INTERVIEWEE: Clarence A. Lovell

INTERVIEWER: Richard R. Mertz

DATE: 19 March 1970

LOVELL:

I don't speak very loud and if I try to speak very much louder than normal I will have laryngitis. Well, some of the rather non-technical part of this story you will probably remember. That is, having skipped high school and done some grade teaching in the South. I went to Mississippi College as a student in Army training. And at the end of the war, I just had one term as a freshman there. And I was allowed to go back to school on the condition that if I did be a veteran in the rest of the freshman year, I would be allowed to stand for a degree and all conditions removed from my entrance requirements.

MERTZ:

You mentioned two gentlemen at that point in your life who did have an influence. One was the Superintendent of Schools, I believe, and the other was Professor Hitt?

LOVELL:

The Superintendent of Schools in Lincoln County, Mississippi was Ronny Gars [?], who was quite an athlete, local county athlete, and also a schoolteacher before he became superintendent. Professor J. R. Hitt who was professor of mathematics at Mississippi College and I guess he died only a very short time ago; he lived a long time. But he helped me make up my deficiencies just by going around to his house and letting him answer questions. I read, I studied on the campus grounds. Just lying around on the campus trying to learn some math. By the way, the Uncle's name was John May. His only connection was that his wife was my mother's sister, so you know a lot of (all of them had a large family in those days, I had a lot of first cousins still do have. Most of them are in Mississippi.

MERTZ:

And it was the library that he inherited from his father that you--that formed your early basis of your reading?

LOVELL:

Right.

MERTZ:

And then you did take this Teachers Qualification test with your aunt's?

LOVELL:

Yes, and taught school two or three years. And then the War came on. I'd been teaching three years and I'd been learning quite a lot too. You learn a lot more teaching then you do studying. Because you're got to stay ahead of the students, they're all, even though they don't know much, they're all intelligent. And if they find a little hole in your background, they can embarrass you very fast.

MERTZ:

Yes, well in fact you did learn perhaps a lot of your high school work when you were teaching grade school and a lot of your college work when you were teaching college.

LOVELL:

That's right. I did teach in Mississippi College, because there simply weren't any qualified teachers there. I just taught freshman work when I was a sophomore, and sophomore, and freshman work and sophomore work when I was a junior, and freshman, sophomore and junior work when I was a senior.

MERTZ:

And then I believe you stayed on for a couple of years after that and taught

LOVELL:

That's right.

MERTZ:

probably senior math as well?

LOVELL:

My mother had lived there; she had a large family and my father had died. And all of the [?] she just simply lived there and kept the whole family in school.

MERTZ:

Did you meet your wife when you were in college?

LOVELL:

No, when I was teaching in school, she lived down in Mississippi County. I met her before I went to college and married her while I was in college. By this time, I had left Clinton, Mississippi where the

college is, we had one child, and that's all we have. So, and then I applied to the University of Pennsylvania.

MERTZ:

You then applied to the University of Pennsylvania where you acquired a job at Drexel?

LOVELL:

Actually, I'm certain that letter went to every college in the American College Book, you know, I don't know what you call it, some kind of a--

MERTZ:

Did they have graduate school in Math?

LOVELL:

I had written every college that had more than two thousand students assuming that they had graduate school there. I actually went to Drexel Institute and they needed a math teacher, but they didn't have any graduate school and they simply said that I could go to school at the University of Pennsylvania which was three blocks away. And I did go there, what I actually did, I got a job at Drexel Institute and then entered the graduate school at the University of Pennsylvania. I stayed there at the Drexel Institute for four years. I got a master's degree in Math at the University and finished the residence requirements for a doctoral degree and started on a thesis.

MERTZ:

And did you write a master's thesis?

LOVELL:

No, I didn't.

MERTZ:

Oh, I see. And was this under Professor--

LOVELL:

I'm not really sure.

MERTZ:

Your doctoral work was under the man who studied with Cartan in France?

