Computer Oral History Collection, 1969-1973, 1977

Interviewee: Milt Rosenberg
Interviewer: Robina Mapstone
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MAPSTONE:

The date is February 19 and this is Bobbi Mapstone. I'm talking to Mr. Milton Rosenberg. Are you a consultant on your own now, or are you...?

ROSENBERG:

Yes.

MAPSTONE:

Okay. In the computer field?

ROSENBERG:

Yes, computer and related high technology fields.

MAPSTONE:

Okay, fine. Well, now we've got it on tape. [tape stopped briefly] Okay. Maybe we should start with where you went to school and where you graduated from, leading up to work with RCA.

ROSENBERG:

All right. I went to undergraduate school at Drexel Institute in Philadelphia, and graduated in 1946. I had spent some time working part-time at the University of Pennsylvania on the [?] project, with the group developing the [?] for the Aberdeen Proving Grounds. This program started approximately in 1943 and was finished in early 1946. The program was headed jointly by [?] and [?]. I worked as a technician primarily, in that program, and graduated in 1946 from Drexel and went immediately to work at RCA laboratories in Princeton, New Jersey, in the summer of 1946. Back at RCA, we had a program for the average new employee, new technical employee, to be exposed to a wide variety of electronic technology: television, some early medical electronics work, work in transmitters; and one of the new fields there, of course, was the field of computing, both

analog and digital. But the early emphasis was on analog computing. So, during a course of this training program for one year, I got exposed to a number of different technologies in the electronics field, television and vacuum tubes.

MAPSTONE:

Radar?

ROSENBERG:

At that time, no. There wasn't very much work at RCA on radar. But primarily vacuum tubes and television, and some work on receivers--high frequency receivers--and some work on materials--basic materials work. The last group that I went to, of course, was with John Lichenor; and his background, of course, was in vacuum tubes specifically. In the immediate year or two before I came with him, vacuum tubes for storage. he was very much interested in storage, and very much interested in the computing field. He was the last group that I was with during the training program, and I stayed on with him--with his group--from the end of early 1947 through 1953. We worked on a number of programs, primarily in the area of storage for digital, digital storage. Initially we worked on vacuum tube approaches--for a period of about three years we worked on a vacuum tube approach called the Selectron.

MAPSTONE:

Were you very much involved with the Selectron?

ROSENBERG:

Yes, very much.

MAPSTONE:

Well, maybe we can get into some detail on the work you were doing on the tube itself, although I know it's [?] .

ROSENBERG:

Yes, I think that it will be covered probably in great detail by the representative on the East Coast, when he talks with Rajchman and other people at RCA. But I think [?] would be about the only individual in that system, in the RCA system, that still could talk about the Selectron in great detail. It was his basic concept, Rajchman's concept, the Selectron. And we worked on that for about three years, plus. We brought out a working model, and went into pre-production with the tube, which [was?] utilized by about two or three groups quite seriously. However, the basic concept of manufacturing a very complex tube which could only be manufactured, sole source, by RCA, and the other limitation of it was that it was quite expensive. I think that of course was a serious

drawback in the acceptance of the device. In addition to that, there were some concurrent developments, of course, which competed with it, namely, the Williams tube, developed by F.C. Williams at Manchester. This gave - of course, the Williams tube was more of a second or third-order effect, you know; it was based on a second or third-order effect of a CRT, some very special CRT, and most of the technique was done outside of that tube. So, it gave an opportunity for a number of circuits people to get busy immediately to develop a system, because the entry was only dependent upon getting some CRT's, and of course, the main emphasis was in the area of [?] circuitry, and the techniques outside of the tube, where[as?] our development [of?] the Selectron was heavily dependent upon the work done at RCA, and to a much lesser degree, the peripheral circuitry, which is greatly simplified, relative to the circuitry required for [the?] Williams tube. However, I think that history has taught us that you get a lot more effort b[y?] going in a given program if the entry is simplified, you see, where the emphasis is placed on circuit people rather than the emphasis placed upon some special device. I think that that was a very important lesson that I learned, and that I think RCA learned, too.

MAPSTONE:

As a company, this group that was working on the tubes, was this... It was part of RCA, not a separate little company by any chance?

ROSENBERG:

No.

MAPSTONE:

It was part of the RCA?

ROSENBERG:

Completely part of RCA.

MAPSTONE:

Was the development, well, were they behind you financially and emotionally?

ROSENBERG:

Well, I think that at the time, RCA, of course, had a major commitment to television. Television was just emerging, black and white television immediately following the Second World War, and the major emphasis was on color television at that time. This seems funny because black and white was just emerging, but a laboratory, of course is working five or ten years ahead. And the major emphasis was on color television at the laboratory. Most of the supervisors had had - had grown, you know, with television, and had worked on it a great deal of their professional career. So I think - and at this time,

incidentally, these people were middle-aged, so after spending fifteen or twenty years of their careers in television, I think that you can understand that this would be a large part of their thinking. Of course, their success was very strongly tied to the TV history. However, I will say that Dr. [?], who was a lab ahead, was very much interested in computing technology. He was very much interested in spending money investigating it, and I think due to him, in large part, more money is available, pushed in this direction. And so there was a small group initially headed by [?] which investigated devices, techniques, which were really an extension of what the laboratories had been doing. There was a heavy concentration of facility, know-how, in the area of vacuum tubes. So it would naturally follow that any approaches that these b[?] people would bring to the computing technology would be that of using their stress. So the group was very small initially, probably no more than half a dozen people, and it was kept at that level for the time that I was there at RCA--that seven-year period from 1946 to 1953.

