and it was also a mercury type machine.

MERTZ:

How about the ACE group?

BIGELOW:

I visited the ACE group, right.

MERTZ:

Now in chronology. By 1947, if I'm not mistaken, one of the sort of intellectual offspring of the progeny of the institute computer, was being either conceived, or had been conceived, as something that was going to be developed in the manner of, or in the school of, the Institute computer, I believe, at the University of Illinois.

BIGELOW:

Yes. Well, when our group really started to fly--let me see if I can put that in the correct perspective. The first period from June of 1946 through 1 January, more or less, of 1947, we were feeling our way, and groping, and we made a lot of block diagrams, but they were not reduced to practice in any sense.

In the first half of 1947, we built, in a sort of a breadboard sense, some basically new kinds of circuitry. I think I mentioned to you once before that our whole machine was interesting and different, in that it was a direct couple machine, which may not mean much to an historian, but to technologists is rather a startling thing. Almost no machines since then have been built which are direct coupled. In some sense, the point is that you don't look at the shape of a pulse wave, but you only measure whether its height is greater or lower than something. This is a subtle point, but the whole circuitry which followed from it became of a radically different sort. The simplicity of the circuitry was something that people could not believe, until they really saw how simple it turned out to be. It also had the property that you could find faults in it much more rapidly than with pulse circuitry. Because, if fact, if you simply put steady voltages on different signal buses, the information stood right there statically and you could look at it. There were many aspects as to why this was important to do, and there are many technological reasons why nobody had imitated it since then. It was a radically different kind of machine.

In any event, the basic circuitry we had ironed out, and we had produced a prototype of enough of the arithmetic unit from the first of January of 1947, until approximately mid-summer of 1947, so that we knew from the summer on that we could demonstrate addition, subtraction, etc., etc., at that time.

Then other groups became interested when Von Neumann, more or less, spread the word around that we were succeeding. His confidence in the group also climbed enormously. I think about that time, he cut loose any relationship that he might be planning, because the Eckert-Mauchly people wanted in, and, so to speak, underwrote the project. Groups who were set up to build similar machines at the University of Illinois, at Aberdeen Proving Ground, at the Los Alamos Nuclear Center in New Mexico, at Aberdeen, Illinois, Rand Corporation, and there were one or two more. Oh, yes, the Argonne National Laboratory, near Chicago, was another one. I think there were six in all, but I can't remember the other one right now. These were all of them supported as similar projects like ours.

Indeed, the heads of two or three of these groups came to the Institute about the earlier part of 1947. Wilkes was such a person who came here, and also Ralph Meagher from Illinois came here and visited the Institute, and was working like a member of the group for a few months, and then went back to start his effort. Other people sent representatives up here.

As it began to look as if we would succeed, and we were able to compute, estimate, how many tubes and parts, and how fast our machine would go, Von Neumann became more and more enthusiastic. He became able to, at that time, estimate that the completed machine had less than 2,500 tubes, and that it would basically be able to add, as far as arithmetic numbers were concerned, two twenty-digit numbers in ten microseconds. Those speeds and performances left almost no problems to solve, except in getting a suitable memory system connected onto it.

There was also the question of input-output equipment, which at that time, was in a very primitive stage. In the early version, they used teletype tape and other very crude methods which subsequently ... But it was clear at that time, that we had solved most of the basic logical problems of the arithmetic system, and we had begun to be worried about the selectron, because it had already gone through several phases. I don't remember exactly what time we more or less believed that we would not get selectrons, but it was approximately that time.

MERTZ:

You had already made your trip?

BIGELOW:

I subsequently went abroad. I took a two week vacation, and went abroad and visited those various centers, and learned a lot at each one, and learned also how different the approaches would be at each one. Then at Manchester, I stayed with Williams and Kilbourn for about four or five days, studying what they had done and watching it operate. Williams was a most amusing man. As I stood there and watched his machine, part of it started to burn up because it was built in such a jerry-built fashion, but it didn't; bother him at all. He just took some clip leads off and said: "This is no good."

He took a soldering iron and took one piece out, and put some others there beside it, and put the clip leads on back on, and got it back into operation again. He was very inventive and a very helpful person, and I remember telephoning back to this laboratory to Jim Pomerene on what I had seen.

Then I left about three days later, and came back on the Cunard ship, the Parthia. By the time I got here, Jim Pomerene had a row of 16 digits stored on a tube, and it could be written in and read our successfully on the basis of that information. I must say that the team we had there was very good and very effective.

MERTZ:

Did Metropolis come over here?

BIGELOW:

Metropolis came ... Metropolis was head of the group ...He came for a visit on two different occasions. At one time, he was here for six months or a year, full-time. I think it was perhaps 1948 when Metropolis came. He didn't do any of the actual machine engineering. He had a man who worked for him who did. I think the man's name was Richards, but I could track down in some correspondence who it was.

Most of these people came and most of them were given the Institute hospitality as visiting scientists, and spent time with Von Neumann, Goldstine, and myself to the extent that I was available. I was very much unavailable during that period of history, because I was very much concentrated on this, and so to speak, didn't even look in any other direction.

The thing which gave Von Neumann confidence was, when we finally had designed our arithmetic unit and set it up for test, we put the switches on and it worked correctly the first time without any further adjustment. It was never necessary to go over and [?]things. We worked things out so carefully that we knew it would work.

MERTZ:

There are two questions that come to mind, in connection with this particular era of the computer and its development. One is that there has been, as I'm sure you are aware, a dispute or at least a disagreement, on who thought of the idea of the stored program, between those who are followers of Von Neumann, and those who are more or less followers of the Moore School of Engineering. This is compounded somewhat by the fact of proximity of people, and the fact that Von Neumann was indeed a consultant there.

I was wondering if some of your own personal experiments could throw any light on the argument.

BIGELOW:

In the first place, I would say that Von Neumann talked very freely with everybody. He never held anything back. Secondly, he was involved with the Moore School group for a least a year before I was around, so it's difficult for me to judge. Thirdly, I think the important thing is to make a distinction. The idea of stored program was not really new. Babbage had it. If you read Babbage carefully, one of the aspects of the later machine, which he never really did produce, was not that it would have a stored program, stored the same as its data, but that it would generate outputs on essentially punched cards, which were known to him. In fact, he knew the technique from Jacquard loom knowledge. The actual plans of his magnificent big engine, included the possibility, and he explicitly says, that he wants to feed the results back into the [?] structure [?]. So he really had that.

