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Computer Oral History Collection, 1969-1973, 1977

Interviewee: Dr. Jan Rajchman

Interviewer: Dr. Richard R. Mertz

Date: October 26, 1970

Repository: Archives Center, National Museum of American History

MERTZ:

Dr. Rajchman, would you like to describe your early experience and training for us.

RAJCHMAN:

Fine. I was born in London, England in 1911, from Polish parents. I had my very early schooling in England and then in Poland for two years. At age 10, I came to Geneva, Switzerland, where I had all of my primary and secondary education.

MERTZ:

Where in - in Switzerland?

RAJCHMAN: In Geneva, Switzerland. The reason - we were living in Switzerland is because my father had a position with the League of Nations. My moving from country to country entailed changing languages, which produced a certain handicap in the education, of course. In my years in Geneva I was highly interested in mathematics and physics. Also I was fortunate enough to have been in a school environment where manual dexterity was highly considered. We were taught how to actually use our hands to build various objects such as scaler, etc. It was a sort of laboratory environment, in which I learned to build things myself. In that period, - the late twenties, - many boys built their own radios, that is their own receiving radios. I was among them, and I built many radio receivers when I was in school, including super heterodyne receivers, which at the time were quite novel, the latest invention.

MERTZ:

Was most of your schooling there in Geneva in French then?

RAJCHMAN:

Yes. Most of my schooling was entirely in French. Actually, while English was the first language I learned to speak, I had more or less forgotten it. I learned it again in high school in Geneva.

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Jan Rajchman Interview, October 26, 1970, Archives Center, National Museum of American History

I graduated in 1930 from the College de Geneva which is the -- incidentally, as an amusing sideline -- a high school that had been founded by Jean Calvin....the famous reformer. I was in the so called scientific section of the school. The other sections, were the classical and the liberal sections. The scientific section, emphasized science, including calculus and physics.

RAJCHMAN:

Yes. From there I went to the Swiss Institute of Technology in Zurich, where I spent four years, and took the regular course in electrical engineering. In that period I was very strongly influenced by two professors. One was Paul Scherrer who was a professor of physics, nuclear physics was his specialty. He was a brilliant lecturer. He was an extremely inspiring, in that he could make students feel that they were at the frontiers of science, even though he was teaching the most elementary facts of physics. As many others I owe a great deal to his inspiration, which showed how physics is a fascinating living science and a very, very interesting subject. The other professor who influenced my career is Tank, who was then the head of the electrical engineering department and was one of the early pioneers in Switzerland in microwaves and high frequency radio techniques, and with whom I did a great deal of experimental work in the laboratories on amplifiers and other radio devices.

MERTZ:

If I might just backtrack

RAJCHMAN:

Yes.

MERTZ:

Was there any particular reason why you went to Zurich? Was it its considerable reputation? What decided you to go into electrical engineering? Were these decisions made while you were in the College de Geneva?

RAJCHMAN:

Actually, since you asked the question, it is a rather interesting one. I was of course strongly influenced by the French culture, and I was considering going to a French University. However, in the French universities, the best ones are the so-called grandes ecoles notably the Ecole Polytechnique and the Ecole Normande, admission to which are by competitive exam.

There is little chance to win the competition unless one has gone through the French schooling system, which is highly specialized. That was one of my hesitations. The other was that I was inclined to study mathematics, and perhaps also physics. However,

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my father was rather influential in steering me towards electrical engineering. His brother was a rather well-known mathematician who had been very prominent, particularly in the area of Fourier analysis. However, he never made out in worldly sense, that is, he had a very difficult time getting remunerative university positions and in the family there was the feeling that pure mathematics was not a way of making a living. The situation is certainly different today, as mathematicians are doing all right. Therefore, my father was anxious that I take up something that might lead to a more practical career. But actually that wasn't against my wishes because, as I said, I did like to play around with gadgets, make things myself. I built a camera. I built several radio sets among them a large super heterodyne set and a very small radio set using a crystal rectifier that fitted in earphones. That set needed no battery or anything else and permitted the reception of the local Geneva station. You could walk and listen as with today's transistor radios. These are examples of things that fascinated me at the time. So I certainly was perfectly inclined to take up electrical engineering. Moreover, of course, the Zurich Poly had a great reputation.

MERTZ:

But also, didn't it mean another language?

RAJCHMAN:

Yes, it did mean another language, which complicated my linguistic career once more. Now, of course, in Zurich Swiss German is the language of the town, and German is the language that is spoken at the Poly. In the first year the lectures were given partly in French and partly in German, and you were supposed to learn the other language in the first year so that in subsequent years you could take the lectures indifferently...in one language or the other. For example, the basic courses such as mathematics were given in parallel in German and in French. But in the second year they were not, and you were supposed to take the course whichever way it came, in whatever language it happened to be. So I did learn enough German to do that. However, I did not become very proficient in the regular conversational German. Actually Zurich is not the best place to learn it, because in town they don't speak it, but rather speak Swiss German. For social life you either have to learn the local Swiss German or get acquainted with French speaking persons. Actually, it was not difficult to find many French speaking people. There was a large colony of Frenchmen, Swiss French, and many other Europeans who spoke French better than German. So I had no problem getting friends. (And, as a sideline; many people learned the languages from girlfriends. And of course that was a very poor way, because the girlfriends were precisely the ones who were speaking Swiss German. The fact is that I did learn enough German to get along at the Poly, but I really never learned Swiss German other than the most rudiments of it. I graduated in 1934. The exams were taken after the summer vacation so actually I graduated in the fall of '34. In March of 1935 I came to the United States.

MERTZ:

Excuse me, what was your main area of interest during this time in electrical engineering?

RAJCHMAN:

I can't say that I really had an area. This was really a general course of electrical engineering. The so-called Diploma - Arbeit was simply concerning an amplifier, a high frequency amplifier, which wasn't a very erudite subject. After all, I was finishing graduate course corresponding roughly to a masters degree.

MERTZ:

Mhm.

RAJCHMAN:

In 1935 I came to the U.S. and, of course, my reasons for wanting to come to the U.S. were obvious. I was fascinated by electronics, which was the great new field at the time, and a field far more advanced in America than anywhere else. I had read Zworykin's paper on the iconoscope which I found to be exceedingly exciting. I thought this was a highly imaginative invention. Moreover, there was the depression. It was exceedingly difficult to get a position in Europe, particularly for somebody who was not a citizen of the country where he resided, which was my case, since I was then a Polish citizen residing in Switzerland. On the other hand fortunately America had the tradition of accepting immigrants from all over the world, even though there was great depression in America, too. But still, immigrant or no immigrant, everyone was on the same footing as far as getting a job.

MERTZ:

Did you - was your father still working with the League of Nations?

RAJCHMAN:

Yes. My father was still working with the League of Nations.

MERTZ:

These were difficult days for the League of Nations, too?

RAJCHMAN:

These were difficult days - yes. These were the days when the fascists were starting to become very prominent. So I came to the United States in March of 1935 with the explicit ambition to work for Zworykin - whose paper I had read as mentioned before. Also, as trivial sideline I wanted to hear the Philadelphia orchestra, and Stokowski whose

records I enjoyed very much.

MERTZ:

Did you know anyone here, in the United States?