MAPSTONE:

Who... Do you recall some of the other names of the people in the group?

ROSENBERG:

Yes. We had an excellent electrical engineer, who came from the Manchester group that worked with, I guess initially, the company in [?] that comes to mind is Metro-Vic, which was a fairly large electronics firm that had been in a wide spectrum of electrical and electronic equipment, development and manufacture, in the Manchester area. They worked in close association with the group headed by F.C. Williams and his assistant at the time, Tom [?]

MAPSTONE:

Oh, yeah.

ROSENBERG:

I guess he's still - Is he still at Manchester?

MAPSTONE:

I think [?] is dead.

ROSENBERG:

Oh, is that right?

MAPSTONE:

[?] was one of the people in the [?] at [?] . Oh, no, I'm thinking of someone else. That's

right [?] is probably still there.

ROSENBERG:

I think so.

MAPSTONE:

I've been out of - I don't know.

ROSENBERG:

I was awfully tempted to stop by to see him last fall when I was in Britain. No, I think that that group was very imaginative, and I think that they made an excellent--or a great contribution to computer technology. The fellow that I mentioned earlier, that came to RCA, was a fellow name of Raymond Stewart Williams, and he was primarily a circuit designer, a logic designer, and those were his two strengths. He had worked on radar, I guess during the war.

MAPSTONE:

Had a background then?

ROSENBERG:

Yes. On pulse techniques. Of course, a lot your early computer people came out of the radar field, because of the fact that it was pulse techniques, and it was a combination of pulse and continuous wave communications. So there was a wide spectrum of activity, but primarily the pulse technique was the major stress that they had to take from radar into the computer field. One of the other people was a fellow by the name of Laars [?], who was an exchange student from Norway; Eric [?] from Sweden. These were people that were exchange students from the American-Scandinavian Foundation. It's very difficult to recollect. It's twenty-five years almost now.

MAPSTONE:

They'll probably come to you as you're talking.

ROSENBERG:

It's kind of a block at this point, but I'll think of some of the others.

MAPSTONE:

Yes, they'll come as you start talking about them. Oh, you know, the Selectron tube was used in [?] out [?ht] here, right?

ROSENBERG:

Early on, it was used in [?]. As a matter of fact, I imagine it was used for several years, until of course they could get a better answer. The problem was that there was a serious limitation, that in the supply of the tubes; and, as I said early on, they were very expensive. And it was quite difficult for a circuit designer to understand some of the intricacies of the tube if, you know, some of the peculiarities that occur in a very complex device that is in its early phase of development. The average circuit designer, you see, has very little background, or had little background, in tube technology, and so in many ways he felt rather helpless under certain conditions. Certain of the peculiarities he would have to interface with the people that developed the tube. This was - I understand this, having been a circuit designer and gravitated into the components, and into the materials in the tube development field. So I felt many of the frustrations, or understood some of the frustrations that they had.

MAPSTONE:

So in a way you're saying that prior to the demise of the tube was, shall we say, lack of communication between the people who actually put the tube together and understood this rather slightly temperamental creature, and the logic designer himself, the circuit designer?

ROSENBERG:

That's right. And a further limitation, of course, was [of?] the tube was its resulting, of course, high price. Due to its complexity and, of course, to its very special nature--that it needed a lot of ongoing development and it needed continuous, I would say, it needed continuous interface with both development people, the manufacturing pilot line people who were making the tubes, and also the wide spectrum of highly sophisticated materials problem with that tube. The tube utilized secondary emission of small islets that were the storage elements. These storage elements were floated in a sea of mica, which was the insulator. Secondary emission as a phenomenon has always been a very unstable phenomenon, and that one aspect, of course, took hundreds and even thousands of hours of investigation and study. So I would say that without dismissing the tube, you see, by stating that it was the wrong idea, I think you can only do that academically because there were some other things that came along, and you never really can make a good argument that if you didn't]t stick with this, you stuck with it, it could have been a very interesting form of storage. It was, I think; a most remarkable development in its time. But I think there were other things that came along, first the Williams tube, which was another temporary answer.

MAPSTONE:

But temporary.

ROSENBERG:

That's a very temporary answer, then Core, of course. Core's simplicity and its low cost and its tremendous emphasis on circuits people and coming up with low cost designs and showing a lot of imagination, I think that combination was destined to succeed. Incidentally, there were a number of other answers for the storage problem, which took place during this period.

MAPSTONE:

Like the [?] .

ROSENBERG:

The [?] . There were the special tubes developed at the Lincoln--not Lincoln Lab, but MIT under J [?] . These were special tubes, special types of storage tubes. Once again, required manufacture, development and manufacture, of a special tube, and that program, of course, was set aside once again with core. Core was the obvious answer when it came along.