LOVELL:

Yes. Joseph Miller Thomas who now is at Duke University, a professor at Duke University. But actually, what happened was that on the thesis work, I had to give up the work under Miller Thomas because it was too hard to do with him in France and me in New York City and the college being in Philadelphia. So, I went to the--.

MERTZ:

Oh, you had moved by that time to New York?

LOVELL:

I'd taken a job in New York and I was going to take [?] but he'd left the University. Then I decided that I wasn't getting any additional advantage in being in Philadelphia. So, I took a job with the Bell Telephone Laboratories in 1925.

MERTZ:

Now one question I wanted to ask you. The work you were doing in research was in affine metric geometries?

LOVELL:

Yes. I have a copy of the thesis which I happen to have because the printer printed it without Clarence "A" Lovell on it and so they had to reprint it. So I have plenty of copies. It happens to be very--I probably will tell you a little about that later. The effect of Drexel Institute had on my education was greater than that of the University of Pennsylvania. Because while I was going ahead and getting a Master’s degree in Math and a Doctor's degree in Math, I got much more interested in engineering than I did in mathematics. All my education is really liberal arts education.

MERTZ:

Were there some particular things in your experience at Drexel that intrigued you, as some kind of specific problems in engineering?

LOVELL:

Not really. What happened was that there was a man at RCA--which was then Victor Talking Machine Co. in Camden, N.J., who was a German engineer by the name of Rudolph Malina. He came to the Drexel Institute and asked for a tutor in mathematics because he was a German textile engineer who had

taken a job at Victor Talking Machine Co. to design a talking speaker box, a speech box that translates from the needle vibrations to speech. And he needed some work in filters. And he wanted a mathematician to teach him how to do this, so he could learn a little mathematics and understand the f filter design. But what happened was that we started to learn the filter design together. I was a mathematician but it takes a lot more than mathematics to understand filter design. You've got to know some physics too and some electrical theory and a few things besides. So, I started out just working with him to teach him filter design and I learned it myself while I was teaching him. He later laft the Victor Talking Machine Company and joined the Bell Telephone Labs. And one day, instead of coming to work, he telephoned me and said he guess he wouldn't be there anymore, and he'd taken a new job and wondered why I didn't come up and apply for a job too. That was shortly after Joseph Miller Thomas left the University and I'd left Drexel. And then I had to write a thesis in abstract geometry. And I went back to the University and asked how to get a new thesis director--just like you do in the new math, you've got to get rid of the first one first, then apply for a second director. How would you do that? And he said, "Well, submit a thesis and have it rejected." Well, it turned out that Miller Thomas had a theorem on affine geometry which, in which there were some conditions on the affine geometry parameters that would make the affine geometry a metric geometry. And these conditions were expressed in terms of the unknowns as well as the known quantities. And Miller Thomas thought he had a way of expressing them in terms of the unknowns alone, that is, the conditions beyond the unknowns that would make an affine geometry a metric. It turned out that the theorem that is, his method didn't work and that he was going to express the conditions on the Ricci tensor. There is a metric tensor from which you can derive from several other tensors and then from that derive a Ricci tensor. And the Ricci tensors results was able to express all the conditions on the components of the basic tensor [?] that would make it a metric tensor. It turns out that it wasn't so; that the conditions he felt would do were identically satisfied, so that there was nothing that was imposed on the metric tensor on the fundamental [?], which would have been the metric tensor if it were a metric geometry. Well, that left out of the things that Miller Thomas had suggested, an interesting little problem which he thought would be a sideline. And my thesis professor said, if you'd just forget everything else that he had in this problem and look at that one little section, I think there is a basis for a thesis. And then what