RAJCHMAN:

No, not really. My father had many acquaintances that helped my way at the beginning, but I really didn't know anyone.

MERTZ:

It's sometimes difficult for someone to come....

RAJCHMAN:

It's extremely difficult. I was a perfectly classical immigrant of the textbook type, who arrived in New York seeking position and opportunity. Fortunately, my father acquaintances showed me the way and I got an interview with, with RCA, in Camden, N.J. I was interviewed by three people, one of whom was Dr. Wolff, who subsequently become a great personal friend of mine and also the Director of the RCA laboratories. Another was Mr. Kellogg who later played decisive role, in my joining RCA. But I did not get a job, I was told that I was a fine young man but there were no openings.

MERTZ:

You were about 25 then?

RAJCHMAN:

Not quite 25, late 24. And so I went to MIT for the summer session of the year 1935, for the dual purpose of learning English again and also getting immersed in the technical atmosphere. And I took some course - theoretical and experimental courses there. While I was there, I went to my first American Physical Society meeting at Cornell. Some of the friends whom I met at MIT invited me to drive with them from MIT to Cornell. This turned out to be one of the more famous meetings, as I later learned, as it was attended by Fermi and many of the physicists that were my later key in the Manhattan (atomic bomb) project. Of course I didn't know any of these people, but –

MERTZ:

You mean by reputation.

RAJCHMAN:

Yes, even by reputation. Zworykin was there and so was Wolff, from RCA. So I again talked to them, but still there were no openings. However when I came back to MIT one day I received a telegram from Mr. Kellogg saying that there was a position and that if I wanted it I should show up immediately. So the next day -- I went by plane as a matter of fact, which was rare in those days -- I was in Camden, and I got a position. This was in the factory, in the so-called testing laboratory. The job turned out to be very interesting. Namely, the -- I don't know whether this is going into too many details or not --

MERTZ:

Oh no.

RAJCHMAN:

In those days the three condensers that are necessary in super heterodyne receivers had to be matched. In order to do that the last blade in each condenser is slightly bent so that, the capacities of the three condensers are precisely matched as the shaft of the condenser is turned. Actually each condenser is compared to or calibrated versus a master condenser. The capacity of the master condenser is measured in absolute terms. In this way all the condensers in the production lines are made to have the refined exact values of capacity. The job of that laboratory was to make the fundamental measurements of high precision of the capacity of the master condensers. This turned out to be very interesting, challenging work. We designed ultra precise capacity measurements that could be done easily and rapidly. It was my first taste of an industrial problem that turned out to be extremely interesting.

MERTZ:

If I might just go back one - this was in the fall of 1935.

RAJCHMAN:

Yes. Actually I started in September 1935.

MERTZ:

In September? You had finished then the summer session at MIT

RAJCHMAN:

No, I didn't.

MERTZ:

Oh, you didn't finish.

RAJCHMAN:

I didn't finish because of the fantastic opportunity of getting a job. In any case the semester was almost through. I just left, I guess, a few weeks before the end.

MERTZ:

You were getting some help from, from your father..

RAJCHMAN:

Yes.

MERTZ:

financially?

RAJCHMAN:

Yes, financially. When I was at MIT.

MERTZ:

At RCA, did you work with any of the people that you had previously wished you would?

RAJCHMAN:

No. I really didn't. I worked in the factory and not in the laboratory. However, the laboratory was just a few minutes away. I and several times I talked to Zworykin and tried in vain to transferred to his laboratory. As it happens, one day I talked to a Mr. Clement who was the vice president in charge of the laboratory and All RCA and who had just come back from Europe. He was rather proud of his French so we talked in that language. Actually his French was perhaps not as good as it might be, but anyway, he was very glad to speak it. Somehow after that conversation the next day there just happened to be an opening in the laboratory, and so on January 1, 1936 I started to work for Zworykin, in the Camden laboratory. This was really what I wanted in the first place. So I consider myself extremely lucky because in those days getting a position was hard and getting one you wanted was even harder.

MERTZ:

And that was within three or four months of the time you actually came to work at RCA.

RAJCHMAN:

Right. And, of course, I had already acquired a certain advantage with respect to my first interview, namely I spoke English much better.

MERTZ:

Ah yes.

RAJCHMAN:

and I had also learned some of the technical jargon. My first assignment with Zworykin was to work on electron multipliers which he and some of his associates had already developed in the laboratory. The electron multiplier is a device for measuring very weak lights by multiplying the relatively few photo electrons by means of successive secondary emissions produced in a number of elect. A serious problem with that multiplier was that there was a "dark" current in it, that is with no light on it still there was some significant output current. This dark current limited the sensitivity the device that is it limited the smallest amount of light that could be detected.

MERTZ:

Like a noise signal?

RAJCHMAN:

Signal noise, right. And so the job I was put on was to try to do something about the dark current. I found the problem to be very interesting. I learned how, much about vacuum technology and how to make vacuum tubes. This was highly interesting in its own right. I succeed in making the tubes according to the design of Zworykin and associates, but I became very annoyed with the magnet that one had to put outside of the tube, because in that design it was the combination of the magnet i.e. field of the magnet outside of the tube and the electrodes inside of that tube not properly directed the electrons. So I first tried an electrostatic focused multiplier that did not require a magnet.

Well now, this in some sense brings me to my first contact with computation because it turned out that the equations describing the electron trajectory in electrostatic fields of arbitrary shape turn out to be not soluble in closed form except in, in very special conditions. For all the shapes of electrodes that were really of some interest lead to equations which were not soluble. So.. I first used an electrolytic tank to get the electrostatic potential and on a map of equipotentials their by graphical methods I could find the electron trajections. The turned out to be very laborious, but produced results. So then the next thing that occurred to me, as well as to an associate of mine, with whom I was working at the time, was to use a so called rubber model. A membrane of rubber is stretched over some metal pieces that were made to have the same shape as the electrodes in the tube, and little ball bearings roll on the rubber. If done properly, the surface of the rubber portray electro the potential and the paths that the ball bearings take

imitate the paths of the electrons. In a sense an analog computer the rubber model was. It was the very analog of the actual situation in the tube itself in the true sense. By bending the metallic plates that supported the rubber to various shapes and by rolling balls, from various starting points I was able to determine empirically the electrodes shapes which would steer electrons where I wanted them to go.

MERTZ:

Mhm.

RAJCHMAN:

This way I developed with my colleague Mr. Snyder the electro statically focused multiplier. It turned out to be a much more practical than the previously developed magnetically focused multiplier tube. And then I started to look at the causes of the dark current. (Synder was assigned to another job) This was in early 1938. In the summer of 1937 I had already...

I went to visit my parents during vacation time in Geneva. While in Switzerland I went to Zurich where I saw Soherrer and Tank who were both very interested in knowing that I had obtained a job in America. This seemed a fantastic event at the time. I asked them whether I could work on a doctor's thesis while being at RCA. There had been many precedents for this in American universities, but not in the universities in Europe. Or at the ETH (Swiss Federal Institute of Technology. And so they said, "oh, yes. of course with Zworykin that would be fine." An so it was agreed that this very work I doing on the multiplier would be a perfectly good topic for the thesis. When I came back, I worked on the dark current and eventually found what its causes were. I found remedies that greatly reduced the dark current to a valice limited by fundamental _____. I wrote a memorandum that became my doctoral thesis. And then, in the summer of 1938, I went back to Switzerland. I took some time off from RCA to study the whole summer in order to be prepared to pass the doctoral examination. I presented my thesis and also passed a short oral examination. I obtained my doctoral degree, Doctor in Sciences, in 1938.

