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Interviewee: Morris Rubinoff
Interviewer: Richard R. Mertz
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MERTZ:

Professor Rubinoff, would you care to describe your early training and background and influences.

RUBINOFF:

The early training is at the University of Toronto in mathematics and physics as an undergraduate, and then in physics as a graduate. The physics was tested in research projects during World War II, which was related to the proximity fuse. In fact, a strong interest in computational techniques, numerical methods was developed then, and also in switching devices because right after the War the proximity fuse techniques were used to make measurements of the angular motions of projectiles in flight. To do this it was necessary to calculate trajectories. Calculating trajectories is an interesting problem since it relates to what made the ENIAC so interesting at Aberdeen. They were using it for calculating trajectories, unknown to me at the time. We were calculating trajectories by hand at the University of Toronto using a method which is often referred to as the Richardson method. So the whole technique of numerical analysis and numerical computation got to be very intriguing to me.

MERTZ:

Was this done on a Friden [or] Marchant type calculator?

RUBINOFF:

MERTZ:

This was a War project at the University of Toronto?

RUBINOFF:

This was a post-War project. It was an outgrowth of a war project on proximity fuse. It was supported by the Canadian Army who were very interested in finding out what made liquid filled shell tumble rather than fly properly when they went through space.

Of course, we as physicists were rather interested in skin friction. We were able to determine the rate at which the spin decreased while in space and could calculate skin friction which was a very important problem in physics at the time.

We also managed to make measurement of the so-called rosette pattern motion of shells which is from a paper by Fowler on spinning tops in resistant media, and we demonstrated that his equations were accurate and that his analysis was correct. There were a lot of interesting things that came out with regard to the behavior of spinning projectiles in space, particularly if they go up to apogee and the speed of sound at the same time, very interesting things happen, but that's got nothing to do with computers.

MERTZ:

Did you have any connection or contact with the Bureau of Standards which was I believe active in that...

RUBINOFF:

No we had no contact with the Bureau of Standards. We did have contact with the Applied Physics Laboratory of Johns Hopkins University who were of course the leaders in the proximity fuse work, and in fact the team as a whole was a pretty good team. Some of the people invented something that later got called Canadian Army and then as time went on it got called capacitor or condenser army--as time goes on nationalities disappear. But the group did do some things up there. Not I but some of the others. But that got me interested in numerical calculations and electronic switching, and because I was doing this work, apparently it reached the attention of a professor Pekeris, Chaim L., who is now at the Weitzman Institute in Israel, who had heard about computers being developed and in fact was a rather close friend of Johnny Von Neumann's and Chaim L. Pekeris. He is still alive. He is at the Weitzman Institute. He is probably by now--well, he certainly heads the computer operation at the Weitzman Institute. He is famous for his work on seismology and on propagation, so he is well known in the literature.

MERTZ:

Was he also active in building the [?] SAC?

RUBINOFF:

He was not active in it. He was however a very active spectator and was on first name terms with Von Neumann and followed the theoretical development in intimate detail because he then went on to build a computer at the Weitzman Institute several years later which was going to be patterned after the Von Neumann machine but instead turned out to be something actually somewhat more sophisticated. It was a 72 bit machine, I believe, 72 bit words, and 1 microsecond memory which in those days was really something it was called the Golem I. I think I've gotten that right. It can certainly be checked in the literature.

MERTZ:

I've come across the term WEIZAC

RUBINOFF:

It's sometimes also called the WEIZAC. They have now built several machines. Which one is the WEIZAC I'm not sure. But there was a GOLEM I and the GOLEM II and I think GOLEM II is also called the WEIZAC. At any rate, Chaim Pekeris is a good man to talk to if you want some intimate information on John Von Neumann because they did one another first name friends and know one another extremely well. He encouraged me.....oh I had written letters to a number of universities and was rather successful and then I got a number of offers, one of which was Harvard which I had decided to accept to work under van Vleck.

MERTZ:

You had completed your PhD?

RUBINOFF:

1946.

MERTZ:

What had you done your PhD work in?

RUBINOFF:

The Ph.D. work was in fact this measurement of spinning projectiles in space by this technique. That's what the doctoral dissertation was. As a matter of fact it led to three doctoral dissertations because it was done only by us. The faculty didn't even understand what we were doing, let alone know [?]. We had only one faculty member who used to follow what we were doing very closely and his primary contribution was a discussion of the ketones and other impurities in alcohol, and he had good reasons to know about alcohols, but he didn't help us in our project particularly.

MERTZ:

Then it was after your completion of the PhD that you were writing off to a number of universities.

RUBINOFF:

Chronologically I got my bachelors in 1941, I got my masters in 1942, the war decided they'd like to see me in England in '43-'44. Then I came back. The War was over in about '45. From '44 to '46 the effort was primarily on this exterior ballistic, and that's what led to the PhD dissertation and to the PhD degree. It was in the fall of 1946 that I arrived at Harvard and under the encouragement of Dr. Pekeris succeeded in getting a half time appointment to the physics department under van Vleck and a half time appointment in the computation laboratory under Howard Aiken, which is interesting perhaps for your record in that van Vleck thought computers were sort of insignificant and unimportant in the world of science, and therefore he and Howard Aiken did not really see eye to eye, and working under both of them was quite an experience I can assure you. They were friendly.

MERTZ:

I suppose it was fortunate it was a post-doctoral experience.

RUBINOFF:

Well, maybe, maybe not. I found I could keep them both happy by working 40 hours in both places a week. It wasn't so bad since they were each paying half time.

MERTZ:

What was the nature of your work for Howard Aiken?

RUBINOFF:

At Howard Aiken's, he requested that I assist him in the building of Mark III, which was a magnetic drum vacuum tube computer. Marks I and II were relay and electromechanical, electromechanical and then essentially relay. Mark III was the first vacuum tube computer that we undertook to build. I worked under Benjamin L. Moore, who headed up the project for designing and building the Mark III computer and then reported directly to Howard Aiken.

MERTZ:

Did you know a Dr. Way Dong Woo?

RUBINOFF:

I knew Way Dong Woo extremely well. He came shortly after ward and we worked very closely together. In fact, just when I left An Wang came as well, and An Wang is very well known today. And he is quite a businessman today. The other one who was there at the time who has made quite a name for herself is Dr. Grace Murray Hopper, who was a programmer at the time, and who encouraged me, and I didn't need much encouragement, encouraged me to program the Mark I even though I was building the Mark III. And she is still fond of quoting the fact that I was the only man at Harvard who worked on the hardware and took the trouble to program the problems to find out what the machine was going to do when it was done. The others there at the time were Richard Bloch and Robert Campbell who has become rather famous. Herbert Mitchell was there on something like a 120 hour a week. He had three jobs simultaneously, from what I can recall.

And then there were some others who have made some name for themselves, Harrison W. Fuller (?) who was the first to use a cathode ray tube to inscribe digits on the face of the cathode ray tube for photographing purposes, and his so-called numeriscope is described in the Proceedings of the 1947 Conference at Harvard, I believe they called it the First Conference on...well it's the first computer conference, and it's Volume II of the Harvard Annals, and it's fully described in there.

There were some others like [?], Charles [?], and [?] Ed White, who then went on and formed a company called [?] Instruments. And Marshall Kincaid. There were quite a number of folks, some of whom still are in the computer field, some of whom have now gone into other areas.

Ben Moore himself went on to Los Alamos where he got involved in atomic energy. He gave up the computer field in about 1949 or 1950. I was there from 1946 to 1948 and participated very actively in the design of many of the aci[?] digit machines.

MERTZ:

Can you give us some of the things you were active in?

RUBINOFF:

Some of things I was in were the read amplifiers and the write amplifiers for the magnetic drum, and the algorithms that were eventually used to implement the logic of the arithmetic unit. And I participated with Ben Moore in the decision to use the so-called two-star 4 - 2 - 1 representation of decimal digits by means of binary digits. This is described in another volume of the Annals of the Harvard Computation Laboratory, the one on the Synthesis of Electronic Switching Circuits, which is an interesting volume on conventional algebra used in such a way that it really is Boolean algebra but it doesn't look like it when you first take a look at it. And there's a mention of a variety of numbered representations, binary representation of decimal digits, 2 star (?) 4 - 2 - 1 -[?].

Howard Aiken remembers the 8 - 4 - minus 2 - minus 1 system which I invented facetiously because he insisted it should have a nine and I said I could make it binary and make it add up to nine so it goes 1, 2, four, eight, etc. and 1 and 2 were minus. That also was mentioned in that book on synthesis of electronic switching circuits because it turns out that that's identical, truly identical with the excess 3 code that was used by Eckert and Mauchly in their BINAC and in their first UNIVAC. Only nobody knew that they were identical until somebody claimed that they had discovered what do you know, if you look at it with a logician's eyes rather than a normal human being's you discover in the last two columns that zero looks like one and one looks like zero, they become the same. But there now is good theoretical reason why they should be identical and you can see it after the fact. Hindsight is wonderful. Those are the things I played with essentially at Harvard University.