I did was submit a bunch of junk which would amount to a thesis; and they rejected it. And I applied to Dr. Beal at the University of Pennsylvania. He was also Miller Thomas' thesis professor when he got his thesis. And Beal told me that if I'd forget everything but one thing that there was enough in there to [?] it. I had already solved the problem which was the basis of the thesis. But it sort of [?] in the case of Miller Thomas. Miller Thomas, he didn't want to fool with anything unless it made a real big slash in geometry. And the minute that he found out that his problem didn't work out, he just didn't want to start over. And he didn't want to start on a small and [?] to his problem and to the basis of the thesis. But it did turn out that it was acceptable to the University and to Dr. Beal. This is a copy of my thesis and it doesn't make a big splash in metric geometries or affine geometries, but it defines a bit of, I call them changes in surfaces. Where you start out with a metric tensor gij and I don't know if you know any this is GIJ [?] this is a double index. i goes from 1 to n and j goes from 1 to n; so it's a metric of a tensor. And you derive a bunch of other tensors and finally by striking the other tensors, you come down to get a Ricci tensor which is also two-dimensional. Well, I said, will suppose this is regarded on a metric of a new space, and then I'd repeat the thing, and come down and get another Ricci tensor or that Ricci tensor. Will I ever come back to the original tensor? And the answer is yes. And there is a set of differential equations that you have to satisfy if you do. And I solved the differential equations in two-space, two space that's really three-space [?] Cartez's corner which is the only one that really exists. And there's a little bit of it, a nice little circle, ring of metric spaces. And this is, I think, it isn't all that important, though I found several things similar to it later on pro-condition. They call them rings, I call them chains.

MERTZ:

Yes.

LOVELL:

And the chains, then if they are closed on themselves, they become rings.

MERTZ:

Right.

CAL:

Actually, when I went to the Bell Laboratories I thought I would find some use for my thesis. And

while there may have been some use for it or at least used for differential geometry and I suspect they were, I could never tie it up. And again, it served only one purpose, it became my thesis in the way of getting a doctor's degree.

MERTZ:

Now if we could just go back a little bit to the time that you moved to New York and started to work at Bell. I gather that the influence there was through the man who had formerly --the German textile engineer--who had formerly worked with you to learn mathematics from you and at the same time, you acquired some knowledge about filters.

LOVELL:

Yes. Actually, I continued my work there. First of all, he was a German, and I had to - I'd studied French in college quite a bit, I had three years of French, but I had no German at all, so the first thing we did, knowing that I was going ahead and trying to get my doctor's degree, I had to have a reading knowledge of German. And I said, alright we'll make a team. You want some physics and I need some German, so we'll get Das Handbuch der Physik, and we'll read it. And we'll read it in German and I'll explain it to you in English and you to me in German. But we'll learn the physics and the German at the same time. So you learn the physics and I'll learn the German. And we started an hour every day until I got, reported to work, and we continued that for several years. As a matter of fact, a long time after we finished the work and I passed my German examination, we continued working an hour a day, which was mutually beneficial.

MERTZ:

And when you came to work at Bell, was there any particular problem that they were interested in having you work on at the time?

LOVELL:

No. And I went into acoustic research. And I worked under E.C. Wingate, who has many of the basic patents for sound movies; super sound movies came into being right at that time. And Wingate had several of the basic patents, like microphones and loudspeakers, and recording sound on film recording. And they still were undecided as to whether they'd put sound on disc or sound on film; and it later on went on film. But I got into that area and also Malina went into this same area.

MERTZ:

I see, so that you continued to collaborate?

LOVELL:

We continued to work. And I guess my interest in the machines got stimulated quite a bit by Malina. I've got several patents. He and I have a, the first patent on the push-button telephone.

MERTZ:

Oh yes? Does it come from, date from that period?

LOVELL:

Oh yes! It doesn't look like that one, but these are my patents. It was again a team effort. Some of these patents are individually to me. I'm looking for the signature sheet. [Shuffling through papers]. As you see the first push-button telephone had ten buttons in two rows. And that was done before World War II.