MERTZ:

....Could you spell the names of the two professors?

RAJCHMAN:

Scherrer, S-c-h-e-double r-e-r, Paul Scherrer, P. And Tank, T-a-n-k, and his first initial, I think, was F.

MERTZ:

Hm.

RAJCHMAN:

So I then came back

MERTZ:

Then you spend, excuse me, two summers in Europe?

RAJCHMAN:

The first summer I just went for a few weeks, and—

MERTZ:

More or less a holiday, a vacation?

RAJCHMAN:

a holiday, and then I took a little time off in addition, to go to Zurich to talk to Scherrer and Tank. But, on my second trip, I spent three months studying hard, nothing but studying. And then I was ready to present the thesis.

MERTZ:

Was this a both written and oral examination?

RAJCHMAN:

No, it was only oral.

MERTZ:

Oh, only oral.

RAJCHMAN:

Only oral, as is usual at the Poly.

MERTZ:

Primarily defending your thesis?

RAJCHMAN:

Primarily defending your thesis. But also a general exam in physics and electrical engineering.

MERTZ:

This was in 1938.

RAJCHMAN:

'38

MERTZ:

'38. In the summer.

RAJCHMAN:

In the summer of 1938. And that was already - the Second World War was already in the air.

MERTZ:

One summer off.

RAJCHMAN:

One summer off.

MERTZ:

September of '39.

RAJCHMAN:

In fact, when I went to France for a little excursion that summer I was mistaken for a Frenchman, because I speak French like a Frenchman. There was a mobilization then and I had a hard time explaining that I wasn't French. This turned out to be just an alarm. The mobilization of '38 was immediately followed by a demobilization. When I returned in the fall of 1938 I did some multipliers particularly for astronomy.

MERTZ:

Did you continue to work in the research laboratory?

RAJCHMAN:

Yes I continued to work in Zworykin's Laboratory and continued to work on photomultipliers, particularly for applications to astronomy. I believe that I continued

that work for about a year (late summer or fall 1938 to late summer or fall 1939). I remember that in that period I supplied multipliers (production of multipliers started in 1940 I believe) to astronomers (notably Zwicky in California) as well as to other scientists who wanted to use these devices to measure very weak radiations or count particles. Many thought that the structure of the multiplier could be used in their own vacuum system. This turned out to be impossible or very difficult as the high photosensitivity of the cathode and the secondary multiplying electrodes depended on a very special processing with cesium which had to be made in a sealed - of tube. So I suggested that some materials could be illuminated by the radiation to be measured or bombarded by the particles to be counted, and thus produce flashes of light or "scintillations". These scintillation, in turn, would be detected by the photomultiplier, well sensitized in its own sealed off envelope. This method has been universally adopted and greatly improved since these early days. It is know under the name "scintillations counters". My work on multipliers was suddenly terminated as I started to work on computers.

MERTZ:

What made you shift to computers?

RAJCHMAN:

This is a very interesting story.- In 1938 the Germans had a great superiority in air power. Moreover antiaircraft guns were not too effective because it was not possible to compute fast enough the direction at which the gun had to shoot nor the time of flight of the shell. A very inspired colonel in the U.S. Army, Colonel Simon from the Farnford Arsenal in Philadelphia approached RCA, explained the predicament and asked whether electronics could be used to make fast enough "gun directors". He said he knew nothing about electronics except that it was fast. We believed that indeed electronic techniques could be employed for this function. I was assigned to the job.

MERTZ:

How did you start?

RAJCHMAN:

My first approach was to use what today we call analog techniques and which in those days we called: use of "simulating continuous variables." This seemed the most direct approach and one that resembled the mechanical directors of the time. After all... digital in electronics were cosmic ray counters that used vacuum tubes for counting cosmic rays. These were just beginning to be used and were rather elaborate. So I started to work on analog computers, and worked on the two essential ___ namely multipliers and a generator of arbitrary functions. The latter is needed on in the anti-aircraft fire control to simulate _____ tables. The multipliers is a key dense in analog computers. It is the device that produces an output proportional to each of two inputs. (The analog multiplier

is not to be confused with electron multipliers mentioned before. It has no relation to it) Addition and subtraction are almost automatic in analog computers but multiplication needs an explicit device. ____ is usually obtained by a multiplier used on a feed-back circuit arrangement. We devised various types of analog multipliers that were based on ordinary receiving tubes and also special tubes. However, I found it extremely difficult to obtain sufficient _____ and accuracy for the fine control antiaircraft director devices in the _____ the events in Europe became more alarming, and the whole project became much more pressing, more people were put in it, and so, gradually, the analog devices were taken over by a colleague of mine, Mr. Vance, who also worked with Zworykin, and I started to work solely with digital devices. I was joined by others notably L. Flory, G.A. Morton, and R.L. Snyder, so we formed a small group in Zworykin Laboratory. Though I did not "head" it, I worked on digital circuit for the longest time; as I was the first to start and remained for the longest period. The small group succeeded in developing key components of digital computers such as binary adders, multipliers, as well as shift registers.

MERTZ:

Were these –

RAJCHMAN:

All these made with tubes.

MERTZ:

Were these for fire control?

RAJCHMAN:

For fire control, yes. But actually, while they were ultimately aimed for fire control, we were really doing the research as though we had general computation in mind. Now it's true that the particular problems of the fire control were guiding us.

MERTZ:

The tables were being calculated elsewhere, I take it.

RAJCHMAN:

That's right.

MERTZ:

So you had those given. Were these to be put into a particular anti-aircraft gun control? Were a special table for each model of

RAJCHMAN:

Yes an anti-aircraft gun was necessary.

MERTZ:

Ah.

RAJCHMAN:

An essential part of the problem, was to invent an ___ physically implementation of tables that they could read very fast. We conceived of many solutions. The most interesting was the resistance matrix memory. I will dwell on it with some detail, because it turned out later to be one of the important components in ENIAC. Ballistic tables express the relation between the angular vertical angle at which the gun is aimed and the position of the shell at various times after the firing. They express a definite function, not expressed by an equation, and are an example of what we called "arbitrary functions" at the time. The tables are in a sense nothing more than a list of numbers arranged in two or more sets, so that a number in one set (e.g. flying time) corresponds to number in another set (e.g. the altitude of the shell). The object of electronic tables or "electronic arbitrary generators", is to construct a physical mechanisms that would establish the correspondences expressed in the tables as fast as possible. For that purpose I devised a "resistive matrix" which was made as follows. Many parallel "x" wires in one plane were mounted so as to be perpendicular to many "y" wires in another closely spaced parallel plane. At the intersection of each x and each y wire a resistance was either connected or else there was no connection. As a result, when a voltage was applied to an x wire, corresponding smaller voltages appeared on all y wires to which the energized x wire were connected. Hence a correspondence was established between the number characterizing the x wire, and the number (expressed in the binary code for example) appearing on all the y wires. Such an arrangement is called "read-only" memory in present day jargon. It was very simple to build. The desired pattern of resistances were drawn on a sheet of paper which was then glued to a bakelite board and a hole was drilled at each position requiring a resistance. A molded resistance (we used an 1/8 watt resistance the smallest available at the time) is inserted in each hole. Then wires were soldered along rows on one side of the bakelite board and column wires were soldered to the other end of the resistances on the other side of the board. While the arrangement is very simple it had an inherent difficulty characteristic of devices in which linear elements (resistances) are used as couplers in digital circuits. Clearly there is an ohmic ? connection between every x and every y wire even though there may not be a resistance at their intersection, because they are many "sneak paths" through all the other wires. Hence the whole matrix is really a monstrous short-circuit. When I first demonstrated the matrix to von Neumann he immediately pointed out this difficulty and hence was very spectacle about the scheme. The main remedy to this otherwise fatal, effect of the "sneak" paths was to use relatively high coupling resistances in the matrix and additional relatively low resistance coupling each column wire to a common bus. We established a