MERTZ:

And you were there for two years?

RUBINOFF:

I was there from 1946 to 1948. Howard Aiken was very interested in a machine which would be highly reliable. He was not too interested in speed. He felt that there were other considerations that were more important. Therefore he decided to go magnetic drum which he felt would be more reliable and probably would get finished sooner and running sooner. He went serial rather than parallel. His circuits were clocked as a result. At the same time at the Institute for Advanced Study I learned through Dr. Pekeris that they were going parallel rather than serial, that they were using very high speed electrostatic storage based on the work of F.C. Williams of England, the so-called Williams storage tube was the direction they were going, and I thought that I had learned as much as I probably could at Harvard at the time and I accepted an invention then to go over to the Institute for Advanced Study.

MERTZ:

During that two year period that you were at the Harvard Computation Laboratory do you recall some of the specific work on which various people spent their time in that period. [unclear].

RUBINOFF:

It’s a little hard to piece together. Obviously Howard Aiken was on top of everything as he always was. In fact, with his long legs he was up and down the stairs between Mark I and Mark III in zero time particularly if Mark I stopped chugging. If it stopped chugging just for long enough to do a multiplication or a division which was approximately one minute then nothing happened. It was clear that if it was longer than a multiplication or a division he went lickety split upstairs to see why the machine had stopped. He was on top of everything. He participated in the discussions on the circuits, of the logic, of the algorithms, of the functions that were being thought of to put into the machine and then did not go in.

MERTZ:

(Unclear)

RUBINOFF:

Way Dong Woo came a little later, not much later, and he started working with magnetic shift registers. I think at the middle of '47, sometime in 1947, and he started to work with shift registers. And that work was done by Way Dong Woo, that is correct. Ben Moore was essentially administrative supervisory type. Coolidge, Charley Coolidge did algorithms, and he worked out timing [?] algorithms for addition and multiplication and [?] as to whether we would use one method or another and he came in [?].

Harry Fuller I've already mentioned working on the numeriscope. He was also working on the logic of the computer. Ed White was directly responsible for the sense amplifier for reading the information off the drum, the magnetic drum. There was a fellow by the name of Chief Porter who was essentially a technician who received considerable stimulus from fire water in order to keep the operation going. And he knew how to harness cable and put together a system for [?] of production. He did a fantastically good job of production. I don't remember who did the magnetic drum design. I guess Howard Aiken [?] that because he was a mechanical engineer with many years experience. I know [?] but I don't remember off hand. I'd have to think. The name Wilkins comes to mind. But again one might look at the manual on the magnetic drum computer and I think one could find credit there to who did many of these things, and that's another one of these Harvard Annals of the Computation Laboratory.

MERTZ:

You first learned of the Princeton project through Prof. Pekeris?

RUBINOFF:

Not quite. There was a conference at Harvard University in 1947. I learned quite a bit about what was going on in the computer field through Dr. Pekeris because he felt I should take an interest. He was kind of hopeful I would go with him to Israel, as a matter of fact. And so he encouraged me to go with him to Harvard and he encouraged me to go to the Institute for Advanced Study. So in fact I first heard about these things through Dr. Pekeris. In fact, I heard of one other system, I heard of the Bell Labs system through him, and in fact I got a copy of Stibitz paper on how relays can be used to do binary addition and I remember sitting on the airplane between Toronto and Boston on my way to Harvard University the first time reading that paper and trying to understand just what on earth was Stibitz talking about. Like so many difficult subjects, when you finally realize what the man is saying it becomes so obvious you wonder how he could have been so obtuse and how you could have been so dense. When it came through it became quite clear what was going on. Of course there was no logic at that time. There was no Boolean algebra. You did it essentially by talking about when this contact closes [?] etc.

If you keep a chain of events all in your mind simultaneously you can go from the beginning to the end hopefully to get the right answer. Particularly if a man says that is the right answer you get to the right answer sometimes you're not quite so sure.

So yes, he helped me get an awful lot of that information. But the real contact was with Julian Bigelow when I was at Harvard University. It calls to mind the fact that one of the things I did at Harvard University was investigate the possibility of using vacuum tubes to do better switching than just two input ? And Howard Aiken was good enough to use some of the money to give to Raytheon in the hope that they could make a three control grid tube, vacuum tube, so that we could do three dimensional switching, and in fact they made some such tubes and I was able to demonstrate that you can do three dimensional switching with a three control grid vacuum tube and thereby save an awful lot of vacuum tubes.

MERTZ:

Do you recall the tube number?

RUBINOFF:

The tube number I don't remember off hand, but that, too, is described in a paper by myself in the 1947 Proceedings, the same one that has Harry Fuller's article on the Nueriscope. And I remember Julian Bigelow came to the meeting because he was quite interested in that switching tube. And I think he gave me much more credit than I deserve because when you stop to think about it with hindsight it was a pretty obvious extension in a direction that had no future. But he seemed to think that it was interesting that somebody who had never been in the computer field could have advanced in something like one year with an AB and got Raytheon to develop a tube and put together something as a demonstration model.

That encouraged him therefore to be interested in the possibility of my coming to the Institute for Advanced Study. You recall that Julian Bigelow was in charge of the Institute for Advanced Study design program at the time. His background is working with Norbert Wiener at MIT during the war on Servomechanisms and guidance systems. And he was highly recommended by Norbert Wiener to Johnny Von Neumann to come and head up this project on a computer at the Institute for Advanced Study.

When I arrived at the Institute for Advanced Study it was essentially in June of 1948. Julian Bigelow was in charge of the hardware part of the project. Herman Goldstine was in charge of the overall project including the mathematics program that was going on simultaneously, which as usual included somebody interested in meteorology, which in this case was Jules Charney who was busily engaged in developing mathematical models and mathematical techniques which the von Neumann computer was going to be able to solve sufficiently rapidly that they would be able to predict weather. And as a matter of fact he did do a number of calculations, not in real time. He did some sample calculations that led him in the direction of at least showing that there was hope, that there was certainly promise, that a fast enough computer could do something towards better prediction of tomorrow's weather in less than 24 hours. Better hope of predicting tomorrow's weather using computers in using the mathematical models he had adopted. I think at that time he was working on what we called the ten thousand foot model. He looked only at the weather ten thousand feet above the ground, and from the pattern that he got in this strange slice over the earth 10,000 feet above the surface of the earth you were supposed to be able to get a reasonable good gross picture of what the weather pattern was going to be.

There were a number of other people who were doing mathematical studies. I didn't follow them too closely. My interest in the Institute for Advanced Study was primarily in the hardware and to it.

MERTZ:

Would you say that Herman Goldstine because of computational background was less involved in direct engineering aspects of the machine...

RUBINOFF:

Correct. Herman Goldstine had no background in engineering as such and found it difficult to follow some of the things that were going on in the electronic and mechanical design of the computer. It's interesting that he felt this sufficiently that he decided that he'd better do something about it. At that time RCA was coming out with their television kits, I think it was the 630 that they were coming out with. And he bought himself a kit for a couple of hundred dollars something of the order of 250 dollars or 300 dollars, and he put together the kit then bought himself a tube and a cabinet and he made himself his own television set which gave him some knowledge at least of what's involved in putting together a electronic--electromechanical equipment, and he also at the same time got some feel for what can be done with triggering circuits and switching circuits and the like, so he got some hardware experience. But at no time would he even claim to have any significant working knowledge of electronics or engineering.

MERTZ:

At the time you appeared on the scene at the Princeton project the decision had already been made not to use the Selectron which Rajchman at RCA had been working on as an alternate.

RUBINOFF:

That's correct. It isn't entirely correct. The decision had been made to look for alternate ways of going because there was little concern that it might not pan out. There was still some hope that Jan Rajchman would come through. The history in fact is interesting. The Von Neumann machine that was built at the Institute for Advanced Study was conceived here at the Moore School of Electrical Engineering and somewhere there is a document which I had in my hands at one point, and that precious document is now missing. It is a report written by Von Neumann in which he says that the way the ENIAC was built is not the way you build a digital computer. But there is a generalized version, there is a basic notion of how you go about doing arithmetic, and there is an algorithmic approach, and if you want to get speed you line things up in parallel and you get everything doing the same thing simultaneously and the control is very simple. You pay a little more for the arithmetic unit but the control is simple. He said the way we must go is by means of the parallel approach, asynchronous, because you don't want it to be synchronous, you want things to run at their own speed. Von Neumann had an amazing insight into how things worked. Of course, he was a physicist and had expertise in quantum theory so one would have expected that he would have some feel for experimental situations. In fact, there is even a very famous paper written by Wing (?) which describes a novel circuit which Von Neumann did in later years, in the early '50's he did as a consultant to IBM and there is a patent that is issued on it, and it is a multiphase switching circuit which is a basic way of doing things today. That's a famous paper. __ published that in the proceedings of the Institute for Radio Engineers sometime in the late '50's. That could be traced down if you wanted to.