MERTZ:

Aha. And was this part of the early--

LOVELL:

See here's one, Clarence A. Lovell and Rudolph Malina.

MERTZ:

Aha.

LOVELL:

This thing here.

MERTZ:

Yes. Well, then you went to work in acoustical research and out of this came general patents of your own and joint ones with--

LOVELL:

Oh yes, with Malina and I became my own supervisor. I never was Malina's supervisor, I guess I was Malina's supervisor but not till several years later.

[Recorder off]

LOVELL:

I think I can tell you how I got interested in mathematical machines. For some reasons, which I don't remember, the Head of the Department in which I worked, was Harvey Fletcher, who was a famous man in his own right. He assigned me to go to MIT and look at their Differential Analyzer that was built by Bush.

MERTZ:

Do you recall when this was?

LOVELL:

Oh, it was somewhere in the thirties, but I'm not sure. But fairly early in the thirties. It was Vannevar Bush that had this mechanical differential analyzer and I went to MIT to look at it and report on it to the Bell Laboratories. There were also several other people there that were dealing with machines of certain kinds, although really all of these had the nature of analog computers. Some of them were model names but there was a man there that had an automatic--a machine for analyzing various kinds of spectra --light spectra; I went to look at his machine. And I remember that his machine wouldn't work when I got there, and we spent the whole morning trying to make it work and we decided to go out to lunch and the minute we went out to lunch the thing started to work. And then after lunch we started, I forget what it was, but I spent most of the morning leaning against the wall and when I didn't lean against the wall it did work. The light beams were so finely lined that I misplaced them by leaning, one of the units on the beam was between two walls and when I leaned against the wall the thing didn't work.

MERTZ:

This wasn't, I don't believe, this was the cinema integraph, this was some other device. Did you spend very much time up there at MIT?

LOVELL:

No only a couple of days at that time. But that did get me interested, particularly Vannevar Bush's differential analyzer. This was a machine that solved mathematical problems by integrating differntial equations.

MERTZ:

Did you have any occasion to work any problems on that machine?

LOVELL:

No. I'll tell you what I really went there for. At the time they were working, while they were making the differential analyzer work, they were trying to put together an electrical differential analyzer, which the various components like an intergrator or an adder or so on were electrical and they were connected together with wires rather than the whole thing was mechanical. And the Bell system was offering them some switches to do the connections so that they could switch in mass. So that they could make the machine do one thing or another depending on how the switching was done. And I was something or somehow or other connected with defining the switching problem they wanted to solve and see if they could do that job.

MERTZ:

Do you recall who else was working on this problem at MIT at the time you were there?

LOVELL:

Yes. M. Caldwell.

MERTZ:

Caldwell. Aha. And --

LOVELL:

[?] Ed Bell…Caldwell was involved in this; he is one, turned out to be one of the members of the committee. The committee set up by Wiener had Caldwell, Gordon Brown and Norbert Wiener were the members of that committee.

MERTZ:

Right. Now Ed Bell who was,…did you, this was then an activity that went beyond the Acoustics Department itself?

LOVELL:

Oh yes. Maybe it's time to get to that. This story has a lot of fantasy-like things in it, but it's actually true, all of it. This handles it’s got lot of computers that solved a practical problem as a result of a dream. Dr. Parkerton who was working on a little analog device, it was really a servomechanism for measuring the level of sound in the room and following it with very high-speed variations. You see, the sound in a room will die at a rate of about 900 DB per second. He recorded the way it dies, or it builds

up. You've got to make the thing that follows that kind of speed ---900 DB per second is very, very fast, 10 DB is changes 10 to 1.

MERTZ:

That's a very rapid rate.

LOVELL:

10 to the 9th change. But in 1940 when the German troopers went into Rotterdam and bombed it out, the next day---two days later Parkerton came in to me and said he had had a dream and he said he and I was a member of an anti-aircraft gunnery .... I mean an anti-aircraft crew and two things I remember every time we shot, we brought down a plane. And there was a _____ geometer around the tringene for the gun and the gun moved the pressure on that geometer; he remembered that. And out of that came the gun director. Though most of it was invented while we were awake instead of dreaming.