relation between the ratio of the high to low resistances and the ratio of the desired voltages appearing respectively on the desired and undesired wires. It turned out that very practical ratios of resistances provided very satisfactory ratios of desired to undesired voltages. We made a fairly large matrix array which had about hundred and fifty thousand resistors in it.

MERTZ:

Was this rectangular array or square?

RAJCHMAN:

Rectangular.

MERTZ:

Aha.

RAJCHMAN:

Rectangular array. And this combined with multipliers and adders really allowed us, in principle at any rate, to make a whole computer for controlling anti-aircraft guns.

But the pressure of the war diverted us to another goal. In the sense of urgency it became clear that our pioneer work could not lead to an anti-aircraft director that could be used in actual combat soon enough. As it happens our work was closely followed by several eminent persons. There were contracting officers from the Army and later, as we worked with the Navy, from the Navy. Among many consultants to the government were Stibitz and John von Neumann. I believe that most of these visitors were impressed by our inventions and technology and agreed with us that it would take too long to apply these novel digital techniques to fire control. As it turned out a much more pressing problem at the time was the computation of ballistic tables. These tables are computed on the basis of trial firings. The actual computations are purely numerical and were done by a number of operators using mechanical calculators, a relatively slow process (which also required excellent organization). New guns were designed but the needed tables for their use were lagging. It became apparent that it was just not possible to engage a sufficient number of operators. A computer for the job became extremely urgent (Incidentally John Mauchly who was running a group of operators was very instrumental in convincing the government of the need of a large computer). The need for a large computer was the genesis of the ENIAC. As I understand it, RCA was being considered as the prime party for undertaking the construction of such a large computer. Our group probably had the most expertise in the nascent digital techniques and RCA had the talent for large R & D programs. We estimated that it would take 20,000 to 30,000 vacuum tubes, an overwhelming number greater than that used in any electronic system in those days. Zworykin thought that a machine with so many tubes would never work, or at least for

only very short periods. RCA turned down overtures made by the armed services (Army I think). At the time I was very disappointed and sorry but I had not managerial influence. The building of the machine was undertaken by the Moore School of the University of Pennsylvania, and became the world famous ENIAC. We were asked to transfer all our know-how, and to cooperate to the fullest with the new project. I went to the University of Pennsylvania many times. The resistive matrix function generator (read-only memory in present day jargon) was adopted for various table in the machine. It was also used in the multiplier (or multipliers as there were several such) which was decimal. The restive matrix provided the elementary multiplication table. --Other circuits from RCA were adopted also, notably shift registers and a ring counter. Approximately at that period we had an idea which could drastically reduce the number of ordinary radio receiving tubes needed in the machine, by making a special tube. This idea was due to my associate Richard L. Snyder and myself.

MERTZ:

What was the idea?

RAJCHMAN:

The idea was to make a single tube which could multiply two numbers and add a third to the product, the numbers being expressed in digital binary code. A number of electron beams emanating from a single central cathode were deflected each by three electrodes, corresponding respectively to a digit of the multiplier, a digit of the multiplicand, and a "carry-over" digit. The beam's targets were internally connected to the deflecting electrodes, were electrically floating, and assumed one or the other of two potentials due a secondary emission mechanism. In effect, the tube was made by "integrated vacuum technology", as we would describe it today. The idea of the logic (multiplication and addition are one form of logic) performed by this new technique, requiring no conventional circuits, intrigued a number of persons in the government. In particular we caught the attention of Warren Weaver, who was then head of one of the committees of the NDRC, I forgot the name of the branch of the NDRC, -

MERTZ:

The subgroup –

RAJCHMAN:

subgroup, yes; Warren Wearer was chief of "Applied Mathematics Panel"

MERTZ:

Was this on synthesis in scientific computation?

RAJCHMAN:

Something like this, yes. Although it had a broader title that encompassed not only computation but also computers.

MERTZ:

Mhm.

RAJCHMAN:

In any case, he thought our proposal was a very ingenious invention and he gave us a contract to work on it. This was known as the Computron. And we naturally worked on that for –

MERTZ:

Do you recall when this was?

RAJCHMAN:

From end of 1941 to end of '42 or early 43.

MERTZ:

I'm just interested roughly in the chronology for the purposes of this documentation, of course we can get the precise dates. When you were working with some people at the Moore School, was this in '44?

RAJCHMAN:

At the very beginning of the Moore School Project, ENIAC, I believe it was, in the summer or fall of '43 and early '44.

MERTZ:

Then you had been working on the Computron before that time?

RAJCHMAN:

Yes, we worked in the computron from early '41 to early '43.

MERTZ:

Thank you.

[End of Tape 1, Side 1]

[Start Tape I, Side 2]

MERTZ:

I assume that the project that Warren Weaver's subcommittee of the National Research and Development Board was based on a written project proposal and resulted in a final report.

RAJCHMAN:

Yes, I have the number, it goes under the name of Computron, and I have the exact number as well as or copy of the report.

MERTZ:

And that was roughly about the same time as the start of the Moore School Project?

RAJCHMAN:

No, it was before.

MERTZ:

So, you had already been working on this problem for some time before the contract went to the Moore School.

RAJCHMAN:

Yes, that's right.

MERTZ:

Mhm.

RAJCHMAN:

You see, at the time (circa 1942) we were perhaps the most advanced group in the country, as a whole. However, since we did not become responsible for building the first computer very soon we lost that position.

MERTZ:

Was your group at all involved in contract with any of the people at Bell Laboratories who were working on fire control?

RAJCHMAN:

We were involved with Stibitz because he was a consultant, but that's all.

MERTZ:

I was thinking of the M-9 gun director. Developed in Bell Laboratories.

RAJCHMAN:

Yea, but the M-9 was an analog, wasn't it?

MERTZ:

Yes, It was an analog device.

RAJCHMAN:

That's right. I think Arthur Vance, who was working on analog devices and computers in our place, was in touch, with Bell Labs; but not I.

MERTZ:

I see. All right. One other parenthetical question in this connection. There, there was in -- around the middle of the war - at least by then - some interest in, and there had been earlier, but particularly by the middle of the war, in flight simulation problems. Air Force simulation, and Navy Air flight simulation; various groups were interested in this. One problem of pilot training, for example, with the Link trainer and the like. Another, in terms of other groups of aerodynamically simulated characteristics of new designs of aircraft. Did RCA get involved in any of this kind of problem? Or did you, get involved?