The person who really, however, pushed the whole field ahead, was Von Neumann, because he understood logically what it meant in a deeper way than anybody else. I think that's the correct statement of the situation, and a lot of people picked this up. A lot of people say: "Oh, I said, stored programmer", but Von Neumann really understood it in a very deeper way. The reason he understood it, is because among other things, he understood a good deal of the mathematical logic which was implied by the idea, due to the work of A.M. Turing, the famous British logician, who in 1936- 1937, had come out with a thesis at Princeton University, on what it means to have a decidable computation. Turing wrote his thesis, using as the method of proof of certain theorems, the notion of a machine, the universal computation machine, which would obey any set of orders and do whatever you want. Now Turing's machine does not sound much like a modern computer today, but nevertheless it was. It was the germinal idea. If you build an apparatus which will obey certain explicit orders in a certain explicit fashion, can you say anything about the kinds of computational or intellectual processes which it can or can't do. Turing's thesis consists of theorems about which a machine can do and cannot do.

Now the essential part of Turing's machine, is that it writes things down, then reads them back, and then does new things. Von Neumann understood this very deeply. In fact, he worked in the area of mathematical logic with [?]. So when the question of looking at ENIAC, or some of the early machines which had simply register store and were very inflexible, he saw better than any other man that this was just the first step, and that great improvement would come. He had this very complete flexibility about inductive change of process due to results of previous stages of process. He had this inductive quality.

MERTZ:

The second part of this question does relate to this, in that there is a good bit of sense of esprit de corps on the part of a number of groups that were working, since they were aware of the other groups that were working, and, in some sense, ... competition with various groups.

Could you characterize or comment on the way it developed here? I have heard it described in several of the other places, the roles of some of the individuals, what it did to you, described just a moment ago when you said you yourself became solely preoccupied with the machine at this point, and working on it. I gather that the role of Von Neumann was obviously quite ...

BIGELOW:

What Von Neumann contributed as far as the engineering was concerned, was simply the enormous confidence everybody had that a machine so simple, and with no more doodads on it could knock dead, so to speak, an enormous amount of the computation that needed to be done in this world for the next few decades. He never came over and said to make a circuit of this, but he did know so much more of the deeper aspects of mathematics and the practical aspects of computation than any of the rest of us. What he did essentially, was to serve as this unshakable confidence that said: "Go ahead, nothing else matters, get it running at this speed and this capability, and the rest of it is just a lot of nonsense."

It was really on a basis of that sort of belief that we went ahead, with six people and a budget. The whole budget, if you subtract the building out, was $800,000. The building added about that, so our budget looked like maybe two million. If you compare that with Forrester's appropriation, which was 25 million, we really feel that we did a spectacular job, and we stuck to our knitting, and we got it done.

MERTZ:

It has been said that many areas of academic interest in computing, were to some extent, stimulated by the fact that Von Neumann was involved in it, and it lent an aura of respectability, so to speak. At least his involvement and interest, among the mathematicians who became terribly interested in computational problems. But here was, probably, of that vintage, one of the most established scientists. When I spoke of his preeminence, it was [?] to me as you suggested. This might very well have provided perhaps some stimulus toward ...

BIGELOW:

He stimulated people everywhere. Von Neumann was generous intellectually, because his resources were so enormous, that he never gave away anything that he couldn't do without. He was a fountainhead of information, and he didn't hold back because there was always much more in depth than he ever exposed at one time. I'm not exaggerating. This was indeed how he was. In the few decades that followed, I have had the experience of understanding what this was like, by working as a consultant for some groups who were at a technical level so far below what I knew myself, that what they felt was worth bickering about was in fact possible [?] that one could ignore it, because one knows one has much more in depth than that and that's not an important point. Let them bicker about it, or let them think that they did something great. He was precisely that way. He was in depth, more knowledgeable than any man, and I say this having worked with Wiener for four years, and Wiener was no slouch himself. Von Neumann was a giant. He was ahead of anybody.

MERTZ:

One of the things that is perhaps interesting here, in contrasting senses of group spirit, or whatever it was, that is perhaps necessary in order to bring a project such as this to fruition, that, for example, at MIT there was a rather untried group of younger engineers who were not established by any means, who were working, and here was a man who had already established himself many years before as an outstanding mathematician.

BIGELOW:

Von Neumann did nothing much for the machine itself, you see. That was the area, for example, in which Forrester worked. Forrester didn't do anything for computation. He has no single contribution to computational science as far as I am aware, whatsoever. Forrester was an ingenious technologist, and also a man who knew how to protect himself, patent-wise, with extremely great skill. I respect him for this. This is where I was a boob. In our group, when we first started the group, we were given a promise that the Institute would take out patents for us. This promise was later withdrawn.

And I can remember to this day, going in to talk to the group of engineers, and pointing out to them that the contract rules had been changed, and the Institute was about to withdraw this agreement, and that we had one of two choices to make. Either we could all walk out and leave Von Neumann high and dry, or we'd have to swallow this unfortunate rescinding of a promise to us. All of us felt that we should not walk out.

MERTZ:

Did they have legal counsel?

BIGELOW:

The engineering group? I should have. This was my mistake. I often said that we should have an in-house patent man, but somehow Von Neumann and Goldstine and others never thought it was important enough. I have a paper right on my desk, which I was looking at the other day, to remind myself of this all. But this was a mistake on my part.

MERTZ:

What about the legal counsel of the Institute itself?

BIGELOW:

He was some fuddy duddy guy in Madison Avenue or Wall Street. Only if you asked about whether the Institute's budget is ... he didn't have ...

No, we had a gold mine here. Maybe I told you this last time, but what actually happened is that unbeknownst to the contracting agencies, the Army, the AEC, and all of them, the thing was funded by three or four different funding agencies, some one of them put one of our reports into the Library of Congress, which constitutes a publication. When it's published, nothing is patentable, because it had been in there a year before anybody caught the fact that it had been put there. The other agencies were furious, because it also gave away their rights through the fact that it was published, and it ruined the whole thing as a ... Forrester is a millionaire, I believe. He got onto one of the right gadgets, and he spent a lot of time on that gadget. In real life, in this kind of area, the patents are made on gadgets. A can opener will bring you more results than general relativity will.