MERTZ:

At this time in 1948 were Willis Ware and Ralph Slutz...

RUBINOFF:

I was going to come on some of that history. That's where I was leading. That's a good question because they are certainly a propos. The machine was conceived at the Moore School of Electrical Engineering. Von Neumann then went out to look for funds. He also asked where would be a good place to put it, and of course since he was going back to the Institute for Advanced Study and since Jan Rajchman had indicated that he could make an asynchronous parallel type memory via Selectron he said the place to put it is right here in Princeton, it just makes good sense. And it did make good sense all around. And he pulled together a crew. And I believe he pulled together the crew in 1945, and that included Ralph Slutz and Julian Bigelow and Herman Goldstine and perhaps some others at the time. I'm not quite sure when Richard Melville, Dick Melville the mechanical engineer came but I'm sure he came fairly early in the sequence of events.

MERTZ:

Let me go back to this document that you were referring to authored by Von Neumann prior to the project, at the time of the ENIAC.

RUBINOFF:

About 1943-44.

MERTZ:

Do you know where it might be now.

RUBINOFF:

Oh, I knew you were going to ask that. I think I'll take one more look in the hope that I can find it, but I searched diligently at one point to try to locate it. It just occurred to me it might be stuck in with my material which was the basis for a manuscript for a book I never published and maybe I stuck it in there. I've probably looked there three times, but I'll look a fourth time just in case. And that's on tape so I'll remember it in case I forget. I will look for it because clearly you would like to get your hands on that.

MERTZ:

There might be some other pieces of documentation for that book that you were [?]

RUBINOFF:

No, I know what's in there and there isn't anything in there that has historical significance other than that one document. The others are in the public domain.

MERTZ:

You were discussing now the inception of the project.

RUBINOFF:

They were there and at that time Ralph Slutz was arguing that in spite of the fact that Von Neumann thought it ought to be implemented on an asynchronous parallel basis that's not the way technology will allow systems to be built. And Ralph Slutz and Julian Bigelow debated this question I guess right through '47 and '48, and when I arrived in June of '48 Ralph Slutz had just left a month or several months earlier to go to the National Bureau of Standards to work with Sam Alexander on the computer which later got to be called the SEAC. And Ralph Slutz went ahead with diode switching and transformer coupling from vacuum tubes which then formed the basis for some of the work that was done over here. It formed the basis for some machines that later was built for the Navy in particular [?] but that's no longer early history.

The thing that is early history is that Ralph Slutz did got to the National Bureau of Standards and designed and built the SEAC which was done in something like 18 months and running at that time. And as I say, it was vacuum tubes with transformer coupling and diode switching and magnetic wire input-output. Those are key items.

At the Institute for Advanced Study when I arrived people were just about giving up home on the Selectron. They were then asking what is the best way to go. And I was put to work to study what might be a good way to make a magnetic drum memory. While others, notably Jim Pomerene, were put to work on how the Williams tube memory might be used instead of using the Selectron. And the Williams tube did in fact progress remarkably well largely through Jim Pomerene's meticulous care in looking at all the problems associated with working with such a delicate device. The Williams memory is a delicate device. You have to be very careful to understand just how you position the spots, what you do with the spot, the type of modulation you put on the spot, in order to discriminate between zero and one, an one of the big headaches of the time, the so-called read around time, the number of times that you could read information or write information in the neighborhood of a bit without destroying the bit because of secondary emission or electron splash. And he did a magnificent job of developing that.

At the same time Willis Ware joined the Institute for Advanced Study just about the same time I came, within so few days that I'm not quite sure who came first, and I wasn't even sure that he hadn't been there for months until they put Willis Ware and me together in the same office, and we sat the two of us in the office for a period of a couple of years. And we were together.

Willis Ware worked on the adder. The Institute for Advanced Study adder was interesting in that it was not a logical adder. It was an analog adder. It was still a binary adder, and in binary addition you get a sum of zero, one, two, or three in any one column and therefore would add up a known amount of current through a fixed resistor and you would either get no volts, one unit of voltage, two units of voltage, or three units of voltage in order to indicate just what the voltage was.

It was also interesting in that they tried to make it self-timing as well as asynchronous. They were trying to make the adder itself know when the addition was over, and that didn't work, so they then had to make it asynchronous by introducing a delay circuit, a single shot (?) the time interval type circuit that measured out an amount of time which was adjustable. And they then studied how the arithmetic unit worked and adjusted that single shot to take just a little bit longer than the longest time it might take for the adder to do its job. Everybody knows the problem with that is not the addition time itself but time for one--for carry (?) to propagate across the register. It was a 40-bit register, by the way.

MERTZ:

This was the maximum time?

RUBINOFF:

It had to stay long enough for the maximum time. If it was self timing then you see it would only have stayed long enough for the action to be complete, which normally is something Von Neumann later proved that system was something like one-eighth, on an average it was one-eighth the maximum time, so in a sense the adder was slowed down by a factor of something of the order of eight, and the multiplier also.

MERTZ:

And originally there was no high speed carrier?

RUBINOFF:

It was straight forward carriage from column to column, just ordinary logic. You're thinking now about the techniques that were developed later by Gilchrist, Pomerene, and Wong. Gilchrist was a later addition to the Institute for Advanced Study. He came probably 1949 although I'm not sure of that. Pomerene of course was there the whole time. Wong was definitely later. And this was after they had been quite a distance along and the machine was essentially fully designed and done, that they developed the [?] high speed adder.

MERTZ:

Was Peter [?]

RUBINOFF:

Yes, Pete Bernardo [?] was there. He was an interesting character. Was he a mechanical [?].

MERTZ:

He was essentially a technician.

RUBINOFF:

He was essentially a technician, right, and he was climbing in and out of the place. There was also a tall thin girl and she was famous because the way the machine was made one of the registers of the machine was connected to a second register by means of wires, very fine wires that were strung right in the middle of space, and in order to get tubes in and out you had to put your hand in between a pair of wires and twist your wrist and get at the tube and hopefully pull somehow. They even developed a tool that you could clamp onto the tube to pull the tube out and you had to be rather tall and have a very thin hand and wrist. And she was assigned the job of tube changing because she was tall and had thin hand and wrist, thin arm that she could reach in and change tubes.

MERTZ:

[?].

RUBINOFF:

Yes. Ted Hildebrandt was there. His father was a mathematician and he was also an electronic engineer. From what I recall the key people were Willis Ware and Jim Pomerene and Dick Melville. Dick Melville as I say was a mechanical engineer who was a wizard. He was able to buy relatively inexpensive mechanical equipment, [?] and the like, punches and dies, and all the fabrication of all the chassis which were open [?] type chassis were done right at the Institute itself. They didn't have to send out to do these things. Very clever about how he would put wafers of insulator and conductor together to make strip lines which had become the way to do things in order to keep noise down. It's a technique which still has its place in high speed electronics.

MERTZ:

[?]

RUBINOFF:

They had just moved into their new building. They were in their new building by then, yes. I was never in the old shop.

One person we didn't mention is Jack Rosenberg. Jack Rosenberg was an electronic engineer who was given the job of stringing up an instrumentation. He was famous for his interest in hi-fi. These were the days when hi-fi was just making its mark. And I remember that he purchased his own recording device and he used to listen on WQXR out of New York every Saturday to Toscaninni playing in New York and he would record it with tremendous high fidelity. Now tremendous high fidelity considering the antenna he had meant that you heard every bit of static, crackle and noise that came through, and you were proud that you could hear it all even after he had made the recording. But there is no question about it, it certainly was high fi. We were really getting from 15 cycles to 20,000 or 30,000 or something on his recording.

End of Tape 1, Side 1

RUBINOFF:

Speaking of Jack Rosenberg we might mention something else about the life at the Institute for Advanced Study. It probably was very stimulating. I don't know that we thought of it that way at the time. We found it very pleasant, but I think it was also stimulating, and that is that everybody lived on campus, and you could get to work and get home to lunch and back to work in less time than you can do it anywhere else. But it also meant that we had a tendency to congregate during the evening, and we got to know one another extremely well, and therefore I believe we not only...I think it meant it made it possible for us to work together extremely well. Fortunately we were friendly. There were no antagonisms. It is rather interesting that since the situation like all situations will have political overtones and undertones it is rather interesting there were no open battlers between people. There might have been the usual competitive spirit, but by and large it was a team that was both working as a team and very friendly.