MERTZ:

Instead of dreaming, eh?

LOVELL:

But he and I then asked the question, what do we need to make such a replica? Well, one of the things that we needed was a differential analyzer because we have got to find out how much that

[Recorder interrupted]

LOVELL:

So, we must know the speed of the plane and its direction. We must know the effect of gravity, wind and spin of the shell on the projectory and we've got to put those things into a machine which takes them to the unknown results simultaneously; the first set of values which make up the plane, the aircraft and the shell needed space. Now what we really did again, was to assume that the machine had some of the properties of the machine we were working with. That is, that we had the properties of the parameters were in electrical signal form and we had to generate functions through this signal, and we were already dealing with a round potentiometer. So, we ended up methodically to invent a system, and after three days we as a potentiometer that's wound like this, the sine function looks like that. Now we can make the plus and equal sign come by taking the polarity of a battery and connecting, say a common one at this point and then putting a negative volt potential there and a positive potential here.

MERTZ:

In reverse?

LOVELL:

In reverse. Then we put two brushes on at 90 degrees apart so that when one of them is picking up the sine of angle, the other is picking up the 90 plus the sine--the sign equal and 90 plus the angle. So then we had to have a repeater which would reverse the sign and not change the scale factor at all. So, you see here we look at this, to put it through an amplifier and at the point, the sign of the angle, at this point the negative of it .... at 180 degrees away the negative of it you see which is the same. And so, we found then a whole set of amplifiers, a whole set of components. This is that amplifier which adds--you see you put three inputs here--and it stabilizes is gained by feedback. And the feedback makes this point really look like zero. So that you got a semi amplifier. Do you understand how a semi amplifier works really?

MERTZ:

As described in Korn and Korn, I doubt that it's [?].

LOVELL:

Well this is the first of the semi amplifiers; this is the first. And the invention was made by K.D. Swartzel who is in fact mentioned in that study ... the report that was made by the Franklin Institute. But we came up with this better set of conditions which we thought would do the job. We had to make a coordinate conversion, you see. You track the thing with polar coordinance and we did our calculations with rectangular coordinates. Then we had to go build a set of servos that would coordinate, convert back to polar coordinance because the gun was going to be changed [?] .

So, we made two sets of conversions and there that's a coordinate convertor. That's the azimuth diagram [?] right there the coordinate convertor. Here is the early basic patent on the predictor. And the names were, Lovell, Parkenson, Swartzel and Bruce Weber. The original idea was Parkenson's. I invented the coordinate convertors, Swartzel invented the semi amplifier, and Weber I'm not quite sure now what he did, but he made some contributions in there which I don't remember exactly what they were, but he had to have them put in there by the time we had a solution. Now there're several inventions growing out of that.

MERTZ:

Excuse me. This was essentially the work on which this citation from the Franklin Institute was based?

LOVELL:

Yes, right.

MERTZ:

[?]?

LOVELL:

And it's also the thing on which the medal for merit was based. And Parkinson was involved in both of those.

MERTZ:

Could we go back just a little bit now. We skipped a period in the thirties there. Your original involvement in the early thirties with the computing machines was, if I recall, was when you went up to MIT to look at the Bush differential analyzer in connected with some relay equipment... or some switching equipment that was to be ....

LOVELL:

Somewhere around about that time, one of my supervisors got-- my supervisor got promoted and I got his job. I had two people working for me, one of which enters into this story only slightly but his name was Dean Woolridge.

MERTZ:

Oh yes!

LOVELL:

And he became the Woolridge of "Thompson, Ramo & Woolridge".

MERTZ:

Was this in the thirties?

LOVELL:

This was in the thirties.