MERTZ:

One of the questions that comes to mind in this regard, is that once the decision had been made, and I take it, after you had made that trip abroad, and then perhaps made an evaluation of what each group presented you in the light of what you needed, the decision had been made to go ahead with the Williams tube, was there subsequently, in the history of the machine itself, any great thought given to the storage problem?

BIGELOW:

Yes. We thought about going in on the magnetic stuff. However, the first commitment was to make a machine that would work, and the machine began to work in the latter part of 1949, and in fact worked in 1950. Most of our efforts, which means we allowed ourselves to turn to that area, was already pretty well in motion, and people didn’t have ferrites in those days, but they had suitable materials and there were [?] core storage devices around.

We had talked about it, but we really did this thing with a skeleton force. We had no surplus. Everybody had a job to do on the machine, and if we didn't do our own jobs, we wouldn't get done on the deadline. And there was Von Neumann with his hot little hands waiting to compute. So we didn't do any of these side excursions that one would normally do with such an effort. We had a lot of ideas about how to do them. We even discussed going into materials research, because we were sure that now that [?] had come out, they would play a role. It would have been a natural, because there were people in the Princeton area who [?] solid state [?] research. But we did not do so. We felt that our first job was to get the machine finished [?].

Then we had started to design a second machine, when the Institute for Advanced Study decided that it could not be contaminated with any more of this sort of thing. Von Neumann, in the early fifties, became an AEC Commissioner. He was away from the Institute for quite a bit of time, so I think it's fair to say that the reaction set in against the project. I had felt that my own responsibility was to see that the people who worked with me got good placement.

But let me get on. It became clear to us that the Rajchman Selectron devices were to be late, and indeed, they were late. None of them were workable in any sense before a year from the time they were supposed to be, and even then, I think if you look back at the correct chronology of it, the first Selectron units which were made available were finally delivered to Rand Corporation, and my impression was that they were installed by Rand in about 1952. The Rand Corporation machine, the JOHNNIAC, first ran from cathode ray tube storage for a year or two, and then these Selectrons, which then were down to 64 by 64 units to cell form 4,096. Of the first bank of them, I think, about 60 percent to of the tubes delivered worked. They still had enough to make one memory bank, but it was very much smaller.

MERTZ:

Which raises a question which might be appropriate to ask at this point. In terms of component reliability, since it was the idea of this machine to take standard shelf items that were available to compose this reliable machine, did this impose any particular difficulties in your design or the actual construction of the machine for diagnostic testing?

BIGELOW:

No. We designed the machine to be a direct couple parallel machine specifically, because this solved the diagnostic testing problem. Ours was one of the easiest machines to diagnose trouble on, and to keep operating for at least four or five years after ours was produced.

For example, the first computation done on our machine was really the thermonuclear bomb, and I would have check, but I think that computation was done in 1951. It's conceivable that it was 1952, but I think it was 1951. A team of people were sent here from AEC, and the problem was programmed, and put on the machine, and then the machine ran for 60 days, 24 hours a day, and the program was written in such a fashion that it would be error disclosing. I think that it was something like three hours in a 60 day, 24 hour run. It was quite a remarkable thing for a machine built in a laboratory.

MERTZ:

[?]

BIGELOW:

Yes, the problem was partitioned and what one calls a [?] was made. And [?] were something like every four hours of computation. They were made anyhow, whether or not the machine was stopped, so that if anything went wrong with the computation, it would be picked up.

MERTZ:

You mentioned just very briefly in passing, that there were other problems unlike a number of machines in this era. The memory problem became almost an obsession [?] some either trying to improve on electrostatic storage tubes, some [?] You touched on input and output as being, at that point, rather primitive, and that this could require some consideration. Could you amplify?

BIGELOW:

Well, in the first machine, the thermonuclear problem was done with teletype tape input, and what we did, was to have two teletype systems set up with two different operators, who punched the material into the tape, and then the two did the same job separately and the two tapes are compared for errors, keying errors which are one of the commonest sources of errors known. When they did occur, they went back to the original material to find out which one was correct. So we did this by independent checking, and then that information was fed by a teletype perforated tape reader into a memory, and doses of a certain sort. It was possible for the machine to call for new doses. When it was finished with some information, it would punch that on teletype tape and run it back to the teletype system, and type it out.

We used that only for about six months. Then we got some IBM punch card readers in, and it turns out that you can get more information per unit time from punch cards, so that most of the computing we did later on was done with punch card input-output.

MERTZ:

I take it the material of the tape itself was not a particular problem? It had been earlier, when it was tried.

BIGELOW:

There were various small problems, but they were not very important.

MERTZ:

It was finally batched?

BIGELOW:

It was finally batched in something like 1952 for the punch card system, and we punched in and punched out. We also put a magnetic drum on at about that time. This drum we put on was one we made ourselves, which stored something like 2,049 different words, so that you could dump the whole Williams tube memory back onto the drum and get the new Williams tube memory back in the drum in a fraction of a second. This exchange capability improved its effective performance enormously.

Then we bought a 16,000 word drum what used to be the ERA company, which subsequently became Minneaspolis-Honeywell, and it was from them that we bought this drum. That was put on in something like 1953 or 1954. I can get exact dates.

MERTZ:

Now earlier than that, though, among other things, I believe Von Neumann was a consultant to IBM.

BIGELOW:

Yes, he was. When our machine was first running in 1949, IBM sent a team of people down here to look at it. In fact, General Groves, who I think at one time, was also head of UNIVAC, came down to see our machine. Wasn't he President of the Eckert-Mauchly Corporation about that time?

MERTZ:

This was in 1949? I believe so.

BIGELOW:

He came here. Some people from IBM came here. For example, a fellow I used to know when I was at Endicott Laboratories, a very shrewd and insightful guy, came here, and two or three other people.

MERTZ:

A graduate of Princeton?

BIGELOW:

Yes, a very respectable man. In fact, he was one of the few people, when I was there as a youngster at Endicott, who knew what I was talking about when I [?]. He understood. But you can't move a big company when they have a tremendous investment in ... McPherson came here, and meanwhile, although I didn't know it, Von Neumann had become a consultant to them and IBM.