I think part of that was because we pretty much lived together and in living together we cooperated and the group was somewhat inspired. Whether that was Von Neumann or

Goldstine or Bigelow, I suspect it was largely Goldstine. Goldstine and his wife Adele Goldstine. They are both extremely pleasant people. They are very friendly people. They had a tendency to wander around and engage in coffee klatches which might be looked upon perhaps as taking up time except that I think it served to stimulate and the net result was that you could see the lights on until late at night in the laboratory making up for some of the coffee klatches during the day and the work got done.

That's interesting because it took several years to design and build that machine, but it had nothing to do with that. That was another problem altogether. The fact is that the fellows really pitched in and really did work.

There was another Rosenburg, Milton Rosenburg, and he was another one of the engineers who participated in the general activities. The main thing I remember about Milton Rosenburg is that the city of Trenton condemned his house out from under him so they could put through a high speed highway, and he was one of the few people who decided he wasn't getting enough money and he spent a lot of time fighting city hall and I believe that in fact got a few extra thousand dollars for his troubles _________. In those days I knew even less than I know now and I don't know very much now about that kind of political situation, but it was very intriguing to me.

_________ the social life over there. The first summer I was there Julian Bigelow decided that he wanted to be a farmer, and he convinced the maintenance people that they ought to come in with a tractor and plow about an acre of ground. And the acre of ground was then given to anybody who wanted squatters rights. And I remember Julian Bigelow was very actively engaged in planting that some of the others who lived on the compound were engaged in planting vegetables and the like. And I remember that some friends of ours named Shult, Albert Shult, he also was from the University of Toronto so we became very friendly, and he was there with his wife and a couple of children, and we decided that we would do it right. And we did it right. We probably picked up a full quarter of an acre, and we planted beans and peas and watered down (?) and tomatoes, and you name it we had it, and it was a good year.

And I remember that we had very friendly neighbors when we knocked on the door offering free tomatoes for the first week or so, but then we found that our neighbors avoided us because they didn't want any more tomatoes. We were flooded with them. It was a year of good tomatoes. The next year they plowed again and very few people were interested. My wife and I decided that we would plant again that year, and that year there was a drought and nothing grew, so at least we didn't have to worry about alienating our neighbors because of too much in the way of produce. We got the opportunity to we got no crops. We understood how farmers can lose their shirts. We had invested a trivial amount of money, so we didn't weep too hard about that.

Again interesting, Dick Melville's wife Claire, felt that she ought to do something for the community, and she ran a nursery school for the kids, and our daughter went to her nursery school, and the children of many of the people in the compound went there. And it was a very good nursery school she ran and very helpful to the morale and spirit of the place.

There were always parties going on for one reason or another, and there was quite a bit of mixing of course with the members of the Institute and that too was stimulating. Being in the environment of mathematicians and physicists of great renown you couldn't help but get into conversations and discover that you were learning something. You also learned don't play poker with them, that can be expensive. You learned quite a number of things. But mostly they were of a theoretical nature. So much for the social life. Where did we leave off?

MERTZ:

One question. I gathered that the alter ego of Von Neumann in the project itself was Herman Goldstine.

RUBINOFF:

Yes. Von Neumann very seldom visited the computing center, very seldom came to see what was being done. He got his information either from Herman Goldstine or from Julian Bigelow. And I would imagine on some occasions there were _____ being held because Goldstine didn't quite know why it was taking so long and Bigelow had reasons why it should be taking so long, and there may have been many discussions.

MERTZ:

This does lead into one question, what is I believe referred to as the Von Neumann factor whenever people outside of the project asked Von Neumann when the machine was going to be ready he would say in about six months.

RUBINOFF:

Right, that was the six months factor. He was not the only one who was doing that. The SEAC people were saying the same thing, and Eckert and Mauchly were saying the same thing about BINAC. It was characteristic of the electronic computer people that they were viewing things through rose tinted glasses. They still are so I don't know whether anything has changed. Pick a number and use it over and over again. Six months is a reasonably good number because it isn't a year and yet it's a reasonable period of time.

Dr. Hopper had an interesting statement. All of these people who were working--this is up at Harvard now--1946-48--she had an interesting comment. All of the people who were developing electronic computers were coming up and looking at the Mark I and saying that archaic thing. We are going to be a hundred times, a thousand times faster, it's obsolete. And her answer was, I apologize if it's obsolete, but this one runs. She used to stand her ground on that basis. And the fact is that one was running for many years before there was an electronic computer doing anything truly useful.

RUBINOFF:

I think the delay was primarily because it was a research environment and there was a tendency to confuse whether this was a computer supposed to get done or whether it was a computer that was supposed to develop techniques. And I'm sure this was part of the discussion that took place in Von Neumann's office when Goldstine and Bigelow and Von Neumann got together.

MERTZ:

Wouldn't there naturally be--at least I'm aware of a number of projects in this era--a king of natural tension between mathematicians, that is users and engineers, for the very obvious reason that users in that era had nothing to work on. After a while there was a great desire, an urge on their part simply wanted to run problems on the machine, since they spent very little time doing it.

RUBINOFF:

I guess they were eager to get started, but there was no other machine they could get on, and even when SEAC was doing they looked at SEAC WITH 256 words of memory and said we couldn't do anything with that anyway. And I think the mathematicians consoled themselves with the thought that when it's done it's going to be a really good machine rather than something slow. In fact, that's an important thing to point up.

As I said, Harvard was going the direction of magnetic drums, and that means milliseconds or thousands of operations per second. Whereas, the Institute for Advanced Study was going with the Williams memory because they wanted millions of operations a second, and certainly they wanted a hundred thousand additions a second. They arranged for a ten microsecond add time. They aimed for it and in fact they succeeded in getting something of the order of ten microseconds for add time.

MERTZ:

This is maximum time for adding ?

RUBINOFF:

That was the time, because remember they put a timing pulse around the adder and therefore every addition was ten microseconds as a result. And that timing pulse ran about 10, 11, 12 microseconds, something in that neighborhood. I'm not sure what the last number was because I wasn't there when the machine got finished.

The other thing was that Howard Aiken felt that magnetic wire was not a good way to go because magnetic wire would never be reliable, and in that connection he turned out to be right. On the other hand, he said he would go punch paper tape, teletypewriter tape because you could also buy a teletypewriter and prepare the tape and it was reliable and it was electromechanical and he was a mechanical engineer, and he kind of liked it and he was talking about relatively slow machines. Actually he ended up with something on the order of a hundred operations a second as compared to shooting for something like 10,000 to 100,000 there was a factor of a hundred in speed over here, but paper tape was certainly good enough for him from the standpoint of input and output.

The other interesting thing is that Von Neumann always felt that any computer worth its salt was going to take in a lot of information. Run a magnetic wire full of data into the machine and then compute for three or four or five days without stop and then come out with an answer yes or no so input and output didn't mean much to him. He didn't care. On the other hand, the magnetic wire research really did hold the Institute for Advanced Study back because it never was truly successful.

MERTZ:

Would you say this was then one of the major concerns of Bigelow?

RUBINOFF:

Bigelow was very concerned about...well he was concerned about all the mechanical things. I think part of the trouble there was he was looking for perfection before he got something running. Even to the point that certain things were put together and he would take them apart and put them together again because he felt that they had not been put together in a way that was satisfactory. Now the trouble there is you could never tell whether he was doing it because he was seeking perfection or he was doing it because he was worried about reliability. Nobody ever had the courage to try a machine that fast before in quite that way. And putting a machine together and finding that it failed every three seconds, you know, didn't do you much good. It had to be reliable.

MERTZ:

I had heard _________ felt that he felt he was designing for the ages in a sense.

RUBINOFF:

That's why I say he may have been seeking perfection or he may have been seeking the ultimate in reliability. And just what went on in the back of his mind is something that he may know, and maybe even he doesn't. But there's no question about it. There were a number of occasions when the system was partially built, and there was reason to believe that it should be left that way and he felt the thing to be done was make changes and he took that portion of it apart and put it together again and lost perhaps a month of time in so doing. And that was precious time.

He also was quite concerned about the Williams memory which was the ___ the reliability that was needed, and I think Jim Pomerene did an extremely good job over there but being first is a lot harder than being second. So Ralph Meagher who came along and was able to profit from what Jim Pomerene had done, was able to put together a Williams memory in the ILLIAC extremely rapidly because he knew already what the results were whereas Jim Pomerene was essentially working with something that was being put together and had all kinds of solder joints that had been unsoldered and resoldered and had all kinds of problems associated with it.

My contribution by the way to that later on after I got away from magnetic drum work when it was obvious that magnetic drum would never give the speed that was wanted, they took me off magnetic drum and they put me onto an output printer. It was decided that maybe we would at least have to have a fairly high speed printer. I worked using something called Alfax (?) which was a wet electrolytic paper that generated color by passing an electric current through the dampness and that gave you a colored marking by essentially electrolyzing the liquid on the paper. There were troubles with that and I was not happy with it, the biggest problem being that the paper would swell when it got wet and then when it ran through the machine because it was swelling it would tend to fold and you would get little ___ of fold and it would be just a mess. And besides wet paper is not good to work with in any case.