MAPSTONE:

Yes. I was just going to say I suspect what was happening around this time was the computers were being built and getting larger, and had obvious needs that it became apparent that tubes were not going to work, or if they did, there was so much to be done before they could reach a point of really being effective that the answer was to go somewhere else, which I suspect is exactly what happened, isn't it?

ROSENBERG:

That's correct. And I think that there was a tremendous lesson to be learned, and I can see this same lesson taking to be learned all over again the medical electronics field, which I have spent quite a bit of time in lately. The reason I say that is that the lesson was that when you have a wide variety of disciplines that have to be brought to a program, unless it's a major program, probably properly funded, it will ;not succeed. The fewer the disciplines required, usually the lower the cost, and the higher the probability of success. Once the Core came along and showed signs of, it showed, number one, the degree of non-linearity that was required for storage, the degree of no linearity that was required for switching, and away the circuits people went and picked up the ball and ran with it. You then had the [?] and some of the physicists who worked on making better C [?] you see. But the circuits people were entirely capable discipline-wise of coming up with configurations of array manufactured out of these. So the degree, the spectrum of disciplines required, was narrowed tremendously. And away these people went, you see, quickly, and of course the earliest devices showed signs of success. And you went from success quickly to the next success.

MAPSTONE:

Where I suspect the order of [?] and reliability jumped so considerably, too.

ROSENBERG:

That's right, because of course the basic Core, you know, is a ceramic. The history of earliest civilizations, ceramics of course play such an important part. So you can see what a stable device it is. So I think that that's the characteristic of C [?] that spelled the death of all of these other things, were[?] quickly discarded. And of course a lot of the people that were working on these other devices also immediately jumped into C [?] .

MAPSTONE:

Which is good that this was able to help them.

ROSENBERG:

That's right. That's right.

MAPSTONE:

Before we really go into C [?], O gave a couple of things... Oh, yeah, going, let's keep on the tube for a moment, RCA as a company, also didn't get terribly, weren't too involved, and weren't too concerned about the manufacture of the tube. Is this true about it? I know that this situation the [?] people had was not only were they expensive and therefore, one expects, would be high costs in reliability, but there [they?] were unreliable. And also that RCA wasn't about to back them up.

ROSENBERG:

Yes, I would say that that is a correct statement. You see, the early tubes were made in the laboratories. The laboratories didn't have sufficient facilities to make any significant number of tubes. The normal procedure for taking a device, a developed device, at the laboratory through to production, was to take it through a pilot run to right, fairly broad specifications for the device, and of course, the materials and the procedures. And then to take those procedures with some of the key department people, and then to take it either to Harrison, New Jersey, which is an RCA tube manufacturing facility for making small vacuum tubes, or, if they were large vacuum tubes, to Lancaster, Pennsylvania, where they made large CRT's and transmitter tubes and other tubes. You know, they had large vacuum systems, they had ways, and their lines were set up to deal with specific units, where Harrison had machines that were large rotary machines that were developed primarily for small vacuum tubes. These were for pumping and for most of your receiving tubes, you know, that were used in receivers and TV's. So we then had to take this vacuum tube to Lancaster, Pennsylvania. They had never seen a vacuum tube that even resembled this device. Pardon me a second.

[Recorder off]

MAPSTONE:

For instance?

ROSENBERG:

Yes. After we made, oh, several dozens of these tubes, which took up a great deal of the facility, of our development facility, we were forced to take them to a manufacturing facility with RCA. I think then that was when the ground rules dramatically change at RCA, because the laboratory, you see, developed the product, and their franchise is not directly tied to profit-loss. They develop products that they think will eventually serve the corporation as far as new sources of income. But there's no direct profit-loss responsibility within the laboratories. Now when you take a device out to a manufacturing facility such as Lancaster, such as Camden-RCA, such as Harrison, New Jersey RCA, there is someone out there who is directly responsible for profit and loss in that division. They ask such embarrassing questions as, "How many of these will be used?" and, "What was the cost of the last hundred of these that you made?" And also such questions as, "How did these last hundred works in the system?" Of course, our statement was that we think that there's a very large market which we can't codify, and you know, all the good things that we believe, which are religious almost in nature. I don't think that that sort of response, after several years, was well received. You have to also understand that these people were heavily burdened at this time with expanding in the area of CRT's for television receivers, the transmitter vacuum business was excellent at that time because a number of people were putting in TV transmitters, and AM transmitters, and there were also a lot of other special tubes, which these people had a good market for at that time, all kinds of tubes which were highly sophisticated, such as the Image-Ortgcon[?], you know, which even at that time sold for $1,200. So, I think that in all fairness - I think that the assessments that were made were less than optimistic. And therefore, when someone has to make a decision as to where best to put the monies, I think that in all fairness to them, I think that they made what they considered to be the most prudent decision.

MAPSTONE:

It kind of makes sense. Okay, how about patents? Were all the patents Rajchman's, or were you involved in patenting the Selectron tubes?