[End of Side Two, Tape One]

MERTZ:

This a continuation of the interview with Professor Bigelow, on the 20th January 1971.

BIGELOW:

I was in the middle of a sentence somewhere.

MERTZ:

You were talking about a number of individuals who had come to visit the Institute.

BIGELOW:

The team from IBM, and also the summer before we completed it Aiken himself came to see us. I do know that Von Neumann was consultant to IBM on the 701 series computer and that they were very largely based upon what we had done here, though they never received our engineering drawings.

MERTZ:

Did you ever keep a record of visitors to the computer?

BIGELOW:

No. We were not at all patent conscious. Now that I'm older and wiser, I would never tolerate such a thing again, but the whole motivation was different, you see. We were here to build the machine that Johnny wanted. That's what kept up going. In a certain sense, the thing that Forrester did, which was better, was that he had control of his own project, and therefore, he could do what he thought was good for the field as he saw it. This put him in a tremendous position to go into tangential directions, to expand wherever he wanted to, and to view the problem in somewhat more of a perspective, and I think he did indeed do that.

MERTZ:

There was also perhaps a difference, in the sense that I don't believe it's an unfair characterization, to distinguish the group at MIT under Forrester as opposed to the group here under Von Neumann, as being much more academically oriented, and the other being much more engineering product oriented.

BIGELOW:

Which was which, do you think?

MERTZ:

MIT was much more engineering product oriented, but maybe that's not a fair assumption.

BIGELOW:

We were missionaries. Our mission was to produce a machine that would demonstrate what high speed computation would do. I think that was not what Forrester's mission was. Forrester's mission was to do that incidentally, perhaps that's what they were doing, but they also wanted to, in depth, look over different methods of accomplishing each of the steps on the line to attack this on a broad front, and to advance the whole technological area, which indeed he did do.

MERTZ:

What I meant by academic, was that you would find characteristic of this less emphasis on the idea of patents, and a greater emphasis perhaps on simply sharing the news with everyone.

BIGELOW:

That's right. We handed out drawings, as far as I know, with no patent agreements, even with people at the University of Illinois and Los Alamos. We sent them great bundles of drawings. We had an ozolid machine someplace to run our sketches through. If we had an idea for something, and it looked as if it might work, we just run a pencil sketch through and send it to every-body else. There were no restrictions on that at all.

However, it's a fact that we did have an agreement in the beginning, that the Institute for Advanced Study would bear across, and take the responsibility for patent protection. I think that was a very painful point, because even with the cavalier attitude we had, we, each of us who gave that kind of extraordinary devotion, should have been rewarded with something.

MERTZ:

Now when this was finally rescinded, was prior to the [?]?

BIGELOW:

Yes. This was midway in the project, about 1949. What happened is that the sponsoring agencies, by some agreement or some law of Congress or something, said they could not support the gathering of patent material, and the preparing of that patent material for the individuals in the totally sponsored project. So their position on it shifted. It isn't that Von Neumann wanted us not to have patents, but that nothing could interest him less than patents. He wanted a machine, you see, and here came along this other clause in the sponsoring agencies, and I think we also shifted from primarily Army and Naval research support to an AEC support, and possibly AEC's contract read differently. When they simply announced that the new contract didn't allow this, what were you going to do? Were you going to stop, or go ahead? That was the way it was presented to us, and so we felt loyalty to him justified us to go ahead.

MERTZ:

And this was not a monumental thing to him?

BIGELOW:

It was one stage in a very long career. No, it's interesting that Von Neumann saw the importance and the power of this, but in some sense, there were certain areas which he did not see as well as one might have expected. For example, Von Neumann was never keenly aware of the importance of the program system side of the problem. The idea of a compiler, for example, never appeared to Von Neumann as an important thing. The idea that you needed a language in which you'd express, a Fortran language, did not occur to him early in the game. I remember because he was such a clever person, he could program the machine language rapidly and see things. He was very very gifted. He could solve problems in any set of symbols, write down a new set, and he would be able to substitute mentally what these new ones meant, and go right ahead. So that the question of intermediary languages and conversion processes, which has now become an enormous area, seemed to him a trivial question, and who wans to fool with that?

MERTZ:

Did the group [?]_? Wilkes and [Wheeler and Gill], who authored perhaps the first so-called textbook on the art of programming. Did they visit?

BIGELOW:

They all visited here at one time.

MERTZ:

[?]

BIGELOW:

[?] Illinois was later. Wilkes was here in fact during the first year of the project, I think, as a visiting member of the Institute, or something like that. I'd have to check it, but I remember very well walking across the compound with Wilkes back and forth to work. He was a great guy.

MERTZ:

You touched on several of the things that you felt made this machine significant in terms of its design, and in the history of ...

BIGELOW:

It was utterly simple. The machine itself was about the size of a grand piano. And the [?] was bare bones in the sense that many other projects did.

MERTZ:

Its simplicity would be [?] Atomic Energy Commission problems. I take it these were shock wave,[?].

BIGELOW:

Diffusion problems, very largely diffusion problems. Three dimension with phase trends and all that.

MERTZ:

Aside from these problems, what other ...

BIGELOW:

Weather forecasting problems and ecological problems.

MERTZ:

Which primarily interested Von Neumann somewhat.

BIGELOW:

Shock waves was quite correct. [?]Tower was here, and he was a top man in the shock wave theory, in physics. The last place he was at was Berkeley, I think.

MERTZ:

[?] of Von Neumann's?

BIGELOW:

Yes. One of the co-editors of his works, right.

MERTZ:

Did C. V.L. Smith ...

BIGELOW:

He was here also, as a scholar, but he didn't take any active part in either the engineering or the logic program. He was sort of an overseer. He was sent as a representative from one of the sponsoring agencies. He was a nice guy, but not technically in the swing of things, at that time.

MERTZ:

You mentioned several. Was there also a Wong?

BIGELOW:

Yes, there was. There was Cy Wong, who ended up with Hughes. The last I heard of him, he was working for Hughes in California. He designed a machine for Philco, the TRANSAC, which I think was the first fully transistor machine.