MERTZ:

You were no longer then in acoustics?

LOVELL:

Yes, I've always been in acoustics. And Woolridge started building a magnetic tape recorder; that's the first work he did that worked. But actually, we had a very nice situation there. Parkinson and I worked on this directory that compensated for about two weeks and one morning then we went into my supervisor and told him what we had. He listened for a little while and said that, I think Fletcher and his boss ought to hear that. And we talked to Fletcher for about twenty minutes and he said, I think Keller ought to hear this. And Keller was Director of Research .... M. D. Keller.. And we went to Keller's office and he listened until about 10:o'clock and he said, I think Jewett ought to hear this. Jewett had just been made Chairman of the OSRD, or the NDRC it was called first, and he's going to Washington this afternoon and he's just been talking to me about the problems that the NDRC are responsible for, we were still NDRC then.

RRM:

Yes, National Defense Research Council.

LOVELL:

Right. So we went down to 195 Broadway here, that's where Jewett’s office was at that time. And he listened for an hour or two and he said, have you got a memorandum on it? And I said no. He said well if you write about a two-page thing on it before I go, I'll take it to Washington with me. So we went back, Parkinson and I, and wrote two pages on it and Jewett took it to Washington that day. From Monday at 8:30 to 9:00 a.m. when we got to work, till 4:o'clock that afternoon, we went all the way up the Vice President's office. [?]. He was the Vice President of AT&T and the President of the Bell Laboratories. Well, that started things.

MERTZ:

That's pretty fast moving.

LOVELL:

That's very fast moving, and it was very timely but, of course all of these things are stimulated by the same thing like with the War in Europe. The bombing of Rotterdam by the [?] stimulated Parkenson and of course, stimulated all of the activities that was leading us right into the War. We took this to the NDRC and it took us from May until December to get a contract out of the government even though the

NDRC was not as sticky as the War, the Artillery Board _ which had the anti-aircraft at that time. Now it took us from May of 1940 to December of 1940 to get a contract.

MERTZ:

Did you in the meanwhile though continue work on it?

LOVELL:

We did. We spent about a hundred thousand dollars on Keller's improvements. We put up a [?] room and we started out there keeping it quiet about what we were doing. And so, we had nearly six months to work on it up until the time the contract came through. And one year later on Pearl Harbor day, we we're testing that model in Fort Monroe, Virginia.

MERTZ:

Oh, mhm. So, you had moved from the

LOVELL:

In a year and a half, we moved from mu supervisor's office to a test ground in Fort Monroe, Virginia.

MERTZ:

Now, was this test a pilot test in a sense?

LOVELL:

We built a model called but called it a D10 Gun Director, we built a model and Fort Monroe was the official testing area. And we had moved it down there in November sometime and we were setiing up to get tests started. We weren't really taking any data by then, though. When Pearl Harbor day came, we were down in Fort Monroe, Virginia, we heard about it down there rather than home. So did we remarkably good, to do this model in a year.

MERTZ:

Well, just to follow this story a little further, roughly when was this put into production?

LOVELL:

One year later. One year and one month later. before we even tested the first production model.

MERTZ:

Aha. And was it in Fort Monroe?

LOVELL:

Well, it turned out to be in Wilmington, North Carolina by that time. We were in the War, and we had started putting new laboratories and new test plants all over the place by then. And we were, in a year, a year and a month later, I think, we were testing the first production model. And the model, of course, that meant that the model was approved, and later on we found out that they had given about one-tenth of the error on that and it was about the best system that they had ever had before that.

MERTZ:

So that increased the efficiency considerably.

LOVELL:

Well, the irony of that is that we found out later that the M 1 director, which was a superior mechanical director, which shot down 17,000 shells for every hit it made, and that's about the best we could do. 15000 shells for every hit. In many cases, the first problem and I was over there, and I watched them and it was shooting against a cover of flying bombs and these were --

MERTZ:

Oh, the Battle of Britain?