At that time somehow or other I got to hear of something called Calodaltos (?) and being young and not knowing better I went to Western Union and got some calodaltos from them. They were using it for transmitting telegraphs over remote lines and actually getting printouts of text by using this caladaltos paper, paper on one side and metallic material on the other side, and you put a few hundred volts between a stylus and the backing and it burned a hole through the paper on the front and there is carbon in the middle it turns out, and so you go from gray to black and then you get a black print. Since it was a dry paper I was able to demonstrate that caladaltos would in fact give the kind of print that Julian Bigelow wanted and do it at something like 20,000 bits per second. We were looking for a lot less than that, so that's the way that went. But after I left Willis Ware was then requested to finish that, and I believe that Willis Ware has a paper on the subject somewhere in the literature, I don't know where, but I believe he finished that printer and in fact has described it somewhere in the literature.
I was then turned over to do something about the power supply for the Williams memory. They were quite concerned that some of the problems might be, because the power supply was not stable enough, and it would have to be stable to on the order of one part in oh a hundred thousand, maybe a part in a million. That meant hopefully even no noise to that extent. And so I enjoyed learning more about voltage regulators and devices for eliminating noise from the power supply. That was my chore, and then finally I left.

MERTZ:

RUBINOFF:

There were a number of levels of power. I forget what all the voltage levels were. This one was nominally 1,500 volts, that is very low current level ____ a number of microamperes. The problem was there was only 50 mill volts of ________. and so the noise level, the stability of the power supply, particularly over short terms, you didn't accidentally introduce some jitter into the electrons flow and make the read around ratio even lower, that is, get errors in the writing or the reading. Because you got...

MERTZ:

I was going to ask you about transients ______.

RUBINOFF:

You don't have transients in those power supplies or you're in trouble, on the Williams memory supply. Just for the storage tubes, they were extremely sensitive. They were so sensitive that the tubes themselves were given conical fittings consisting of a good magnetic material, I believe it was permaloy, and then a cone of copper, and then a cone of permaloy. The permaloy was to make sure that there were no magnetic couplings, and there were some, as a matter of fact without the cone, because they had a sixty cycle power transformer outside the building and the radiation from the 60-cycle power transformer was sufficient to introduce distortion in the electron path enough to make the Williams tube fail on occasion. But with putting the magnetic shielding on that cut down on magnetic perturbation and the copper in the middle was to cut down on electro mechanic, any time varying _____ that might get in of high frequency, they were kept out by the copper shield. In fact, the front had a piece of copper gauze on it and when you wanted to look in you looked in at the light spots through the copper gauze just to make sure it was completely shielded.

MERTZ:

Was there any other part of the machine, did you do the power supply for the whole thing or...

RUBINOFF:

No. The other power supplies I think were commercially purchased. ____ Supplies or somebody.

MERTZ:

RUBINOFF:

No, they were just standard regulated power supplies. _____ 5 percent and would hold 3 percent with no difficulty. As far as transients there, you just didn't want things to happen overly rapidly, so just the regulatory, the single stage of regulation of the power supply which also included a pretty chunky (?) capacitor was sufficient to keep those voltages essentially constant enough.

MERTZ:

What you did was

RUBINOFF:

You couldn't sell it. What I put together was made out of bits and pieces of things. By that I mean it would not be commercially marketable because it was string and sealing wax. That's the problem that we felt, you see. We were doing research at the same time that we were in production. So Ralph Meagher had a head start on us. He could say, gee, I know what's needed, I know how you go about it, but the right part in the right way in the right housings, get the components that do the job and I'll put it together right. So that gave him an edge.

MERTZ:

So it was a combination of features

RUBINOFF:

It was a Babbage sort of a __________. We were developing the technology and developing the circuits and developing the logic at the same time we were putting it together. Another way of saying the same thing, we would say today that it was not completely specified before the building got started.

MERTZ:

On the other hand, correct me if my impression is wrong, from the fundamental component point of view, with the exception of the storage tube, there was no basic component research in the way of transistors or tubes.

RUBINOFF:

There were no transistors...

MERTZ:

Resistors, excuse me, tubes capacitors, any of the fundamental parts there would be no research, you were using commercially available.

RUBINOFF:

That is correct. It had to use commercially available components, but now you remind me that I did do some work on resistors. One of the problems was whether or not resistors were stable, and we in fact did experiments by putting resistors in ovens to stimulate high frequency use in circuits, and we did thousands of hours of measurements on resistors, or various resistance values in various types ________ and manufacturers. So we found that some of the resistors had a tendency to be very erratic in their behavior and you could never know what was going to happen with them.

In particular at that time there was the Allen Bradley resistor which was also resistor, I understand, in fact, I'm sure it was, Ohmite, also Allen Bradley, and they were interesting in that they did drift with time and temperature, but they drifted in an interesting fashion. You could measure two or three of the resistors out of a batch and you could see immediately what was going to happen to all the other resistors in that batch, as time went on as a function of temperature.

MERTZ:

So there was a uniformity of performance.

RUBINOFF:

Yes. In fact, they were rather interesting in their behavior. They would go up in resistance value for a little while, which apparently was cooking the moisture out of them, and then they would gradually decrease in resistance value as they baked some more, and that apparently was the final cure that they didn't take the trouble to do at the factory. But we were then in a position where we had some control over the time variations in resistance values which was very important in the design of the system.

MERTZ:

This raises another question and that is the vacuum tubes themselves.

RUBINOFF:

The Institute for Advanced Study were the ones who first found...no, MIT was the one that first found in Whirlwind I they found that one half of the flip flop seemed to burn out faster than the other half. I don't know that we did any work on it. RCA was beginning to come out with vacuum tubes that solved the problem from our point of view. They were coming out with the non silicon cathodes so you didn't get barium silicate or whatever it was.

MERTZ:

That was a build-up of barium silicate.

RUBINOFF:

Right. If you talk to enough people you'll know more than any one of us.

MERTZ:

The project then did not do tube life studies as such, they relied on the existing state of the knowledge.

RUBINOFF:

I seem to recall that Dick Melville or somebody had some tube run ___ just to make sure that there would be long life. There was a pre-burn, that was just a check to make sure that the tube would be all right. That was just a check to make sure that the tube we were buying satisfied the requirements, the specifications that were put on them. But there was no tube research that I can recall.

MERTZ:

It's interesting that the Princeton project picked on resistors. I take it there capacitors, for example.

RUBINOFF:

I think there was a reason for this. The reason was they were depending on RCA down the vacuum tube problems. They knew that Rajchmann was aware of the problem, if Rajchman was worrying about it what was the point of them worrying about it as well. It was in competent hands.

MERTZ:

How about crystal diodes, was there any work...

RUBINOFF:

I don't remember any significant crystal diode work being done. I would be surprised if there weren't some tests made on diodes. In fact, vaguely I seem to remember either Willis Ware or Jack Rosenburg falling around with crystal diodes. I don't really recall.

MERTZ:

_________ on crystal diodes and stability and

RUBINOFF:

Oh, but in those days nobody in any installation would trust anybody else anywhere else. And particularly the Institute for Advanced Study was never going to trust Ralph Slutz and his gang down there at the Bureau of Standards.

So I would imagine that there was some work done, but I don't recall offhand who did it unless it was Jack Rosenburg. I don't even....Remember there weren't many diodes in the end in the Princeton machine. They were primarily vacuum tube machines.

MERTZ:

RUBINOFF:

Oh, no. In fact, that was the whole point in going parallel because you then would reduce the control and while you put it into the arithmetic unit it became a relatively simple machine.

Just to backtrack for a moment to some of these social highlights or low lights or something. It's interesting to note that the team was primarily youngsters by a normal standard, people in their middle to late twenties. I was a pretty old guy, I was already in my thirties by then. I was 31-32 that made me one of the old guys already. And maybe some of the team spirit came from the fact that we were young. It was sort of our first crack at doing something exciting, and we pitched in because we were just gung-ho for this realm of science and technology and this great new burdgeoning field of computers.

MERTZ:

Also, wasn't there some sense of competition with several other groups, sort of in comparable...

RUBINOFF:

Oh, yes, there were of course the Harvard, and the Institute at Princeton, and the Bureau of Standards, and the Moore School, and out on the West Coast we heard about Harry Husky beginning to fire up--who was also an Institute for Advanced Study man who left and had gone off there...

MERTZ:

Was he Institute for Advanced Study?

RUBINOFF:

He was a National Bureau of Standards Man out on the West Coast as his was the SWAC.