MERTZ:

This was a rather small group. About how many were there totally at its peak?

BIGELOW:

There were twenty, including technical people and people in the shop. The first six months, we had probably five or six people, and only two or three technicians. Then we went up to about eight professional people, engineers and what not, and about eight people who worked in the shop, including some student help.

MERTZ:

From Princeton?

BIGELOW:

From the area. There were high school girls. A lot of our machines were run by high school girls and what not, in the summers.

MERTZ:

[?]

BIGELOW:

Just grapevine. We were so small we didn't have any [?] or put ads in the paper. We just heard of somebody and they wanted to join us. Leon Harmon, who is now at Bell Laboratory, was a member of our group who came as a technician in high school. He got his bachelor's degree and he's now a research man at Bell Laboratory. There were two other cases like that, of people who came in with not too good backgrounds and during the transition period here, raised themselves up to professional levels.

MERTZ:

Would you say from approximately 1952 on, and also in between occasionally, that Von Neumann's direct activity with the computer, sharply decreased?

BIGELOW:

Goldstine ran the group essentially when Von Neumann's interest decreased. Goldstine is very good and very efficient. He and I don't do things the same way, but I must say I have a great admiration for him. He is a competent, efficient, intelligent, first class man. In Von Neumann's absence, he operated the thing. Of course, the thing he operated [?].

There was, meanwhile, various groups of people who came, and worked on certain problems. The meteorologists, for example, had their own programmers, and they did their own numerical methods. Charney, Phillips, and Gilchrist started in the meterologic group, but then it shifted gradually over to other areas and became more interested in the machine itself.

MERTZ:

This raises another area of interest. Would you say that the decision not to go on with another machine and to invest itself of the existing machine made by the management direction of the Institute was, in any way, conceivably related to the increasing demands that had been placed by the government and others on Von Neumann's time away from the Institute, as a sort of friend of the court here ...?

BIGELOW:

Oh, yes. That's exactly correct. I don't know where you got that picture, but it is exact. All of these forces were beginning to operate in a certain way. However, I think that even had Von Neumann lived, even though he were still alive, you had to fight every step of the way if you wanted to continue in that area. The only reason it ever got going, was because he was such a competent and respected figure, that they essentially couldn't say no.

The Director at that time, Frank Aydelotte, was a humanist, who knew nothing about mathematics or any of that branch of science, but he knew a brain when he saw one, and he respected what Von Neumann wanted, and he sort of said: "Well, let's give the man what he wants."

MERTZ:

Would you say that Von Neumann fared as well under a humanist as he would under ...

BIGELOW:

He fared better.

MERTZ:

He fared better than he would under, say a theoretical physicist?

BIGELOW:

He fared better. He fared better. The humanist was willing to take him on faith, and thus, when you really show a talent, if people believe in you, you can do the job. Now it isn't that Oppenheimer did not respect what Von Neumann did, but there was a very interesting kind of a dance at a distance in their relationship, like two game cocks who, if there were any hens to fight over, they would fight, but since there were no hens for them to fight over, they would walk past each other with a yard or two distance, with their feathers out and their hair ready to go if they have to. This was a steady state in their relationship.

Oppie came around 1950, if I remember correctly, I'd have to go back and look at that, too. Maybe it was 1951. Anyhow, he came well before Johnny went into the AEC, and before that machine was completed, Oppie came over and had his picture taken in front of it a few times, but that was his major contribution.

MERTZ:

Wasn't it under his tutelage that the Institute did make the decision?

BIGELOW:

That's not a fair statement. No, Oppenheimer was never against the machine. What really happened, was that Oppenheimer exacerbated the bad relationships within the Institute, to a fantastic degree. Oppenheimer was a brilliant man and a competent man. His technical skills level was certainly in the same ball park as Von Neumann, but he didn't have the diplomatic sensitivity that Von Neumann had.

Oppehneimer genuinely considered himself to be gifted in every field at once. This was really a fantastic aspect of his personality. He would do things like go to the seminars given by the historians and the humanists and what not, and he would act as if they should look to him as an expert in their own field. He would make pronouncements and so forth, so that in a very surprisingly short time, those who didn't know or understand physics, felt that he was a royal pain in the neck, and a pretender and everything else. Nobody can feel that way more than somebody, for instance, who is in an art's or historian's point of view where the main kudos, the evaluation, is not done on the basis of the immediate performance, but on the basis of how much past material you have written and contributed, and so forth. Oppeheimer, in fact, split the Institute wide open. He made it a continuous battleground.

MERTZ:

One of the things that I was interested in attempting to learn, was whether or not it would at all be fair, or it would be a great distortion of the situation, to say that those who favored or viewed with a certain amount of benign [?], the continuation of the computer project were fundamentally scientists, and those who were opposed to it were fundamentally humanists, or were there actually divisions?

BIGELOW:

It doesn't split that way. Point one, Oppenheimer split the Institute and caused continual internal warfare. In fact, the warfare was so severe, that he was asked not to attend departmental faculty meetings, because he disrupted them so. Secondly, the Institute originally consisted of two major schools: the School of Humanities, and the School of Mathematical Sciences. However, within the Mathematical Sciences, there was a sharp split. The people who were pure mathematicians felt unsympathetic towards anything like computation, whereas most of the physicists respected it and felt it was really important and should be pursued. Oppie himself felt this way. Oppie liked to be in the limelight and liked to be around when the flash bulbs were flashing, but nevertheless, he understood what computation was about and he was 100 percent for it.

But at that time, having Oppenheimer for something, was exactly the way to get it stopped by all the rest of the faculty. Unfortunately, anything as major as having the continuation of a project, requires essentially unanimous consent of the faculty. There were some whole schools against it, and some other whole schools [?]essentially, this proves [?]so it's unwise to continue it.

The School of Mathematics became taken over by a very puristic group of mathematicians who were at least about as difficult to handle as the humanists were. Therefore, nobody was in favor of this, excepting Von Neumann, who was then an AEC Commissioner, and some of the physicists who were after all [?].

MERTZ:

This was to remain a [?] experiment on the part of the Institute itself.