LOVELL:

Yes.

MERTZ:

Oh, I see.

LOVELL:

Well, anyway, they went through this system. And then I worked with Dr. Keller, on other contracts. One to the Navy, and one to the Airforce. One was a naval antiaircraft weapon, and the other was the bomb factory. And Dean Woolridge, who was taken out from under me and put in charge of the bomb factory and at the end of World War II he was still working on this. Then he went with Hughes Aircraft and set up Ramo and Woolridge, which is now Thompson and Woolridge.

MERTZ:

Yes. Could we go back a little back in the history of the thirties. Did you in your career at Bell stay on then officially in acoustics until the time that you--1940--

LOVELL:

Well, we stayed in the acoustics group all through the research part of it, and the Fort Monroe, Virginia, tests were conducted by their research department. Now you know in the Bell Laboratories there was a lot of mathematician's, there was a whole at Whippany, but down there they didn't do anything but develop military weapons. This whole job was contracted out privately. but this was one of the ways that they brought in the military part of Bell Labs to bear on the problem. While the military area was developing radars, we never got into that organization at all, we stayed outside of it and it was a non-military effort that developed this rocket.

MERTZ:

Did you have any contact with George Stibitz or any of the really --

LOVELL:

Yes, Stibitz was, what do you call it, technical assistant to this Committee.

MERTZ:

The NDRC Committee?

LOVELL:

Yes, technical assistant to this committee. George Stibitz was made, along with Professor Russell of Columbia University, made technical assistant to this Committee, and they helped test, set up test programs.

MERTZ:

I see.

LOVELL:

Actually, Stibitz and I stayed in the same room many years as office mates.

MERTZ:

Oh, you were one of his roommates also?

LOVELL:

Yes.

MERTZ:

Was this in the thirties?

LOVELL:

Yes. And he did a lot of-- well, he built a digital testing machine to test the very thing as part of his work. During the War and after the War, he built a digital tester for testing this analog computer.

MERTZ:

I see.

LOVELL:

As a part of his work.

MERTZ:

Now was this his relay, one of his relay machines?

LOVELL:

Yes.

You know, Stibitz was always a non-conformist--I'm getting off the subject a little bit, but not off the subject of machine computers. And Thornton Fry, who was head of the Mathematical Research Department at the Bell Labs all through the War, well, he built an analog machine to solve a rush of high degree equations say a 10th order equation. And Stibitz was always critical of everybody else's work. He was a non-conformist, and he said to Fry "You spent 50000 dollars, and I can build a machine in my basement that can do the same thing." Fry didn't believe it. And Stibitz did. So he built the first digital machine as a result of a and I know it.

MERTZ:

How was he as an officemate?

LOVELL:

Oh, he was a non-conformist, he was he was always arguing with somebody.

MERTZ:

Did you enjoy him?

LOVELL:

Oh, yes. He was a very down-to-earth individual as well as being a very, very… He really built the first specific-purpose relay digital computer. And all the computers that the Bell Laboratories built during the War, including the Model V, were based on Stibitz's design. He built the first binary digital computer at Bell Labs and I guess he ran neck to neck with Howard Aiken at Harvard University on building the first general-purpose digital computer. A token beat him out

MERTZ:

Did he ever have any sort of anecdotes or stories about him from that era in the thirties that might be historical?

LOVELL:

Well, the only thing--I don't know whether it's acceptable to publish or not--but Stibitz invented the relay digital computer with the specific purpose of making his boss look bad. He had an argument with his boss, and his boss told him he was out of place in this organization.

MERTZ:

[laughter]. And he went ahead and did it.

LOVELL:

Her went ahead and did that. He actually copied in a digital form or built a digital machine to do the same job that was done by an analog machine prior.

MERTZ:

That didn't make his relationship to his boss terribly friendly.