ROSENBERG:

A number of them were Rajchman's, and a number of them were mine, you see. Oh, I imagine there were something in the order of about fifteen patents while I was on the Selectron, because of the fact that it had a great many sophisticated both storage and

switching, storage of course using secondary emission, and the collector relationship, collecting the secondary electrons, also the switching of the beam which was done in a very clever combinatorial selection of a number of X and Y bars. It was, I think, very, very sophisticated device. Probably of all the storage devices developed at that time, it's my opinion that it was by far the most sophisticated device. Therefore the longest to develop.

MAPSTONE:

Yes, and then technology overtook it. On the patents, do you have a list of the patent numbers by any chance?

ROSENBERG:

Not at this time. I think that that information is available from Rajchman

MAPSTONE:

Okay. Then it's also, the other thing would be, the patents would be in either or both your names?

ROSENBERG:

That's right. There are some joint patents, and there are a number of patents in Rajchman's name, and in my name.

MAPSTONE:

Okay. The reason I'm asking is, I have to go to the library, so I'll probably do an index search . Would you say the dates would be...

ROSENBERG:

The dates would be 1948 through 1951. Incidentally, I think that if you want to avoid this exercise, I think that Rajchman or the RCA patent department have this information all catalogued and readily available. I think you can avoid that exercise.

MAPSTONE:

I have to go on those kinds of exercises. (Laughter) Okay, so when did the work actually stop now on the Selectron tube in the group?

ROSENBERG:

It started in about late 1947--late 1947 is when it started.

MAPSTONE:

Stopped?

ROSENBERG:

And really I guess early or late 1951 or early '52, that's my best recollection.

MAPSTONE:

And then what happened? Did you move into the Core area, or what happened next?

ROSENBERG:

Yes, we moved into Core in 1951, very active by sort of latish '51. Of course, at the time I left in 1953, we were still very active and the group had grown. My reason for leaving RCA, very interestingly enough, was that I didn't feel that RCA was going to exploit computer technology, and therefore left because I wanted to be very active in it. I thought that it was not the place for me. Several other people in the group thought that too, and Raymond Stuart Williams and myself left in 1953, and came out here to the West Coast, specifically to work under Dr. George Brown at International [?]. The head of that group was Dr. Louis Ridenaur, and Dr. Brown worked for him.

MAPSTONE:

Dr. Brown worked for Ridenauer, or Ridenauer worked for Dr. Brown?

ROSENBERG:

Dr. Brown worked for Ridenauer. Dr. Ridenauer headed that group. Incidentally, have you talked to Dr. Brown? Have you met with him?

MAPSTONE:

I hope to see him on my next trip to San Francisco. He's up there to a year.

ROSENBERG:

At Stanford now?

MAPSTONE:

Right.

ROSENBERG:

Yes, he's on a sabbatical. That's right. Have you spoken to William Gunning?

MAPSTONE:

Yes; I spoke to Gunning.

ROSENBERG:

You have. Well, he's very active during this period also, and he later did work for Dr. Brown, too.

MAPSTONE:

That's right. He was at [?] and had some unfortunate problems with security.

ROSENBERG:

Security, yes.

MAPSTONE:

It was kind of messy.

ROSENBERG:

Well, so did I.

MAPSTONE:

So did you?

ROSENBERG:

But I preceded him in that experience.

MAPSTONE:

What a terrible period!

ROSENBERG:

Yes, it was. This was the McCarthy era, and it was a bad period. Of course, most people did recover their clearance, including myself. However, it left an indelible mark on me, and I know that it did on Bill Gunning.

MAPSTONE:

Yes, well, Bill--I don't know when you got yours back, but Bill got his back only very recently. He said the incredible thing was that he felt, he just felt, that after all these years there wouldn't be a hassle. But even knowing what had happened, you know, in the McCarthy era, the problems; he still had to really fight to get it back. It was a very unpleasant deal.

ROSENBERG:

Yes.

MAPSTONE:

I suppose it must be the same for everybody. You don't get it handed back to you very easily, even considering what happened to McCarthy, and that's that, perhaps.

ROSENBERG:

That's right. But it was, you know, there were a number of articles that had been written, and I think a book that had been written, on this period. I think it was a very unfortunate period in American history.

MAPSTONE:

Um, terrible.

ROSENBERG:

I think that it'll rank, well...

MAPSTONE:

Well. (Laughter)

ROSENBERG:

Well, on a scale of ten, it may be one, relative for ten for the Vietnam War. But it was a very unfortunate period.

MAPSTONE:

Really bad. But this little... Who started International [?] ?

ROSENBERG:

Well, International Telemeter was primarily started with the idea of exploiting pay TV.

MAPSTONE:

Oh, this is the Paramount.

ROSENBERG:

Yes, Paramount. Well the individual who started it was a fellow by the name of Carl Lesserman. Carl Lesserman[y?]: he had been an old motion picture producer, and had produced a number of, I guess I choose to say highly-successful-financially motion picture[s?] with very few artistic successes, and had made several fortunes in the motion picture industry prior to recognizing the opportunity of pay TV. Pay TV, he sought out Dr. Louis Ridenauer to handle the technical aspects of the pay TV problem. Dr. Ridenauer's first love, of course, having come out of the radar at the radiation laboratory at MIT, he was one of the assistant heads of the radiation laboratory. He was a very young man then, and anyhow had been very much aware of the work that they'd done on pulse techniques, because he was the editor-in-chief of the MIT radiation series.