BIGELOW:

Yes. [?] I'd like to express some personal feelings. I was not altogether horrified at the notion that we were not to build another machine, because in some sense, I felt that having built a machine and made it work and the field itself going ahead, that the interest should no longer be on whether a computing machine of speed x or speed y or speed z could be made available, but that the important direction to go into, would be the relationship between logic, computability, perhaps machine languages. In other words, the things that you can find out scientifically, now that this tool is available. The thing which was to me a great personal disappointment, was that in this area which had promised so much, and in fact, I, on very many occasions, had said that we would have the greatest school of applied science in the world, we could show the theoreticians that we would find out the answer to their number theoretic problems and their problems in physics and their problems in solid state, and their problems in mathematical economics. We would do planning, we would do things that would be known for centuries, you see. That whole program collapsed.

MERTZ:

Was it 1954, when he proposed that as a sort of projected activity, quite apart from whether they did more of the hardware?

BIGELOW:

Oh, yes. The reason he made Goldstine and me permanent members, was that he wanted to be sure that two or three people whose talent he respected would be around no matter what happened, for this effort. He talked to me many times of putting this together. He wanted biology, he wanted mathematical biology, he wanted mathematical astronomy, and he wanted earth sciences. He visualized the ability to do these computations as it has, as affecting these branches of science to a great degree, and as enormously expanding his personal abilities to get into and to contribute to all these different fields, which indeed, I think there is no doubt he could have done.

It is a sad fact that he somehow became, in the last five years of his life, so involved in political questions, I think they essentially were, that it was very difficult for him to be able to refuse, because the gravest of responsibilities were offered to him. This was a greater tragedy than that he lost the machines. Von Neumann's aversion to doing things which [?]

MERTZ:

[?]

BIGELOW:

His personality was also a very complex one, and as many people who are very gifted, he had compulsions here and there, and although he was quite wealthy, he was always attracted by making more money. I remember that one of the motivations for his going to the AEC job, was that it paid 50,000 dollars a year, and the Institute was paying 20,000 dollars.

MERTZ:

Was this in any sense, related to the preening of the feathers of the then Director, for whom money was not a ...

BIGELOW:

A hobby? It's complicated. The Oppeheimer trial also occurred around this same period of time, when he was uncleared by this hearing.

MERTZ:

The very same agency that...

BIGELOW:

Yes, and this was a very complicated, terribly complicated thing. Von Neumann himself was terribly upset by the Oppenheimer hearings. He was not in step with the rest of the faculty about that, essentially I think, because Von Neumann was a terribly realistic person, and he though to support Oppenheimer because he supported, so to speak, liberal politics, was a whimsical position to take, a somewhat...

MERTZ:

Not necessarily an unreal position?

BIGELOW:

Quixotic position to take, to some extent, I would say. There was a big issue with the faculty, and this issue didn't really help to cement matters. Practically all of the faculty was engaged in signing a memorandum, saying that we support Oppenheimer and feel that his loyalty is beyond question. I was one who signed it. Von Neumann objected to signing it, and he objected to its existence. He said it would split the faculty, and that was right.

Then, at the same time, he was about to take Oppenheimer's place at the AEC. Not actually his place, because Oppenheimer was one of the key people, but he was to become an AEC Commissioner.

It was very confusing. I don't know. Von Neumann may have had some premonition that he was not going to live very long at that time, because a great many of his actions were a little bit erratic and inconsistent with his previous history. He died in 1957, I think, and he was ill for about two years, from 1955 to 1957.

MERTZ:

Well, he gave this address in Amsterdam, and also others that he gave were of the nature of last [?] very basic kind of [?].

BIGELOW:

Yes, but that's not quite the point. The point was, why did he personally immerse himself in the political position?

MERTZ: [?] the picture of applied mathematics in the Institute, it isn't necessarily so if one thinks of aspects of physics [?], that applied mathematics is not necessarily dead at the Institute for the future.

BIGELOW:I think it largely is. I don't know if you want to hear a position statement on that, but I think that the Institute is a sterile place for applied mathematics, because it is unwilling to come into closer contact with the problems which are really important, which are contemporary problems.

MERTZ:

One logical thing that could have happened, conceivably, was once it has divested itself of the hardware as a machine [?], sort of acquired the use of ...

BIGELOW:

Yes, that is correct. Of course, now there is a tiny bit of that. There is a remote terminal that has been installed in the last year or so, but it's not a very high powered effort.

The point is, that the Institute does not have the breadth to even do that kind of development kind of [?]. The kind of thing that tends to happen in the Institute, is what happens in very many places. The Department of Mathematics is now headed by a very brilliant, highly specialized mathematician, [?], in any other problem in mathematics except the one in which he excels. The Department has built up strength in such areas, and there is nobody there who has the influence and the power to withstand the tide, much less the desire to do so..

In physics, there has been a great reversal of the strength we had here. When Oppenheimer was here, there were like 20 to 25 people in theoretical physics, and now it's down to five or four people--five. It's down to five people, and it's changed over. It calls itself the Department of Natural Sciences. It's a very good group, and I respect every person in it enormously, but again, none of them is quite of the stature, Dison possibly, but none of the others is quite of the stature of Von Neumann. They can and do tackle problems over widely different areas, and in fact, solve them so as to become famous in each area, so that the experts in it think that they are the most famous men in that area.

MERTZ:

But it might not be fair necessarily to compare them to [?].

BIGELOW:

This is not fair, but you see, for example, what Courant did at New York University, is in some sense, a very much greater thing than could be hoped for here.

MERTZ:

[?] .

BIGELOW:

You mean in the humanities? I'm not sure. We have some very good people. The point is, that a sort of golden opportunity existed for excelling, and it was not taken up, and I think possibly now the time is past.

In any event, my point is that the building or not building another generation of machines was not a very important loss. The important loss was that whole new sorts of areas which are now important areas of science, are inaccessible, because we don't understand them and we don't have any confidence. In any event, they are important.

MERTZ:

Did you and Von Neumann, and to some extent, Goldstine, [?] Did you and Goldstine attempt to ... I'm not sure when he left ...

BIGELOW:

He left in 1957 or 1958.

MERTZ:

But there was a period of a couple of years when [?]_.