RAJCHMAN:

I wasn't involved in it. Now it's possible that RCA did.

MERTZ:

Mhm.

RAJCHMAN:

In fact, I'm pretty sure they did. However, you know at the time there was lots of security.

MERTZ:

Yes.

RAJCHMAN:

Even though I worked on classified contracts, I didn't know of other classified contracts, because I didn't have any need to know.

MERTZ:

Now were the problems that you were directly concerned with in your project that of developing digital computational techniques specifically related to the computation of ballistic tables, as distinct from any other kind of computational problem, so far as the sponsor in the government agency were concerned?

RAJCHMAN:

No. By the time we were awarded the contract from NDRC it was a generalized computer.

MERTZ:

I see.

RAJCHMAN:

The project was specifically for a multiplier and an adder, its purpose was completely general. The ENIAC, (started two years later) however, was originally meant for a specific problem, the computation of ballistic tables. There were unavoidable delays and by the time they the computer started to operate that problem didn't become as urgent. It also became evident that it was a good idea to make the machine more general. This was achieved at first by the use of plugging boards by means of which the nature of the problem, could be changed. Later came the idea that the plug boards could be replaced by relays. Since all other switches were electronic, very soon came the idea of using electric switches rather than relays to determine the nature of the problem to be solved, or rather its related computation. Thus, slowly the idea of a stored program machine evolved.

MERTZ:

Well, that is leading me to another question which I wanted to ask you, and that was: Was your project, you mentioned that George Stibitz and John Von Neumann had acted in somewhat of a consultative capacity for the NDRC project—

RAJCHMAN:

Not really. There were consultants to the services and were watching our work for them. They did not consult on the project.

MERTZ:

Aha. In behalf of—

RAJCHMAN:

On behalf of the Army and, and also the Navy.

MERTZ:

Did they visit your facility?

RAJCHMAN:

Yes, they did.

MERTZ:

And discuss with you some of the problems

RAJCHMAN:

Yes.

MERTZ:

I see.

RAJCHMAN:

Later also Herman H. Goldstine visited us.

MERTZ:

Mhm.

RAJCHMAN:

In fact, I think he may have visited us at the beginning (39 or 40) because he was a lieutenant who worked for CL. Simon.

MERTZ:

Aha.

RAJCHMAN:

But later he had another position, and so he also visited quite a bit.

MERTZ:

There was, if I'm not mistaken, quite an interest on the part of Von Neumann at this time also precisely with regard to the computation of ballistic tables. But he had some other areas of interest as well all concerned with large scale computational problems; in connection with the Manhattan project.

RAJCHMAN:

Yes. I know so now but of course did not know precisely what his interests were at the time because of the very tight security classification.

MERTZ:

And Von Neumann was also interested in meteorological problems as well?

RAJCHMAN:

That's right

MERTZ:

And so he had several interests.

RAJCHMAN:

That's right. He had several interests. It became clear that computers would need to be more generalized. Exactly when we started to forget the problems for which the machine was supposed to be built and started to work in earnest on a universal computer for all problems I can't say exactly because it was a continuous evolution. The idea of the stored program, as we know it now, and which is a clear-cut way of achieving a universal computer, wasn't invented overnight. Rather it evolved gradually. First came manually changeable plug-in, __relays, and finally the modifying contacts themselves became electronic switches. Next came the idea of storing the __ of those switches in an electronic memory. Finally this resulted in the idea of the modern stored program in which "institutions" and "____" stored in a common memory. As far as I know nobody in those days bothered to look up the literature. So credit to Babbage became an after-the-fact historical rediscovery. The idea of the stored program was... It is very difficult to say who the inventor was, because I don't think there was a single person. I think there was a small number of us who were in this field were thinking about

Computer Oral History Collection, 1969-1973, 1977

Jan Rajchman Interview, October 26, 1970, Archives Center, National Museum of American History

computers. Also we all knew each other. The people at the Moore School, and Von Neumann and Stibitz, Goldshine and a few others. The number of people who were in the field was very small. We all knew each other and when we had informal contacts, such as luncheon conversations it is in these that the board idea of the stored program evolved. Of course, Von Neumann was definitely a fantastic catalyst. Moreover he was vitally interested in critical problems not soluble without computers. Also his great genius. He finally crystallized the idea of the stored program computer that we know today.

RAJCHMAN:

He catalyzed people's minds, became a champion of the idea and pioneered the use of it. There is no question that his merit is tremendous. However, I think it's and oversimplification to believe that some day somebody invented the stored program computer.

MERTZ:

Do you recall any personnel changes during this time in terms of the group you were working with at RCA? You might wish to comment on some of the people who were working with you in the group at this time. You've already mentioned I believe several, Snyder.

RAJCHMAN:

Richard Snyder, Leslie Florey, George Morton,

MERTZ:

Yes. Did they have...Dis they have each of them a specific task that they were working on in particular or was there overlapping?

RAJCHMAN:

Florey and Morton developed many of the so called dynamic circuits that worked with pulses and which in fact perhaps are more akin to those used in the _tabel computers. I was more inclined to work with DC coupled circuits. This mode of compiling became widely used after the advent of the transistor.

MERTZ:

Mhm.

RAJCHMAN:

R.L. Snyder was a very prolific inventor. He thought of static multipliers and adders and

also an early storage tube which used an electron beam to store charges on a target.

MERTZ:

Did you by any chance have any contact with a man named Sydney Darlington? Dr. Sydney Darlington?

RAJCHMAN:

Yes. I remember Darlington vaguely. Now let's see.

MERTZ:

At Bell Laboratories.

RAJCHMAN:

I think I remember him, but I don't quite remember in what connection.

MERTZ:

Well, he worked with a man named Dean Wooldridge and some others in the development of a compensating scheme for DC drift in DC amplifiers.

RAJCHMAN:

Oh, the famous Darlington circuit.

MERTZ:

The famous Darlington - yes.

RAJCHMAN:

Oh, yes, that's right, but that, of course, is not related to digital computers.

MERTZ:

No, not to digital computers. That's correct.

RAJCHMAN:

Right. I think I met him in that period; but I must confess I've forgotten in what connection.

MERTZ:

He was - he was interested in, in DC circuit design related to computational problems but they were analog.

RAJCHMAN:

Analog, right.

MERTZ:

Yes.

RAJCHMAN:

I was a great proponent of the compiled digital circuits in those days. And then of course of the Computron, relying on DC ___ in that tube.

MERTZ:

Excuse me a second. If I might go back to the Computron: did you make some prototypes of the Computron?

RAJCHMAN:

Yes. We made prototypes of parts of the tube.

MERTZ:

Mhm.

RAJCHMAN:

We were planning a tube for 14 digits that is ___ digits. we never constructed a whole tube. Rather we made a four by four prototype. I believe, so it could multiply up to 256.

MERTZ:

Mhn.

RAJCHMAN:

However, I believe that in a very crude way it was in a remarkable system, even though it turned out to be very complicated tube.

MERTZ:

Where was the facility in 1941?

RAJCHMAN:

First in Damden and then moved to Princeton late in '41.