MAPSTONE:

Right. Which was even today stated as a [?]

ROSENBERG:

Which is a treatise, yes.

MAPSTONE:

... great historical document.

ROSENBERG:

Right. No question. But it's a technical series. It's just a complete technical series on what took place at that time at the MIT radiation laboratory. He was very much interested in this emerging field, called electronic-cable processing. So what happened was, he either by subterfuge or no matter what you want to call it--in the back of his mind I don't think he had nearly as much interest in pay TV as he did in this field called electronic data processing. So he made an issue of the fact that they needed a group to work on the handling of the records, the keeping of the records, of who bought what program, you see, for pay TV, and that this was a major problem, and also the problem of doing the electronic bookkeeping digitally of not only who had what program, but of what percentage went to the distributor, what percentage went to the producers, and so forth, that it was a horrendous problem in electronic data processing. That was the justification for starting initially a core group and of course eventually a computer group. But he saw that there could be initially there some economic success of a core group doing just that peripheral portion of the total electronic data processing problem.

MAPSTONE:

Let's turn this over before we...

[End of Side 1]

MAPSTONE:

So we were talking about the reasons for setting up International Telemeter and getting into the Core aspect of it.

ROSENBERG:

Right, right. Well, anyhow, the history of that period was that Paramount was, their primary interest in International Telemeter was to find a new large box-office, of course, for their distribution of film, and it really excited them because they saw the inroads that TV was making into their theater box-office receipts, and they thought that if they could go on and get onto the bandwagon of TV and exploit it in the form of pay TV, that this would be a tremendous bonanza for them. Louis Ridenauer saw this that was not his first interest, of course, his first interest was to try to exploit computer technology. He used that then as a vehicle to accomplish this. That's how the group got started. It was initially a small group, and as it grew and got acceptance, it became more and more obvious that that group would become an economic reality before pay TV would.

MAPSTONE:

I'll say!

ROSENBERG:

And this happened very quickly. So, if you like, it was the tail wagging the dog. So I think that the early work done at International Telemeter, that was completely subsidized by Paramount, got the group one product and about half a dozen installations, on being the [?] installation, another at Aberdeen Proving Grounds, one at the Weitzman Institute in Israel, and one at what is known as Cape Kennedy in Florida, on a data reduction computer down there called - I think it was called FLAC

MAPSTONE:

They named it.

ROSENBERG:

Yes. And the one at the nuclear laboratories just outside Chicago, called...

MAPSTONE:

Is that Argon?

ROSENBERG:

Argon National Laboratories. Yes. There were about five or six installations, and very successful installations. The group didn't make any money on this, but of course they got lots of interest was generated, and I think that it was quite obvious to all that the successful company could be built out of exploiting this technology, out of investing in new designs and following this with a spectrum of products following this initial development. Now the development was a very small computer, I mean a very small computer membrane. It was in the order of four k-words of thirty-six or forty bits, and the total of like a hundred and sixty kilobits of storage. Today there are desk calculators that have a hundred and fifty kilobits of storage. But this was made out of vacuum tubes, and it was quite a large installation. You've probably seen the one down at, was it Rand? Right. Did you ever see this document?

MAPSTONE:

Yes, I ;have. The [?] ?

ROSENBERG:

Yes.

MAPSTONE:

Right.

ROSENBERG:

It gives a lot of the history.

MAPSTONE:

It's a very good document. This, along with the Grueunberger Chariok history...

ROSENBERG:

Right.

MAPSTONE:

... really gives an excellent history of the machine and the people and the heartaches and the fun. That's a very good one. You know, I'm interested in the preliminary core work,

it was done at MIT, right?

ROSENBERG:

Well, MIT and RCA and also IBM.

MAPSTONE:

IBM, right. What I was curious about, though: did the MIT people, who I think started it, get a patent on it, and then did the people who then went on and did further work in it--yourselves, RCA, IBM, etcetera--have to go back to the original patenting and work through them, or, once the idea became known was it sort of up for grabs, so to speak?

ROSENBERG:

Yes. Well, let me just explain what happened. MIT was working on the Sage program, and the Sage program had a requirement for large computer memory. MIT, under contract to the government, an MIT group worked on a variety of storage devices, including vacuum tubes, and the[y?] looked I think, at mercury delay lines. They looked at a number of things. (Tape stopped briefly for telephone and then resumed.) A group under Jay Forester at MIT looked at a wide variety of different devices that could solve the storage problem. That was their franchise, okay? Jay Forester. My understanding of the chronology was that Jay Forester filed early on a patent on an approach to utilization of a non-line magnetic core in an array for accomplishing both the storage and the switching problem. The concept was the coincident[?] card, the magnetic core array. He sketched out the method of selection and sketched out a method of sensing, and he sketched out part of this was the way in which you would write in, and the way in which you would interrogate, the way in which you would sense the information. He had a very large group working with him of people who were not only hard-working, but very inventive. So in my own mind, from my vantage point--you know we had a very small group at RCA and we had a great deal of interface with them--I was tremendously impressed with this group of young graduate students; and some of them had just recently graduated. In my own mind, it's very hard to distinguish whether or not this total invention was put down on paper by Jay Forester, or whether or not it was an exchange. But from the standpoint of patent law, the inventor or inventors are the only ones that can be on the patent. They must be on the patent. If there are people who invented this device, and are not on the patent, of course that is, I think, a very bad situation. Anyhow, as I say, I have no hard and fast proof that Forester wasn't the original inventor. However, he was not a very impressive man in that system. When you went there and met the group, there were so many young tigers running around... True, he was the professor who headed the group. But there were so many youngsters, extremely diligent, and with nimble minds, who, I think, clouded this issue as to who was the original inventor. However, I have over the years remained very close friends with a fellow you may want to interview along the way, and in my mind is most closely identified with coincident current for memory systems in that whole group at MIT, and his name is William Papian. My last knowledge of him was at Washington University in St. Louis,

and that is about two to three years old. Papian spelled P-A-P-I-A-N. He wrote the first paper and gave it at a convention on coincident current memory systems.

MAPSTONE:

That was at the one in Europe

ROSENBERG:

Yes.

MAPSTONE:

What department of the university is he in?

ROSENBERG:

He is in, I guess, the instrumentation laboratory at Washington University, and he does teach also, I believe. Now that's my last knowledge of him. He may not still be there.

MAPSTONE:

Okay. If someone was going to see him, they could go there to try and contact him.

ROSENBERG:

Yes.

MAPSTONE:

Okay. So really what I'm trying to get clear is, it was open field for anybody who wanted to take the early work and improve on it. This is what happened. This what...

ROSENBERG:

Right. There were a number of groups, as I say, that existed at this time. One the [?] was an active group at IBM, there was a very active group at MIT working in both materials and working in systems. There was a small group at RCA which saw the opportunity in this area. It became obvious to all and sundry, those skilled in the field, that this was a very significant development. Its simplicity overwhelmed you, you know.

MAPSTONE:

Yes. It's like you look at it, and you say, "Well, why didn't we think of it before?" you know. Because it's been sitting there for all this time.

ROSENBERG:

That's right. It's like the Mazda engine. Rajchman often gives the example that if you were to have someone land, an intelligent human being land on this planet from outer space, and you tried to explain to him how the internal combustion engine worked he just would not believe you, that you went to all that trouble to take rectilinear motion and convert it to rotary motion. But it's the history of the way most rectilinear, most of your motions, were developed: from rectilinear to rotating. People just thought that way. Because the steam engine, of course, the early steam engines, was a conversion of rectilinear motion to rotary motion. You say, "If you want to go around, why don't you start with a device that does go round in the first place?" Because the problems associated with the mechanics of taking a piston and first compressing and then sending the piston down, and then having it stop, and exhausting it, and then coming up, and of course taking in both gas and air, and then compressing it. It seems so complex and such a ridiculous way to do it, it just doesn't make any sense. You show a little Mazda--not a Mazda, a Wankel engine or show a turbine, you know, it's so much simpler; but of course it took so much longer to develop, for a myriad of reasons.

MAPSTONE:

The analogy with the Core is the same.

ROSENBERG:

It's very similar, because it was so simplistic. You know, five to six years before Core was developed, in discussions that I've had with not only Rajchman but with Eckerman Eckert, Eckert and Mauchly at the University of Pennsylvania, we could put down on paper a series of specifications for what we needed in the way of a storage element, its characteristics, and we put down things like, "We want it to be non-linear, we want it to be stable, we want it to have at least two stable states of storage. We want it to--when I said 'stable' before I meant stable relative to one state or the other. But we'd like it to be stable with time. In other words, we'd like this thing to last and be reliable. We'd put all those specifications down and we'd go through and we'd look for devices with [which?] exhibited these characteristics, you know, in a variety of different disciplines. You know, whether it be vacuum tubes, whether it be devices such as mercury delay lines. These were all approaches to the solution of the problem. As soon as the Core hit, then it's very interesting how quickly everyone jumped on the bandwagon. But, you know, invention is funny in this regard, and that is that the invention of a device, the government has a legalized monopoly called the patent, and the whole concept of patents is really archaic, because early on, you gave someone a legalized monopoly called the patent because he, beyond the call of duty and on his own initiative, developed a device for a system that would better mankind, or would specifically better the citizens of the U.S. Because he put in all this additional initiative, you said, "I will take and bestow upon him this tremendous advantage called a legalized monopoly for him to exploit it." Okay? Now that may have been true two hundred years ago, but in recent years large corporations spend millions of dollars for people to work full time on developing devices and systems.

So now that original concept--and incidentally, these are in large measure paid for, at least fifty percent of it is paid for, by tax dollars--so that the legalized monopoly concept doesn't really make any sense. It's really quite archaic. But you won't find many people that will take that position.

MAPSTONE:

I'm sure. There's too many issues involved, and one of them is you're talking about millions of dollars being spent on research and development. There are also the millions of dollars that have been spent on the legal suits that constantly go on over patent issues.