BIGELOW:

Oh, yes. [?]. I felt then that completing the machine ourselves would be better. And in the number of projects which have continued. For instance, Gerry Estrin at UCLA, is Professor of Computer Science. He is still trying to develop new hardware and new computing techniques, and with some modest success, but nothing very startling. He's spent the better part of his life doing this, and my feeling is that he's had not too much success, not because he isn't good, but because the field has been [?]_. There's not too much you can do [?].

MERTZ:

You were speaking of [?] of the area.

BIGELOW:

Also the logical organization.

MERTZ:

There are a lot of products around. As far as I am aware, Ralph Slutz [?] never really knew _______.

BIGELOW:

It would be question of point of view but [?].

MERTZ:

What would you say would be the next direction, taking computer technology in its broadest sense? What areas do you feel, both in the short term and in the long term, will have the greatest impact scientifically and in society, of the computer?

BIGELOW:

On society or scientifically?

MERTZ:

Either one or both.

BIGELOW:

Let me make one or two free association comments. I think there is an area where computer development needs a breakthrough, and that is for very large problems in the mechanics of continua, like the meteorological problem, weather forecasting, or understanding [?] circulation. The problems [?]_. The present concepts of the machines are inadequate. There is a tendency to think that the very large machines like the [?], the 6000 machine, and the biggest of the IBM line and so forth, ought to be the avenue by which one approaches these very complicated, higher dimensional problems. I think not. I think that what is needed here again, is a breakthrough in computational logic. I think that if adequate funds were available, and a small team of good people put together to work on that problem, for something like three or four or five years, that a new kind of machine would evolve, which would be radically different and radically better. A new kind of logical approach to how to do it, and than an actual machine [?] itself. That is a technological plus logical problem.

MERTZ:

Correct me if I'm mistaken. Are you referring here, to use the expression, computational logic, to problem formulation, in a sense?

BIGELOW:

Yes. I'm saying that one has to take the partial differential equations, etc., with all the mass [?] into them and start, I think, right from the beginning, as to how one expresses these in numerical computational form, and to enter there with the effort to see how, what it means in a problem in logic, rather than take the problem after that stage, as being a certain method of solving that particular [?] equations [?] .

I think to work afterwards and look at these things has resulted in the various kinds of large parallel array machines which are being evolved now, one of which is, for instance, the ILLIAC IV or something like that. My feeling is that those are transitional machines and that they will not be very successful. The programming questions and the efficiency of the machines themselves, as they get larger, of course, are growing more rapidly then the efficiency of the machine's performance on the kind of problem in question.

So, as I say, there is a missing link in my opinion. These transitional efforts will look very much like the SSEC, which IBM built, by the massive use of [?] technology, and that computer never worked very well, though I respect the people. I respect Hanny Zlotnick and other people working on it very much, but I think what is needed is a lot more work on the mental level.

I don't see anybody around who wants to put up money to do that, and you would need quite a lot of hands-off support to develop it. Anybody who wants to see it, to realize a machine in two or three years, is going to be disappointed. That's my point.

The second question is, that I think there has been an enormous explosion in various kinds of accessory activity to computing, like the study of machine languages and natural languages and so forth, by themselves. I think that these areas are important and present interesting and beautiful problems which however, in some cases, many cases, appear to have the same kind of inbred characteristics that I described as being true of much of modern mathematics. Namely, that somehow or other, they have gone off to invent problems in the milieu of the field itself, with the language expression, with the communality of the other problems, just aspects of it, in such a fashion that they don't really apply to what I think is the fundamental and solid areas of science. They are to some extent like people who are talking about modern differential topology, and are solving important questions in it, which however were questions invented by topologists to interest and to point out things to other topologists, rather than to do problems which would be recognizable as related to something in physics or astronomy, something [?].

I'm saying that they have broken off their connection with any other field of application than the area itself, and they are generating within the area. My feeling is that these are fascinating, but they tend eventually to become sterile. I think that though some great things can be done there, the important thing is to try and tie it to something which is in the real world, applied, in that sense. I'm convinced that that's why the computing machine is important.

MERTZ:

I have two questions, I think. One is, would you care to describe, on the basis of your own personal involvement, the two men you worked with over a number of years, who were quite well known in their respective fields, [?]_, Norbert Wiener and Von Neumann, in the sense that Plutarch sometimes would pick parallel individuals who were similar in their activities, and tell how and in what ways they differed as individuals and personalities.

BIGELOW:

Well, they were absolutely different, of course. Von Neumann was a fantastic technician in mathematics, as well as being imaginative and insightful, and he was one of the few people who would really think out loud and prove a theorem on the blackboard, inventing notation and inventing procedures just as he went along. He really lived in this kind of a skill field, and he was so competent in it, that his invention occurred really in that milieu.

Wiener, on the other hand, was as inventive and as full of ideas, and in some sense as great a genius as Von Neumann, but he was not a technician. Invariably, when I was working with Wiener, the things which he said were true, mathematically true, but the proofs he have were never correct. The insight was correct, so that sooner or later, somebody else in effect cleaned up the proof, or there were five different stages of improving it, and eventually, it turned out that that statement was true, with certain kinds of side conditions.

But here was a greatly inventive person. Wiener was a very generous and a very spontaneous and a very disorganized person.

MERTZ:

Was he personally as secure a person as Von Neumann?

BIGELOW:

No. Certainly not.

MERTZ:

Von Neumann comes across, I didn't know him, but in talking with people who did know him, as a man who was rather secure in his opinions.

BIGELOW:

Oh yes, he was.

MERTZ:

He knew the measure of his own mind. Would you say there was a distinction between Wiener and him?

BIGELOW:

Yes, that is correct. I think the distinction was there, because in fact, it was true that Wiener's brilliance ... That he himself never had the discipline to do everything with his brilliance that he should have been able to do, and therefore he was always unsure as to whether he approved of himself. But nevertheless, with all that, Wiener did work as great as any of the ten best mathematicians that America has produced. He was not an expert in computing machines. That's just not true.

MERTZ:

So far as I know, he never had anything to do with a computer, although he discussed computer technology and the ideas that ...

BIGELOW:

Sure. He understood it well. He understood the jargon and could talk the language, but ...

MERTZ:

[?]

BIGELOW:

But on the other hand, Von Neumann could do that walking down the hall.

MERTZ:

Von Neumann was indeed [?] computer. Would you say that it is true that there is a difference in an involvement with the physical apparatus itself ...