MERTZ:

Yes.

RAJCHMAN:

The early work was in Camden - and then we continued the work here.

MERTZ:

Where was the facility in Princeton?

RAJCHMAN:

Right here, at the RCA Laboratories, later dedicated as the David Sarnoff Center.

MERTZ:

Oh, it was right here?

RAJCHMAN:

Right here. Yes. This building was built in '41.

MERTZ:

Oh, it was. I see. Parts of it look like they were very new.

RAJCHMAN:

Actually the part we are sitting in now was built later,

MERTZ:

Oh, mhm.

RAJCHMAN:

but the other wing was - was built in 1941

MERTZ:

I see. And then you were one of the original occupants.

RAJCHMAN:

I was one of the original occupants, right.

MERTZ:

occupants who came from Camden.

RAJCHMAN:

From Camden, right.

MERTZ:

I see.

RAJCHMAN:

Yes. In '41, a group came from Camden of which I was a part and another group from Harrison, NJ moved to Princeton, New Jersey, and we occupied the building. I remember moving to Princeton just a week or two after Pearl Harbor. Much of the work on the Computron and some of the DC circuits that I mentioned, was done in Princeton.

MERTZ:

I see. So –

RAJCHMAN:

For contribution to the ENIAC project was in 1943 - 44 when we were already in Princeton for two years.

MERTZ:

Were you by this time.. still working in Zworykin's laboratory or had there been any change in the , in the organization of the research?

RAJCHMAN:

I was still working with Zworykin.

MERTZ:

Yes. Mhm.

RAJCHMAN:

But my position officially "Member of the Technical Staff" as far as title is concerned, didn't change. The organization at the laboratory remained very stable in fact it remained practically unchanged for a number of years, until Zworykin and Wolff and the managers of that generation retired. Then the changes occurred very rapidly then.

MERTZ:

Was that after the war?

RAJCHMAN:

That was way after the war, yes. When we finished the work on the Computron I started on another project that had nothing to do with computers, namely on a Betatron. This was early in 1943.

MERTZ:

Mhm.

RAJCHMAN:

In the midst of the war radar became increasingly _____. There was a great need for producing higher and higher frequencies. At that time (early 43 or late 42) the traveling wave tube was not yet invented or at least we had no knowledge of it. In the traveling wave tube the electromagnetic wave is slowed down by means of _____ electrode and thus electrons can be injected so as to keep on the crest of the wave and this can give energy to it. In contrast, our idea was to speed up the electrons to the speed of the electric wave very close to the speed of light. So that the electrons are also at the crest of the wave and thus can efficiently give up their energy to the wave. To do this successfully the electrons had to attain very high energies. Our idea was to use a betatron to obtain high energy electrons and then within the betatron put a resonant cavity. (Incidentally the betatron, due to Kerr, is an induction electron accelerator that utilizes an AC driven magnet. The magnetic field keeps the electrons trajectories to be almost perfectly circular, while the changes in magnetic field produces a tangential electric field which accelerates the electrons.) The idea of a betatron _____ generator was conceived by me and a colleague of mine, Bill Cherry. We worked as a team of two and we built a little betatron. (about 4 1/2" in diameter) that produced about 500 Kv electrons. We found that one of the problems in the betatron is the injection of charge. Only a relatively small amount of charge is actually accelerated. We analyzed this fundamental problem, that is, just what determines the amount of charge that can be trapped in the betatron. This analysis turned out to be a very interesting as it involved electron optics at relativistic speeds, with

combined electric and magnetic field, and the effects of the trapped space charge. We developed a theory leading to a simple model in which all forces could be derived from a potential function. The combined forces of the magnetic field and inertial forces produced a ring shaped regions within which electrons were probably trapped. When charge is trapped in that region, it reduces the forces tending to keep electrons within that region. Thus there is a limit to the amount of charge that can be accelerated. This limit charge could be calculated from the model potential function. We presented our findings an article for the Franklin Institute that later won us a prize from the Franklin Institute. This was no doubt due to the fact that we published a theory of charge capture which applied to all particle accelerated using circular trajectories. This theory predicted a relatively low limit so low that the original idea of the betatron microwave generator proved not to be practical.

At this point I would like to point out that we missed by a hair a way of capturing a relatively large charge, a ___ that was later invented by Christophos a Greek physicist who immigrated to the US. Christopholos had a very profound _____. In any of these accelerating machines, a key problem is focusing that is the keeping of the charge in the middle of the structure both in the radial and the axial directions. We have shown in the case of the betatron that a very strong focusing in the radical direction can be obtained at the expense of axial focusing and vice-versa. In other words a strong axial focus axially, produces a radial defocusing and vice-versa. The best one can do is to make the two equal. Then the amount of captured charge is the relatively small amount predicted by our theory. We..., Christopholos ides was that if you deliberately --I don't know if this is going into too many details or not-

MERTZ:

Not all, please continue.

RAJCHMAN:

deliberately focus very strongly in one direction, so as to deliberately defocus it in the other, and then quickly, along the path of the electrons, you quickly interchange the focusing directions, that is you focus it in the other direction and let it defocus in the first; then he showed, by intuitive insight, that that the overall effect would still be one of focusing. In the words, when electrons are alternately focused and defocused along their path then the overall effect would still be one of focusing, moreover the overall focusing can be very much greater than that obtained without the alterations. This type of focusing is now called "strong" focusing and is adopted in most modern particle accelerates. Interestingly enough we thought of the scheme during our work on the batratron but we could not fact the mathematical difficulties of computing tranjectories in an angularly varying field. Such computations are now routinely made on computers. Christopholos could not compute either but depended on intuition (and later _____results)

MERTZ:

About when was this? Was this contemporary during the war?

RAJCHMAN:

Yes. It was in 1943 and 1944.

MERTZ:

What did you do next?

RAJCHMAN:

I came back to the computer mostly thanks to Von Neumann. The war was finishing, and the idea of the stored program computer was well established and Von Neumann was determined to make a computer designed solely on it. As it turned out also he was interested in many problems other than the military problems, among others the weather prediction in which Zworykin was also interested. The two got together, and then decided to establish a cooperative program between the Institute for Advanced Study and the RCA Laboratories to work on an electronic computer with a stored program. This required a memory of course. We were to develop the memory for the project. Again I was assigned to this task, and started work in late 1945. I first considered an electrostatic storage tube, which was a natural approach that everyone was suggesting. The storage tube suggest at that time were very similar to television tubes and used an electron beam to deposit charges on a target. My own experience with deflecting electron beams (dating from my first work on analog computers in 1939) was that it was far from easy to deflect beam accurately and in a reproducible fashion. So I conceived a purely digital tube which I called the Selectron and immediately started on this approach.

MERTZ:

Would you comment on some of the people with whom you were working at this time? Were they the same?

RAJCHMAN:

I was working all by myself at the beginning of that project. Shortly thereafter, I was joined by colleagues who were definitely very junior to me, in the sense of age particularly. The first one was Milton Rosenberg, who later left RCA and had a career of his own. But I am jumping to the modern time.

MERTZ:

He was an electrical engineer?