MERTZ:

I believe he had been at Teddington (?) he had been exposed to British computer...

RUBINOFF:

That was of course the EDSAC, Wilkes and Gill (?) and the group there.

MERTZ:

I believe he had spent some time there prior to going to the Bureau of Standards.

RUBINOFF:

Now this was Cambridge. Wilkes was Cambridge. The Teddington group were more mathematicians, numerical analysis like Leslie Fox. Husky may have been at Teddington as a mathematician not as a hardware man. Wilkes was of course a hardware man with mercury delay lines and the EDSAC I, and his book on Bootstrapping, Forty Instructions that would get the System to the point where it would do its own ______. The four of them wrote the book.

Of course, all this competition was taking place, and Turing himself was wandering around the world telling people how things should be done and shouldn't be done, a philosopher turned mathematician, turned hardware man, and he was best as a philosopher I guess because it is common knowledge that Turing eventually committed suicide at about the age of 40.

MERTZ:

There was a counterpart in the Princeton group (?) Burks (?)

RUBINOFF:

Art Burts was an interesting person. He was there and as a matter of fact very important person. He was there, went to Michigan and then came back summers. And in fact, the reports that were written on the project were likely written by Burks, who would sit with the engineers and find out what the engineers had done and then he'd write it all down. And he was very close to Johnny Von Neumann, and we found later, he was the one who picked up the self-reproducing machine theory that Von Neumann developed during the period when he was very ill, and really on his last legs _____ Art Burks followed this extremely closely, pieced together the information, and then took a tour, one or more tours around the country lecturing on the cellular model that Von Neumann had developed for the self-reproducing machine. Art Burks had a very close personal relationship with Johnny Von Neumann. I'm sure he felt the loss of Johnny Von Neumann very keenly. And he was the scribe of the Princeton group.

MERTZ:

This gets back to another subject that I am interested in, and that is record keeping on the project. There are many different ways in which projects keep their records. Were there fairly comprehensive laboratory notebooks kept. You mentioned, for example, you worked on I assume calibrating the testing temperature _____ and voltage...

RUBINOFF:

We kept log books. I think that like most engineers we kept them when we had to and we didn't keep them when nobody was watching. But Willis Ware, for example is meticulous, and so is Jimmy Pomerene, ______ keep notes. I'm famous for keeping my notes on the backs of envelopes. I think I was just as bad then... No I guess it must be worse now, I couldn't be getting better the way I keep notes nowadays, so I was petty bad then.

MERTZ:

I understand from Pomerene that he still had two notebooks relating to his work on

RUBINOFF:

I have no notebooks. On the other hand, whatever notebooks I had I would have left them with the Institute. I would not have taken them with me, because some of the work I did might have been considered to be unique and patentable. For example, my magnetic drum work was done with a very fine wire about one mil in diameter. You couldn't see it. I was soldering it onto a plastic device and use it as a head. The reason for doing that was that the geometry of the situation was completely known. Therefore, the magnetic situation was completely known. Therefore, I was in better control of the situation from the standpoint of understanding what might be going on both from the recording and from the playback the sensing of what went on, and in fact I was able to make some inferences which I hope were true with regard to how magnetic spots, magnetic information, is in fact stored on the surface of these materials.

By the way, they didn't know how to make magnetic drums in those days. The Brush Recording Company which later became _____ Brush had developed a technique for creating a think skin of nickel alloy by electroplating in a hot bath while the drum was spinning, but that came a little later and they really couldn't do it under good control, and if you ever scratched it there was a great big mess. We created a material by spraying powder onto the drum much as it's done today except nobody knew how to do it then. And I became an expert with an artist's spray brush and developed techniques for using a lathe with automatic feed and automatic controls—I'll tell you, I became a mechanical engineer and an artist and everything else, which is why Dick Melville decided that he would let me use all the lathes and milling machines there. He said that he didn't think I was one of those electrical engineers who would cut his hand off at the wrist or something by using it, most encouraging type of way of saying you can use it too.
But, yes, we did our own…we in fact found that there was quite an art putting the material on the drum in such a way that we knew what we were getting on the drum because they were awfully _____ layers of magnetic material. I'm digressing. I would like to come back to the social material. Not only were we young and eager, but being young and eager we were very receptive. And there are two instances in particular, and I know who they are but I'll avoid associating them by name. One person on the project got very interested in a new theory called diabetics which was proposed by a man named Ronald L. Hubbard who first presented it in Analog Science Fiction.

MERTZ:

RUBINOFF:

That's ________ It was a hodgepodge metascience mixing psychiatry and psychology and electronics and cybernetics and anything he could throw into the pot. And he even formed the Dianetics Institute somewhere in the New Jersey area and was successful in getting all kinds of people to come and spend lots of time and lots of money at his Institute. But needless to say, with such a group there had to be at least one who felt that this was it—this was the solution to mankind's problems. And he became a rabid dianeticist or whatever the word is.

MERTZ:

RUBINOFF:

Or one over here Yoga follower—anybody who comes along with something different is guaranteed to get a reasonable number of followers, and somehow or other they all preach asceticism and living happily with poverty but guys get rich preaching it. There must be a story there which is perhaps self evident. However, Ron Hubbard did become very wealthy. His theory was soundly, roundly, and correctly debunked and five or six years later he was driven out of the Institute a sad but wealthy man and had to go off and retire and spend his millions somewhere. It's called crying all the way to the bank. However, one of the colleagues got involved and was an active proselyte trying to …an active missionary trying to get all of us to realize the importance, and the one thing it said in the book was that husband and wife must stringently avoid helping one another in the dianetics Path, and the first thing they did was, since we're so clever, because after all we're people at the Institute for Advanced Study, this couple proceeded to be the diagnosticians for one another and it was a mess. Within six months it was getting very difficult to live with them, and pretty soon they caught on and they stopped. So at least it is to their credit when things started going wrong it became pretty obvious to them that's nonsense. We had another case which wasn't quite so pleasant from the standpoint of the way it ended. And that is that one of the fellows felt that the Read method of natural birth was for him, only forgetting that he was imposing it on her, not on him. And he convinced her that she should have their first child by that technique, and she in fact either did, or was planning to up until…I'm sure she did, or maybe she was just up to the last minute, and psychologically the effect was very bad and she had to go through therapy for a number of months to recover from that particular escapade.

It's an interesting commentary that people who are research-oriented and creative don't necessarily always work on the basis of proper fundamental principles. They sometimes miss things on the judgmental level, if you will allow a philosophical comment that we'll delete later. I suspect we have an awful lot of that today on much too mass disseminated a level for our country to be able to cope with it.

MERTZ:

Would it be necessarily incorrect to assume that the age and enthusiasm of the group that it would be natural to expect enthusiasm in some new fads like that?

RUBINOFF:

I don't see how any one of this bunch could have failed to hit at least one. The surprising thing is it didn't hit all of us simultaneously one of these and completely bog us down. Oh, sure, that's why I'm saying it. These are open minds, receptive minds, continually challenging the status quo. And somebody comes along with something that looks like it has strong theoretical foundations, it borrows from fields that you don't know anything about, but you say, gee, it sounds reasonable, and you don't take the time to check it out, you go along with it. Sometimes with catastrophic consequences. I thought I would mention these because they give you a little bit of color on the people themselves.

MERTZ:

Von Neumann was not only the senior man on the project, but he was senior in years also.

RUBINOFF:

Oh, very much so. And we saw very little of him, which kind of disappointed us. There was a caste system at the Institute for Advanced Study. There were the permanent members like Von Neumann and Oppenheimer and the like and they were just one level below God. Except for Einstein, who had a smile for everybody, and he would say hello to you if he had never seen you before or if he had seen you a hundred times, he didn't know you from Adam but you got a smile. On the other hand the others were essentially one level below God. Then there were the members who again were young people who were invited to come for a sabbatical. They were one level above devils, I guess __. They were interesting in that they were feeling their oats, and then we were of course nothing but hack engineers. The fact that I had my PhD in physics and discuss a theory—at least discuss it, I couldn't create one, but I could discuss it with them was irrelevant. We were in that building. We were the engineers. We were not really even members at the Institute for Advanced Study, and that showed up on occasions when there were parties in the compound. Sometimes there was a tendency for the group to separate into the engineers from the computer program who were there to make a piece of hardware and they would go away someday, and the more genius-like individuals, the members who were there on sabbatical or the full members who…

MERTZ:

RUBINOFF:

It was a concession to Von Neumann even to allow it. That is correct. And it was done only because Von Neumann assured them that when the computer was done it was going to serve the mathematicians and physicists who had problems on nuclear physics to solve and problems in atomic energy which was what interested him. He was concerned with the quantum theory of particles in heterogeneous media with nonlinear boundary conditions, and that's a mess even today to solve on a computer. And he was hoping to solve that problem.