ROSENBERG:

That's right. Now backing up here, there were, as I say, at least three active groups, and a number of lesser active groups at the time that Forester and the group up at MIT were working. The group at IBM[?] were working, the group at RCA were working, on Core techniques. Any one of those groups could have come up--or would have come up--with the answer to the problems, given this first glimpse of material that was developed by Indiana General, or at that time was called General Ceramics. The head of research at General Ceramics was a ceramist who had developed some material for microwave applications, and he had printed a B.H. loop of this material in a magazine. Bill Papian and a number of other people had seen this B.H. loop, and said, "Hey, this may be a device that we can utilize in coincident current storage." Prior to that, you see, we were investigating perm alloys. These posed a major problem because they were very costly, very difficult to produce uniformly, and they were quite large. But the first memory systems, first coincident memory systems, were made out of perm alloys.

MAPSTONE:

They were quite large, too.

ROSENBERG:

Yes, they were.

MAPSTONE:

I've seen a Hern route, what was it? Well, there was a 16 x 16, but I saw a much smaller one.

ROSENBERG:

And it was very large.

MAPSTONE:

It was VERY large.

ROSENBERG:

In the same physical space you'd put like 16,000 Cores, in that same space that were four by four.

MAPSTONE:

That's right.

ROSENBERG:

Same volume, yeah.

MAPSTONE:

Now the other, I think, interesting comment is that--which ties back into the patent--is that because there were so many people into research and development, everybody was discovering the same thing at approximately the same time.

ROSENBERG:

That's the point that I was making, yes. Because of the diligence and, you know, no one has a quantum intelligence[?], and no one has a corner on facilities. So there are so many facilities now, with intelligent people working on similar problems. And the communications and that exist today, and what's going on, is such that everyone is on the threshold of coming up with the same ideas. So that's the point that I made relative to how archaic our patent system was. See, now one fellow may emerge by a microsecond of having written this down and gotten it into the patent office, and there may be an argument for many, many years by expensive lawyers on who was the first inventor, which is a very ambiguous thing anyhow.

MAPSTONE:

Yes, and the money can be utilized in so much more important ways.

ROSENBERG:

That's right. Relative to the - I didn't really answer your questions specifically; I just wanted to make this point: Forester did personally, you see, have significant financial gains out of this. But you see, it was done on government funds mainly. But the government has a working relationship, or contractual relationship, with people who work on these programs, whereby they will take--these are contractors--they will take out a patent, and the patent will belong to whoever the contractor is. At that time, MIT owned certain commercial rights to the patent. The government had royalty-free license

to this patent, and a certain percentage of whatever this patent yielded in the way of financial return would go to the inventor. So MIT, at my last rough assessment, MIT has gotten about twenty to twenty-two million dollars return. Now there was quite a bit of money spent on litigation to finally collect these royalties, but I think in large measure the net at MIT was close to twenty million dollars. I think--I don't know, I think--that Forester got 7% of that number. So, you know, it was upwards of a million dollars.

MAPSTONE:

Okay. But somehow or other this didn't stop the work proceeding at [?] and various organizations. What I was very interested in, and take it [?], did they pursue many avenues that other companies weren't? In other words, was their research different from what RCA was doing, or was everybody sort of following the same line?

ROSENBERG:

And just doing them when they come to them [Who said this?]

ROSENBERG:

Well. I think that the earliest exploitation by International Telemeter was in the area of small memory systems. Sage, th[?] its requirements were for large, and this was the work done at MIT. It was for a very large, high-speed attached to a large computer system. Work at International Telemeter was channeled into, initially, small memory systems done for a number of computer manufacturers, such as Remington Rand, such as UNIVAC, such as RCA later on. Of course, practically everyone, Honeywell, practically every one of the computer manufacturers, both complete memory systems and the stacks of Cores. [this sentence needs a verb] That was the direction that that group took. These were for buffering and for switching applications. They had an immediate market develop quickly because, I think, for the main memory systems, most of these people like UNIVAC, like RCA, wanted to retain that under their own control. Of course, that changed later on because this group at International Telemeter, which became Ampex, and then other groups that [?] from there, they applied a wide spectrum of product, even including the main systems, you see, later on. But the early product that was developed by International Telemeter after the work done on the original design, that I told you, for Rand and the other --the opportunities there were for independents who were making their own computers who needed a memory. Those people, there was only a handful of those people, because after that the computer manufacturers and they were the primary opportunity for significant commercial opportunities. The groups, soon, the groups at Illinois, and this was a group at [?] , University of Illinois, a very active computer group in the late 1950's and 1960's. The special computer groups soon died off, you see, because the government didn't choose to fund them if there were other available sources, particularly.

MAPSTONE:

Well, weren't some of the giants on their way to having UNIVAC systems and IBM Seven-O series?

ROSENBERG:

Sure. Right. Because you can't really justify a group of people re-inventing the wheel or something. So those other people became the opportunities, the commercial opportunities, for peripheral manufacture like International Telemeter. Of course, that's where the effort was directed. The needs of those people were of course specific types of core memory other than the main memory. So there was a lot work done early on through buffers. They're called buffer numbers.