RAJCHMAN:

Electrical engineer, yes. And then there were others which I will mention as we go. The principle of the Selectron is, in a few words, as follows. Instead of aiming an electron beam at a selected position as is done in storage cathode-ray tubes, a rain of electrons on the whole target is used. This overall bombardment is interrupted in all target locations, but the selected one, by an arrangement of controlling electrodes. These electrodes consists of two orthogonal sets of parallel controlling bars, that in effect provide a "matrix" digital control that gives an absolute certainty of selection the desired location, as opposed to the non--too-certain selection by analog deflection of a beam. This absolute certainty is achieved at a price: the complexity of the selecting grid structure and by the necessity of sealing many leads through the vacuum envelop of the tube and of providing a control addressing circuit for each one. One of the main features of the Selectron was a drastic reduction of the seemingly required number of leads and circuits (from n in each direction to $2n$) by recognizing that the gate for the passage of electrons between two bars can be controlled by the potential of either one and the __ that gate is in effect an "AND" gate. It is interesting to note that the Selectron, a sort of "integrated vacuum technology" device was already plagued by the necessity of providing many leads to it, just as is the case in to=day "semi-conductor integrated chips". Another feature of the Selectron is the method of storage. The availability of electrons at every location of the target makes it possible to forcefully keep the potential at each target location at one or the other of two stable potentials (namely the cathode potential or the potential of the last accelerating electrode) by means of a natural mechanics due to secondary electron emission. This forceful means of storing information on the traged is in contrast to the less-than-certain means of storing it as a charge on an insulator, as was proposed in storage beam tubes, since that charge could leak off. At the beginning we had a great deal of difficulty in constructing the tube, which in a sense was not surprising since we were pioneering with a very small staff a new technology, that of "vacuum tube integrated technology". Eventually we succeeded in the laboratory and transferred the job of making the tubes to a product division of RCA, the Tube Division, at Lancaster, PA. That division made about two thousand tubes. Some of these were used in the Rand Corporation computer and worked without any failures for many years until they were superseded by magnetic core memories.

MERTZ:

SWAC? Did they use it in that computer out there? The Standard Western?

RAJCHMAN:

Yes, they did use it in SWAC.

MERTZ:

I think so also.

RAJCHMAN:

And also in Rand, yes, in two places

MERTZ:

Do you know, whether the Laboratory still have any of the prototypes of the Selectron?

RAJCHMAN:

Yes.

MERTZ:

They do?

RAJCHMAN:

Yes, yes we still have them. Yes. I can show you one if you want.

MERTZ:

How about, the Computron that is the sort of a mock-up of a four by four that you had mentioned?

RAJCHMAN:

I don't think we have that anymore because that was a structure that wasn't sealed off (but used in a ___ vacuum system) and so I think we threw it away. But the Selectron we have, as, there were quite a few of those tubes made.

MERTZ:

Yes.

RAJCHMAN:

During the time we were laboriously working on the Selectron the Princeton project at the Institute for Advanced Study decided to use an ordinary cathode ray tube that Professor Williams had developed in Manchester, England, because they were a little impatient with waiting for the Selectron. And so their machine worked with the Williams tube. (an analog device which proved to be very delicate) However, the copies, the SWAC and Rand computers which were supposed to be copies of the IAS machine and which really started to work at the same time, did use our tube. This was a peculiar fate of events. As far as my collaborators; Rosenberg was my main collaborator in those days. There were other persons one of whom was Igon Grosdoff

MERTZ:

Let me, be sure of the exact names. This was Milton...

RAJCHMAN:

Yes, Milton Rosenberg.

MERTZ:

Milton Rosenberg.

RAJCHMAN:

Right.

MERTZ:

Do you recall from where he had come to RCA?

RAJCHMAN:

From an university, directly.

MERTZ:

Ah. Do you know where he got his training?

RAJCHMAN:

M-mm. I don't remember which university.

MERTZ:

But this was more or less his first position.

RAJCHMAN:

It was his first job, right. In fact he was a student engineer, or trainee, first.

MERTZ:

He wasn't from MIT?

RAJCHMAN:

It was Lehigh, I think; but I'm not completely sure.

MERTZ:

You were about to cite another person.

RAJCHMAN:

Yes Igor Grosdoff.

MERTZ:

Igor Grosdoff.

RAJCHMAN:

Igor Grosdoff was a man of my age maybe slightly older, who worked on some of the early counters, **MERTZ:**Mhm. **RAJCHMAN:**and who developed a very good counter that worked up to a megacycle and then later to 10 megacycles, which, in those days, was very fast. **MERTZ:**Mhm. **RAJCHMAN:**He built also some of the circuits, to operate the Selectron. **MERTZ:**I see, where is he now? **RAJCHMAN:**He's deceased now **MERTZ:**Oh, he is, I see. **RAJCHMAN:**for quite some years now. Actually he was in the laboratory for some years before I joined. And let's see, there was also another person: Max Mesner who was an radio circuit expert. He is still at RCA. He's now working the Astro Division.

MERTZ:

Mhm.

RAJCHMAN:

Was also working for me then.

MERTZ:

And Max is the first name of Mesner?

RAJCHMAN:

Max Mesner was an electrical engineer, yes. By the way, I forgot one name, Ed Goldberg, who is also still with RCA with the Astro Division. He worked with me at my very beginning when I worked on analog computers and when I started with digital computers in Camden;

MERTZ:

Aha.

RAJCHMAN:

he worked for a short time with me.

MERTZ:

This was in the late thirties?

RAJCHMAN:

In the late thirties, yes. On the resistance matrix, for example, he worked with me for a very short time.

MERTZ:

Aha.

RAJCHMAN:

During the time I was working on the Selectron, I had contacts with many in the computation community. This included Aiken and his associates, several persons from the Moore School, government consultants and several Englishmen.

MERTZ:

Mhm.

RAJCHMAN:

In essence I believe that we had many contacts with most everybody in the world interested in computers.

MERTZ:

Did you sponsor any meetings here or, or were these contacts just informal?

RAJCHMAN:

Informal.

MERTZ:

Persons who dropped in when they are coming by or passing through.

RAJCHMAN:

That's right, mostly individual visitors.

MERTZ:

Was RCA represented or did they attend the famous summer symposium series of lectures on computers given in the summer (July 8 to Aug. 31) of 1946, I believe it was, at the Moore School? At which Von Neumann was a guest lecturer, Aiken gave a talk, I believe, Stibitz perhaps also?

RAJCHMAN:

Yes, I think –

MERTZ:

Eckert and Mauchly both were there,

RAJCHMAN:

Yes, I think I was there; in fact I think I gave a talk.

MERTZ:

Aha.

RAJCHMAN:

I think I did though I'm not completely sure. I think I must have given a talk. If I did it must have been about the Selectron, as by that time we had talks with Von Neumann and it was likely that we would be _____ Institute of Advanced Study. I think I emphasized the economy of leads.

MERTZ:

I do not yet have a set of the course but there were some printed. The lectures at the meetings were later printed up and distributed for the participants. I don't have a set of those, but there are several people around who do. They have, well, the Moore School has a set; and the lecture given by Von Neumann, the manuscript of that is over here in the Firestone Library at Princeton.

RAJCHMAN:

Mhm.

MERTZ:

Actually, I believe, identified.

RAJCHMAN:

Mhm.