There is one other anecdote with regard to Von Neumann since we've come to him. There are a couple. You know about the one where he used to drive his automobile at 60 miles an hour lickety split down the road I forget the name, the one that ended up on the Engineering Building, I forget…my memory for names is almost impossible so either Lane or it's not.

Any rate, there is one other story about him. And that is he was once driving his automobile lickety split somewhere up in the White Sands area and off on a side road, and as he drove lickety split he went over some kind of a rough terrain and the cap on the oil chamber got knocked off by a stone in the road or something, completely oblivious of the fact that he was running out of oil he kept driving, the temperature went up, the thermometer showed it was hot as could be, and he just kept going until eventually the engine ceased, and that was the end of it. And his only comment was, "They don't make automobiles properly." One other comment on Von Neumann which may be interesting from the standpoint of the manner in which even the ultra great disappoint…occasion

End of Tape 1, Side 2

Start Tape 2, Side 1

RUBINOFF:

________ The other item with regard to Von Neumann is a very interesting one, and it comes not from the period of time when I was at Princeton, which was again '48 to '50, but it is a story that I heard a little later when the digital differential analyzer was invented by Floyd Steele, I think it is. Von Neumann was asked to look at the digital differential analyzer, and he flew to California to study it, and he is said to have made the remark that the digital differential analyzer is really the answer to the mathematician's problems. We don't need any further developments in computers. And it's rather interesting to ask why. And the answer is Von Neumann was primarily interested in problems of atomic energy. And the problems of atomic energy are primarily the solution of differential equations. And the solution of differential equations is precisely what the digital differential analyzer was set up to handle. He said, I have a problem. This machine looked like it will handle it. That's the way to go. I really don't care about any other problems. The fact that it served no useful purpose to businessmen or to other types of mathematicians escaped him at the moment he made the comment.

MERTZ:

Integral equations, for example.

RUBINOFF:

Oh, then you're very well aware of what I'm talking about. You probably know, then that the digital differential analyzer is passé and nobody uses them anymore. It has essentially got to the point where Electronic Associates is wondering whether it is going to stay in business even with the regular analog, never mind the digital differential analyzer. They served a purpose but they're not tremendously useful, again for reasons I could go into because I have a paper on the subject and know why they don't work even after I wrote a paper saying how wonderfully I can do a digital...

MERTZ:

When was this?

RUBINOFF:

That paper must be in the 1950 to 52 area. I did a paper together with Leondies, Cornelius N. Leondies, who would know, Professor at UCLA and in fact got his PhD under me here in about 1953. It was called DINA, Digital ____ Analyzer, a machine for solving the _____ diffusion ____ equations at high speed using a magnetic drum. The magnetic drum was my life blood at that time, obviously... (unclear)

MERTZ:

RUBINOFF:

________ I know him extremely well. He was here and worked on my projects for two years, something like 1953-55, ______ University of Pennsylvania. Then when I went to Philco in 1957-59, he came in to do the ALTAC language for the Philco 2000 which is my machine which I did when I was chief engineer for computers for Philco on a two-year leave of absence. And so we overlapped on two different occasions.

MERTZ:

I recently talked to him at home and he is particularly interested in the oral history of magnetic drum machines. So I assume that you would know...

RUBINOFF:

Oh, he would probably know quite a bit about it because we were here together for some time.

MERTZ:

I know he is interested in.

RUBINOFF:

The next time we get together at a meeting he can buy me a drink and I will tell him more. No drink, no tell. Where were we. I was off on Von Neumann and talking about how the ________ intrigued him. He thought this was the answer to every mathematician's prayer it was the answer to his prayers, and in fact it doesn't even answer it, because it's fantastically good for homogeneous differential equations. When you get to heterogeneous ones you get so many problems with what we call the ______ problem, the changeover to a new range of numbers, it's just a mess, and it turns out it didn't work. For a while he was tremendously enthusiastic about it.

MERTZ:

You said there was a sense of apartness, physically too...the building is not located.

RUBINOFF:

I said there was a caste system which is a little different. Apart that they can't get together even. You don't see one another in public. The caste system means you've got levels of intelligence (?) and there really was a caste system. Rather interesting that you could separate out different types of members and different types of full members on the basis of their willingness to engage in conversation or even associate socially with the engineers out of the Princeton computer project.

MERTZ:

They all tended to be true to caste?

RUBINOFF:

No. There was a mix. There were many exceptions. Some of these exceptions came from the divergences of background. There would be social reasons why...social background or educational reasons for similar orientation with regard to social or political (?) ideas that would cause people to gravitate together and would tend to break down some of the barriers on technical matters. But on technical discussions the mathematicians and pure scientists if you like had a tendency to look down their noses at the crass engineers who were essentially soldering

MERTZ:

RUBINOFF:

Oh, but this has always been true. At the University of Toronto the same thing was true. There was a man who was a rather famous man who worked with Einstein. His name was Leopold Infeld, he is no longer alive. He is the one who came to this country, wrote a few books, went back to Poland after the War. He is Jewish. He was driven out of the country because he was Jewish. He went back to Poland because they put in government and he felt that because they invited him into government he would make progress and show that really the Poles should not have the bad reputation they had. He helped to build up the educational activities in Poland over a period of three to five years. The last we heard of him he had been dealt a severe blow because after _____ ______ had essentially dislodged him from a position of prominence. But the reasons why Infeld comes up because when he came to the University of Toronto he came to become a solid member of the Department of Applied Mathematics, which existed because the Department of Mathematics which was headed by Professor Beesen was interested in pure mathematics and was completely down on applied mathematics and wouldn't even let applied mathematics into the Department, into the college, even.

And so when the Applied Mathematics Department was set up by a man named J.L. Synge, who is famous for his book, ______, on principles of mechanics, a very good man, right now in Ireland if he's still alive in Dublin, Ireland. And he was literally driven into a second-rate run-down shack on what was then the outskirts of the campus. And B essentially said if you must have applied mathematics please do it where you can't be seen. I was privileged to take courses in both Departments, and both of them had magnificent Departments. Neither one of them had a reason to be ashamed of their capabilities _____. But that's forever.

MERTZ:

But there are exceptions in Princeton at least in terms of Von Neumann's group.

RUBINOFF:

As I mentioned earlier, he was a man with tremendous practical sense. As I say, sufficiently practical sense that he could recite dirty limericks by the score and really entertain an audience for hours, so he was a very human person, and that's part of the reason why he had very little time that he could grant people because ______But I've heard it said by many people on many occasions, give me five minutes with Von Neumann and I'm willing to _____ to get it, because I get my problems solved, I don't just get an audience. Julius or Julian ______ up at Harvard had the same attribute. You could almost never get to see him but he was worth waiting a month for.

MERTZ:

You remained with the ________ computer until 1950. What then prompted you to depart....

RUBINOFF:

I decided by 1950 with four years beyond my PhD and earning not quite enough money to be able to save anything that I ought to go elsewhere and pretend that I had served my apprenticeship. And I cast around asking where I should go, and I discovered that there had been a tremendous upheaval here at the Moore School. Everybody, but everybody in the computer field was leaving. And as a result I decided that I would try to come to the Moore School which was a famous (?) place, and it was the right place to come at the right time. It also had one other attribute. I had been at a place where they were using magnetic wire and Williams memory and parallel machine and high speed, I had been at a place that was using relatively low speed with paper tape an magnetic drums and serial form, decimal arithmetic. At Princeton it was binary arithmetic. I said, the Moore School is a good place to because they are using mercury delay lines with transformer and diode switching, and all the serial kind of machines that I didn't have anywhere else, and everything was different. And so this would be _______. And once I got here I kind of like it.

MERTZ:

But at this point the pioneers of the era of....

RUBINOFF:

Eckert and Mauchly had formed Computer Control Company and we were busily designing BINAC which led to UNIVAC. Travis had just been called away by Burroughs to become the head of their research effort, I forget whether he became Vice President at that time or not, he may already have been Vice President. I remember him particularly well because he was still on the appointments and promotions committee of the Moore School and they sent me downtown to visit him in Burroughs to be interviewed, and after he went to the trouble of interviewing me for the Moore School he said, if they don't offer you a job, come on down. I could use you at Burroughs. I remember that to this day wondering what kind of people I'd be working with (?) should I really come here.

Then ______ and his group, Stu Eckert and Cook and they formed Technitrol. Then there were some others. Dick...hm. He had just gone to Aberdeen with the EDVAC computer that still wasn't working...

He was the Ralph Meagher type who went with the machine so that EDVAC could in fact be...Synder. He went there and in fact did get that ____ to run. When I think what he went through I discovered one of the things that has stood me in good stead many times since, the wire reader that they had designed at the Moore school for the EDVAC had been designed by three different groups. Part of it had been contracted outside by I think Reeves Instrument, and part of it was manufactured inside, and another part was purchased from a third place, and they brought the three parts together and nothing fit. They didn't even have the screw holes in the right place, just a mess. Which says something about maybe being a little careful like Bigelow said that it really will go together. He overdid it, they under did it. Talk about variety of computer experiences...