MAPSTONE:

When did Ampex pick up International Telemeter? At what time

ROSENBERG:

Ampex acquired International Telemeter in about 1960.

MAPSTONE:

Were you still with the company at this time?

ROSENBERG:

Yes, I was.

MAPSTONE:

And then you went to Ampex?

ROSENBERG:

We were acquired by Ampex, International Telemeter, and I stayed on for about seven months after the marriage. It was not a happy marriage.

MAPSTONE:

It seems like so many of these aren't. What happened To George Brown? Did he... ?

ROSENBERG:

Well, George had left the International Telemeter situation prior to that time. He had left a number of years prior. And I think that when you get with him, he'll give you the chronology, but he was not in the system called Telemeter Magnetics. Let me give you

the chronology of what happened. A new company was formed out of International Telemeter in approximately 1956. This new company was 80% owned by Paramount, and 20% owned by the principals of this company. The name Telemeter Magnetics. When it became obvious that there was a commercial opportunity for this activity that was within the International Telemeter structure, this group working on Core memory systems, Paramount decided to spin it off as a separate entity. As I say, they retained the 80%, and their contribution to this new company was some facility all of the patents and technology that had been represented by this group. The principals who obtained 20% put in hard cash[?] into this picture for that 20%. It was still a very nominal value. The company was incorporated for something on the order of several hundred thousand dollars. As I say, the name of that company was Telemeter Magnetics, separate facilities, separate books, completely separate management except for the Board of Directors, which Paramount retained of course, 80% of the stock, and they retained a corresponding number of directors relative to their ownership. So the group that we've been calling International Telemeter all along was really a group spun out of International Telemeter called Telemeter Magnetics. The group acquired by Ampex in 1960 was a group called Telemeter Magnetics. The franchise of that group, Telemeter Magnetics, was 100% met in the field of Core memory systems.

MAPSTONE:

Just to get back, when International Telemeter was set up, now, approximately when?

ROSENBERG:

International Telemeter was set up about 1951 or '52. But the Core activity didn't start within that group until 1953, late in '53. Stuart Williams and myself came out here, interestingly enough, almost twenty years ago. We arrived here April, '53. That's almost twenty years ago. We came out from the East Coast at that time, and then that was the group set up when he and I came down there to set up the group whose primary franchise was to work on core storage.

MAPSTONE:

Bringing your knowledge from the East.

ROSENBERG:

That's right, from the East.

MAPSTONE:

Okay. And how about some other key names, for instance, do you remember who some of the principals were at the time Telemeter Magnetics was created?

ROSENBERG:

Yes. Initially, as I say, Ray Stuart Williams and myself. And then he brought someone, an old friend of his, from Britain, by the name of Andy Alexander, who's still out here on the West Coast, and a fellow by the name of David Bird, from [?] . That's nothing. Ray Stuart Williams was originally with a company called Metrovics. It's Metropolitan Vickers is the name as you know it. But I guess I used the contraction, Metro-Vic. Metropolitan Vickers. The group - I think that there was a merger of activities between the computer activities of Metropolitan Vickers and Parante. That was a computer group that these people came out of, Ray Stuart Williams and David Bird and Andy Alexander, and later on there was Telmacher[?] and Telmach[?] came in with... As a matter of fact, he came in initially as a marketing director[y], and then became president of the company called Telemeter Magnetics.

MAPSTONE:

There's so many things I want to talk to you about. Okay, again back to the patent subject: Were patents taken out acting on various aspects of the Core manufacture?

ROSENBERG:

Yes, there were a number of them that now, of course, belong to Ampex. These were more--rather than basic patents--these were more refinement patents. Of course, there are myriads of these.

MAPSTONE:

Yes. Right. I was just curious. Do you think there were any that were really significant?

ROSENBERG:

That's hard to say. Significant in the usual sense means commercially significant.

MAPSTONE:

Right.

ROSENBERG:

Of course, there are very few patents that were as commercially significant as the Forester patent, because of the magnitude of that money. You see, MIT has a pool of patents which, I'm sure, run up into the thousands. I think that there are very few of them that have ever made the return of the Forester patent.

MAPSTONE:

I think by significant I was thinking more as significant to the technique development of

the technology.

ROSENBERG:

The patents very seldom ever do, ever play that role. For instance, if you read a patent, it's very difficult. The patent's supposed to be a teacher.

MAPSTONE:

That's right! Yes.

ROSENBERG:

But there's very few things you can ever make from reading a patent, because it's what's unsaid that allow you--really is the heart of how to assemble or manufacture something. Because a patent really claims broad things, it doesn't specifically call them. The things that are unsaid, the techniques that are unsaid or unspecified, are important to the manufacture. I think of course the Wankel patent may be a contradiction here, because, you know, there aren't too many things that are conceptually different, such as the Polaroid camera, the Xerox [?], the [?] . But in this case...

MAPSTONE:

And in this case the original core patent.

ROSENBERG:

The original core patent. I don't think there were any real basic or economically successful things other than that.

[End of Interview]