BIGELOW:

It's very hard to make this sufficiently concrete to ... If they were left on a desert island, I'm not sure that Von Neumann would be any more skillful with the physical problems of survival and re-establishing technology, than Wiener, though he might. Certainly he would be more practical, and certainly he would understand the points on a more practical level.

MERTZ:

Well, now you're comparing them on ability, but would he be more interested in the problem?

BIGELOW:

Oh, yes, I'm sure. Wiener's interests were wide, but he loved to play sort of power intellectual games with the problem, whereas Von Neumann would see the point just like that and get to work, and then when that's disposed of, let's move on.

MERTZ:

Were their breadths of interests similar, or would you say Wiener was less broad?

BIGELOW:

They both were. Wiener was as broad as you can get. He thought that he knew something about everything, and he probably did know something about everything. He was a more gregarious talker than Von Neumann. Von Neumann had enormous amounts of reserve, which he only gave forth when he felt like exerting himself. Then, he was one of the most lucid and precise people I've ever heard.

MERTZ:

He has been described by one person as having a sort of earthy sense of humor.

BIGELOW:

Yes, he did. He had all facets of humor. He could pass the subtle and the earthy.

MERTZ:

The last question is, your own assessment of your contribution to the development of the Institute machine.

BIGELOW:

I can't assess it. We got it done. We got it done with a small number of people, and it worked very well, and it was a very small budget. If I had my life to live over again, I would live much more expansively than I did, but I was hewing to a tight line.

We had enormous numbers of ideas which we never did anything with, and, as I brought out, that's one of the saddest things. I think we were very inventive, but I think that to document it now would be an enormous exercise. We were inventive all over the place in our ideas, the way Forrester's group were exploratory all over the place in technological componentry. What we should have done was to magnetic tape record, or something, all of the intellectual exercise we did in the course of evolving the machine, and talking about its successors.

[End of Side One, Tape Two]

[Tape II - Side II ?]

BIGELOW:

It's very hard for me to say what parts were contributed by each person. There were a few basic ideas that I contributed, which dominated the particular design of the machine. One was, in some sense, that the main part of the machine should run without the use of an extraneous clock at all, that each particular order that you initiate should run through a sequence of logical events, and when the function is completed, then that completion should correspond to a clock step which tells the next operation that it should be initiated or start off. The idea of using the whole machine, instead of having a fixed time clock, of using the length to complete an operation, whatever that operation may be as a typical time step, was a basic idea of my own. The fact that one can do this well, with direct current circuitry in certain kinds of logical things, was a contribution of my own. I mention this simply because it is one of the first I remember.

There were a great many others. The whole team worked very closely together. We were so few when we talked to each other, that it's very hard to state from the standpoint of original invention, which parts came from which people.

Pomerene had a great deal to do with the Williams memory. Willis Ware, now at Rand, worked a lot on pulse circuits, and both he and Pomerene were among the best people we has in actually making circuits _____ experimentally before the version was actually built, whether or not they were stable and had the right characteristics.

Ralph Slutz was an extremely competent person, who left the project about midway, to go to the Bureau of Standards, to finish the SEAC. He was competent in many areas, and did great deal to serve as an interaction between various members of the team and myself, in how the machine would eventually be built.

Estrin came rather later in the game, and was concerned primarily, at that time, with input-output facilities and putting the magnetic drum on.

Synder was with us only about a year, and left early in the game to go elsewhere, as did Shore and Davis, who were with us about a year at most. Each one contributed to some extent, in getting it going and getting pulse techniques going and measuring equipment ... You could understand within our group, what it meant to measure events in a fraction of a microsecond, and see that they were completed correctly.

MERTZ:

Do you happen to know where any of them are?

BIGELOW:

Somebody told me that Davis is in an Army Signal Laboratory in Johnstown, Pennsylvania, which is quite a well-known radar and pulse center. I don't know what happened to Shore. Shore was a gifted person who had some physical handicaps. He was an albino and his eyesight was very bad because there was no pigmentation in his pupils, but he was a very clever and competent person. I don't know what happened to him. I think he may have been _______.

I was trying to think of various other people. There was a man by the name of Hildebrandt, who came to the project fairly early, and he is now head of the computer center of the ESSA research group in Boulder, Colorado. That is the weather computing group. I see him twice a year now, because I'm an advisor to that sort of thing.

You know that Goldstine and Pomerene are at IBM, but Ware is not. He's at Remington Rand. Another fellow who was in charge of running the shop and construction, and who got his bachelor's degree while here, is Dick Melville, who later on went to IBM. Since then, he has become quite ill, and I don't know if he's still alive or not. I may have left out some important people.

MERTZ:

Bond ? _____________.

BIGELOW:

Bond was here for about two years, or perhaps two and a half. At the time he came, he was not so experienced as some of the others and we were well in the middle of our final stage. He was very effective and very skillful, and picked up very rapidly, and did make some contributions. What is really in my mind, is not the impression of what he did then, as the fact that when he left us he, on his own initiative, designed an entirely new and radically different machine at Philco, which I think, however, was a parallel machine, and a direct couple machine as ours was, using transistors. TRANSAC was his machine. His great ability came over more on that machine than during the period of time he was with our group, primarily because he joined the group after the conceptual level had been settled, and we were just sort of plowing ahead.

Rubinoff was with the group, and I think he left after a year and a half to become a professor at the University of Pennsylvania and later became _______. He also worked on high speed circuitry and ________.

MERTZ:

Did Von Neumann's wife make any contributions?

BIGELOW:

No, she did not. She occasionally did some programming at which she was very competent. She did that work after the machine was essentially finished and the ______. Also Mrs. Selberg, the wife of Professor Selberg, did some programming__________. Gilchrist, who came to work on meteorology and became interested in other areas, made some contributions. In fact, he wrote a paper with Pomerene and somebody else on a variety of .... adder circuit which tells when it's finished. It tells, no mater what the addends are, when the operation is complete, by a variety of complementary circuit.

That idea is typical of the basic ideas that I had produced in the machine earlier. That particularization is an extension of it. Gilchrist is a great _________. I don't know what else to say.

MERTZ:

Well, I would appreciate it if you would _________. Thank you very much.

[End of Tape II - Side II]