MERTZ:

You might want to look through your papers to see whether you still have the notes of the course.

RAJCHMAN:

I could do that if you like. However there is a problem in that all my notebooks have been taken by the patent department in connection with the controversy with respect to the core memory patent.

MERTZ:

Could you tell us about the core memory

RAJCHMAN:

During our work on the Selectron I thought of the core memory in a ___ way. But the fact, is it's hard for me to imagine the day when I hadn't thought of the core memory. I thought about it for years before writing anything down because it didn't seem very elegant or practical to use a diode in series with each core. Yet this was necessary for selection because in those days magnetic materials did not have sufficiently rectangular hysteresis loop to permit direct current __ selection. But diodes were expensive and also required to be connected so ___ one wouldn't think of using thousands of them. As mentioned above the hysteresis loops of most of the magnetic materials available in those days were so far from being rectangular that you couldn't think of switching them directly, nor were the remnant states well defined. So, that even though I thought of using directly driven cores for years I always dismissed the idea, thought keeping in the back of my mind. What made me think of it very suddenly one day was a publication of a good square hysteresis loop material consisted ultra thin ribbons of permalloy. They were developed during the War for use in magnetic amplifiers by the Germans.

MERTZ:

Hm.

RAJCHMAN:

amplifiers for submarines, I think.

MERTZ:

How did you obtain these ultra thin ribbons?

RAJCHMAN:

The process for making the ribbons was brought into this country. The Armco corporation obtained the process and started to make these ultra thin ribbons, and in some magazine Armco actually published the article I saw. The loop was just as square as one would wish it to be. When I saw these loops it appeared obvious to me that it was possible to make a memory with that material. So I went to see the Armco Company which happened to be at Philadelphia.

MERTZ:

This is Armco?

RAJCHMAN:

Armco, yes;

MERTZ:

Mhm.

RAJCHMAN:

and asked them whether we could get some cores made from the ultra thin _____. These turned out to really have the ____ characteristics. At this point I think I'm right in saying (though of course it's very hard to always remember exactly what you thought some time ago) that it would be fairly obvious to make a memory from individual cores, one for each bit. However this may not be practical because of the necessity of handling many individual costs and that of wiring them. So the first thing I tried was to make an integrated structure consisting in having a sheet of permalloy in which holes were added and windings were made by "printing" through the holes. So the very first idea was really a much more complicated idea the simple idea of an array of individual cores. However, the first experiment gave miserable results. The hysteresis loop was terrible.

MERTZ:

Not uniform?

RAJCHMAN:

Not uniform, and not square, and the whole thing was just miserable. So I said, "well, why don't we just buy some cores made out of this thin ribbon from Armco and, and then laboriously wire them and then see how it works." We found out, first of all that the memory worked very well, and secondly that it really wasn't so painful to wire the cores. Then I thought that one turn would be quite enough, and so therefore the wiring would be that much easier. While the premalloy cores made it possible to demonstrate the principle of the core memory, they were expensive, delicate and relatively _____. There had been a lot of work on ferrites that had been done in RCA Laboratories because of their use in television yokes. Also RCA had a factory for making yokes. I asked the ferrite researchers whether they could make a square loop ferrites and, _____ to my mind in a few months they had succeeded. So as soon as they made the ferrite material I thought that one of the most important things would be a machine that would speedily mold these cores in great quantities. I went to the Stokes Company in Philadelphia and they adapted one of the pharmaceutical machines for making aspirin tablets to actually mold the cores on "doughnuts."

MERTZ:

Mhm.

RAJCHMAN:

Approximately that time that I heard that MIT was working on a project similar to ours. I got in touch with MIT, though I don't remember at what date. Shortly thereafter the MIT group gave a paper at the IRE (1951) I met W.N. Papiian who gave the paper. We became very good friends and we visited each other many times, back and forth, to our mutual benefit. I _____ later a patent litigation between MIT and RCA on the core memory became a long and complicated affair. Our work on the core memory was hampered by the ongoing work on the Selectron. The Selectron was working at its best. We had already transferred, or were about to transfer, the work to the factory, but still we were making many tubes in the laboratory. Here was a bird in hand so to speak. The magnetic memory was unusual, to say the least, because nothing in electronics was really introducing magnetics into electronics. Moreover the idea that you could wire thousands of individual cores seemed fantastic. Even years afterwards, it seemed fantastic, and so that we and others always thought of some other integrated technology that would eliminate the necessity of handling myriads of individual wires. These were really psychological barriers. From hindsight it's surprising that we did not _____ immediately. Greater effort although _____ a year had working model of the core measure. Another fact is that we were a very small group. There was only Milton Rosenberg and myself, plus the help of Igor Grosdoff for the circuits. In addition there were student engineers; trainees that we had in the laboratory. Several of them were Scandinavians who stayed at the laboratories for a year under the auspices of the Scandinavian-American Foundation. Many of those student engineers were assigned to my group. example, Lars Person was one of them.

MERTZ:

That's P-e-r-?

RAJCHMAN:

P-e-r-s-o-n, Lars Person. And he was assigned just when we were making the first core memory. In fact, he built a lot of the circuits. And then there were later others. Stemme was another one.

MERTZ:

That's S-t-e

RAJCHMAN:

S-t-e-m-m-e. Eric Stemme. Another, much later was Arbaeus,

MERTZ:

Do you remember his first name?

RAJCHMAN:

Tore.

MERTZ:

Tore?

RAJCHMAN:

Yes.

MERTZ:

T-o-r-e

RAJCHMAN:

T-o-r-e

MERTZ:

That brings the story somewhat up to about 1949 or '50?

RAJCHMAN:

1950, '51.

MERTZ:

'50, '51. During this time--we're just about to run out of time and tape -- I just wondered...: were you involved in any other aspects of computer componentry other than the Selectron and the core memory?

RAJCHMAN:

No.

MERTZ:

That was your main area?

RAJCHMAN:

That's right. That was the main area, that's right.

MERTZ:

I think we have covered most of the people who were associated with you at this time.

RAJCHMAN:

That's right.

MERTZ:

Can you think of any others that we might have left out?

RAJCHMAN:

No. Well - there's a man by the name of Herckart, H-e-r-c-k-a-r-t. Paul Herckart, who is still here, who worked on the Selectron. He was in the shop, not strictly in the engineering research department, but he contributed a great deal in the actual making of the tube.

MERTZ:

There was also a fellow who went later I believe to the Rand Corporation who was interested in the –

RAJCHMAN:

Oh, that's right. Oh - I completely forgot. Brown, of course; George Brown,

MERTZ:

Mhm.

RAJCHMAN:

George W. Brown, and he was the early days of the Selectron.

MERTZ:

Was he a mathematician?

RAJCHMAN:

A mathematician, right. He worked with us in the early days of the Selectron. Then he left, went to the West Coast, and then later he had a connection with Ridenour. He and Ridenour eventually induced Milton Rosenberg to join them.

MERTZ:

Ha, ha.

RAJCHMAN:

Oh, another man who in the meantime had joined us was Stuart Williams, who in the later part of the program joined me when I had only Rosenberg. Rosenberg and Stuart Williams left RCA at the same time.

MERTZ:

Right. Well, this concludes the first portion of our interview. Thank you, Dr. Rajchman.

[End of Tape]