MERTZ:

Did you have any contact with Samuel Lubkin?

RUBINOFF:

Yes. As a matter of fact, his son Gale Lubkin was a student of mine her in the early fifties. Then Sam Lubkin also was one of the ones who left, was disappearing at the same time. Mostly my contact was with Gale Lubkin, his son.

MERTZ:

When you came to the Moore School what were you concerned with in the computer...

RUBINOFF:

When I came to the Moore School I was assigned the job of reconstructing and reconfiguring the computer activities ____ computer group. The man who was running the group at that time was Simon ______ who was an engineer with a bachelor's degree, who had obtained his experience on the EDVAC and really had been done a disservice. He was in an environment of people who did not really have the skills of Eckert and Mauchly. To begin with, they didn't have Pres Eckert who was a real genius. He didn't really have the support of John ________ Brainerd, who did an awful lot of things to make sure things went together in the days of the ENIAC. The EDVAC group was rather loosely held together. It was headed up by somebody who left in the middle. Dick Snyder was a Johnny-come-lately in the group. It was a travesty, because there was just turnover and chaos, and Si Goff was a victim of that chaos, and the result was that he was trying to develop a new machine called the MSAC for Moore School Automatic Calculator under army funds and was not overly successful. One of the people who was here at the time talked about interesting social stories (?). This is partly economic.

One of the persons who was working on the project was a young man called ____ Rooney who was one of my first students, my first masters students to do a masters thesis under me. I remember at the end of the year he was very unhappy about the way the MSAC was going He felt that leadership was lacking. In particular, Goff was beginning to experience a phenomenon like the Von Neumann phenomenon, when you get out of your depth you try to impose your will in the hopes that somehow or other it will come out all right instead of relaxing a little bit and letting some people who might know how take over and do the job. And so _____ Ruman (?) was a casualty of Si Goff's insecurity. I remember he said he wanted a job and he wanted to go, I believe to Raytheon. Bob Campbell was at Raytheon, so I spoke with Bob Campbell and gave Ruman a very good recommendation and _____ Ruman immediately got a job at something like 20 percent more than I was making. A guy who can't quite make it, and a graduate student for a masters degree gets 20 percent more than you're making there must be something wrong about the economics. And there still are.

MERTZ:

I notice you've taken leaves of absence to industrial...computer organizations.

RUBINOFF:

If you're interested in autobiography in that respect, from about 1950 to 55 while I was busily engaged in asking how do we build up a computer activity I realized that we lacked proper rapport with the community, the Philadelphia community. And I spent a little bit of my time trying to convince the powers that be that something should be done. Discovered that the powers that be didn't want anything to be done, couldn't be moved. And they said if you want to do it you go do it. But I felt that (a) I wasn't interested and (b) I wasn't trained to do that kind of management and political manipulation, and so I decided that that's not for me.

At the same time I felt that if you are at a University, particularly if you are asked to build up a computer group you ought to concentrate on doing that, so I would not be consulting until '53 which is before the '55 period. And I took that because a company called the Philco Corporation had invented something called the surface _______ transistor, and a man there called Herman Appel, Jr. had decided that surface ______ transistors might make good computers and the computer field was the way to go. He was making literally mechanical bombsights. Precision ground with a machine that weighed about a ton when it was done. And any part that wasn't precision ground and absolutely perfectly mounted and the whole system would come apart. But he was doing it, nevertheless, he was a fantastic mechanical engineer.

At any rate, one of the problems at the Moore School was that we were an electrical engineering department the transistor had been invented and announced at any rate in 1948. Here it was 1953 and there were no transistors in the Moore School. And so I got bribed into becoming a consultant for the Philco Corp. so I would find out what a transistor looked like. And so I started consulting with them on a day a week basis as the university allows, and made the fundamental mistake at the end of four months of succeeding in inventing something that was patentable, and from there on in I couldn't get out of being a consultant to Philco anymore. In fact, it was what they called direct coupled transistor logic, DCTL, which in fact was the basis for the Atlas guidance computer built by Burroughs under Isaac Auerbach, Ike Auerbach's supervision.

And in fact, there was another machine built at Bell Labs called Conductorcon which made use of direct coupled transistor logic, an DCTL in fact still exists today. They are still using it in integrated circuits and large-scale integration LSC circuits. At any rate, that's how I got into transistorized computers, namely at Philco. And I continued my consulting for them. I helped them get into the digital computer business. In 1957 I suggested that maybe I'd better come there and help them a little bit more because their youngsters had grown up to the point where they already knew everything. And in knowing everything they were leading the company along the paths that was going to make a digital computer from my point of view, at any rate, which was going to be a catastrophe. And so I wandered in 1957 for a one year leave of absence, and proceeded to redefine the Philco Transac S-2000 Model 100, and then at the end of six to nine months had that to the point where it was on the floor, so then we defined and designed the Philco Transac 2000 model 211 which was a factor of 2 in speed. Got that on the floor after circumstances that led to my staying at Philco a second year, very strange circumstances that indicate again that man's destiny is often determined by other people in strange fashions. We'll let it go at that.

I stayed at Philco a second year and we finished both those computers and we were well along to the completion of the Army ___ ____ computer and the Leeds and Northrop computer, the C-2000 which was their first digital computer for use in the digital control of chemical and petrochemical processes. I figured that was enough for two years and I went back to the University.

MERTZ:

You had a leave of absence for this time.

RUBINOFF:

So that was 1957 to 59 that the leave of absence. Then I came back here and got bored with life and opened a company, partly because the University looked even worse from the standpoint of association with the outside world, which I felt again was wrong, that the University owed something to the community.

And when I formed a company this...because I formed things lousily _____ this created some raised eyebrows and some questions. And the questions in fact led to meetings in which these things were discussed, and to a certain extent then led to the notion that maybe it's a good idea after all, and we now have something called the University City Science Center whose responsibility not it is to do these things, and as far as I'm concerned I'd be just as happy if I knew what to do to get rid of my company _____ right now. It's a software company, and anybody in software now is either going bankrupt or closing up before he goes bankrupt, so maybe I have a good chance to _________.

Just to show you what happens when people have a variety of interests like being in math and physics and a certain amount of chemistry and ending up in electrical engineering because they want to be in computers, and then getting involved with transistors and companies, I also felt that there was one other problem and that is that the city of Philadelphia was a very poor place for entrepreneurs to get venture capital in. And it's still not all that good. So I proceeded to express myself again rather vocally, and the net result is that there is now a venture capital group in Philadelphia which is strictly a risk venture capital...formed at least initially to support people in ______ technology who had bright ideas and might have one chance in a hundred of succeeding, but they should get some financial support. And in fact I'm still active in that group _____ _____.

MERTZ:

Did Isaac Auerbach have any contact with you during your Moore School…

RUBINOFF:

Ike Auerbach I first met in 1948 in Princeton, New Jersey, and we became very close friends. We seriously considered instead of my going into the University we talked about going into business together. We decided the time was not right, and I think wisely so, wisely on his part. I wouldn't have know. I was in a purely academic environment. He was in Burroughs at the time. Was he? I don't think he was in Burroughs but he was in industry at the time, and he had some feel for these things. He is an entrepreneur just built in. When I came to Philadelphia in 1950 he was delighted, and in fact he took advantage of my presence to see to it that now that he was at Burroughs that there was a closer relationship between the Moore School and Burroughs, and he was responsible, not only because of me but because of his own insight into what makes things go, he was responsible for Burroughs supporting a number of projects here at the Moore School, independently of myself, and also supporting some teaching at Burroughs by myself on the field of computers, and then we interacted in a number of local organizations. One of them was in the American Technician [?] Society interested in supporting the Israel Institute of Technology in Haifa, Israel.
And then we got to talking again or he got to talking again about maybe forming a company in 1956, and we chatted a little bit, and he convinced me that I'm not an entrepreneur. He didn't really want to. He wanted me to join with him. But it was quite clear that the way he talked and the way I talked were quite different. And he wanted to form a company to make money, and I was interested in forming a company to stimulate the environment. I really didn't care to make money. That would never go together. I was right. So he now has a company and is making money. And I'm still his friend. And we meet very often. We were in Nantucket accidentally we got together in Nantucket together.

MERTZ:

Then, you return to the Moore School in 1960?

RUBINOFF:

I came back in 1959 and I've been here ever since. Not quite so. I took a partial leave of absence last year to see what was happening in _____ and whether the company can be saved or not, essentially spending a fair amount of time trying to determine which way the company should go, and took many actions to adjust the company...

MERTZ:

Well, thank you very much.

End of Interview