LOVELL:

No, he never did come back after the War; he never did come back to the Laboratories. He left the Laboratories and went out to be a special consultant on digital computers at the end of the War. Stibitz did. But Stibitz is one of the real originators of digital computers.

MERTZ:

Did you enjoy your years in an organization as stimulating as--

LOVELL:

Oh sure. I was a little bit of a non-conformist, too, I guess. And we all shared one thing didn't think very much of either one of us; so, we had something in common. Well --

MERTZ:

Did your work overlap at all with Stibitz's?

LOVELL:

The only overlapping that took place during the War was that Stibita was always assigned to the testing of our machines. The minute you go to change the accuracy of the machine by two to one or three to one, it means that the tests were adequate before are no longer good, because they need to be revised somewhat the same way.

MERTZ:

This is particularly true when you get to the goal change the accuracy of a machine the disk setting for adding control that needed

LOVELL:

Yes. So, there were three--there was another thing that was kind of interesting about this job. Thornton Fry, Gordon Brown, Warren Weaver and Sam Caldwell wanted this machine and had gotten a contract. They came up with a concept for the same type of machine which they felt was better. And in many ways it had better things; you see. What we did is is take position in polar coordinates, transform them into rectangular coordinates, predict the future in rectangular coordinates, and then go back. They said "well, why don't we, instead of doing that, why don't we make a one-plus machine. We'll keep the polar coordinates, and we'll calculate predictions to the angles. And this would have been a beautiful thing if the to the angles wasn't sometimes at 180 degrees. You see, if we had turned the thing right over into future position, and the present position is backed off, well, then the future position is already a 180 degrees.

MERTZ:

Hm.

LOVELL:

And this was a beautiful thing. 180 degrees.

MERTZ:

That's right.

LOVELL:

If they went right over you…

MERTZ:

From a negative to a positive relation

LOVELL:

So, if the one thing that really had…But they asked the Laboratories to take on that job of building one for comparison with it. And incidentally, Rudy Malina was the mechanical on that competitive machine. It actually was so good that they had to decide, the Bell Laboratories. whether or not to substitute it for the one we had. one we had. Now that decision was made later on, not to be done, because they were testing the second model at the same rate that we were testing the first production model, and they figured that that year and a half to two years was just too much.

MERTZ:

So, in a way the pressures of the War itself

LOVELL:

Kept a somewhat better machine from being substituted for one which may not really have been good enough. The differences between the two machines were enough-- well, as a practical matter but it didnt make all that much difference. I think the machines were nearly expendable. They had a better concept, but they didn't, and it was never built.

MERTZ:

Do you recall, were there any other things that Stibitz was doing? I assume he was working on other problems as well as test programs.

LOVELL:

I don't really know. I didn't know what Stibitz was doing except the contracting. There was a very interesting thing about this job, which had to do with the tying up of the radar. You see, at MIT there

was a group of people under Louis Ridenour and Dr. Strauss, I don't know, Professor Woodrow and others, they were doing radar designs.

MERTZ:

Was this in the Radiation Laboratory.

LOVELL:

Weaver, there was a Weaver there, too, in the group, not Warren Weaver.

MERTZ:

Well, there was Wiesner.

LOVELL:

No, that wasn't the one. There was one other man whose name I can't think of. But they, they had a job to build a Radar for ODRD, NDRC. The Bell Laboratories had taken on the job for building a radar for the Signal Corps. So, the Signal Corps gave Bell Laboratories the specs. And the specs did not mention they were all data did not restrict the Radiation Laboratory to the data transmission. So, the end of it Ridenour and company came with us and we built them a bunch of potentiometers and put them on the shafts of their tracker. So, when we went down to Fort Monroe testing and they were down there with an experimental radar called XT1 testing, and they just put the two together. And so, actually when the system got in use; it was used with the XTR 584 radar which was the outcome of the XT1. The M9 and the MK and M8 directors were all used with the XTR 584 very much more than they were used in the Bell Laboratories.