Computer Oral History Collection, 1969-1973, 1977

Interviewee: Herbert R. Grosch
Interviewer: Richard R. Mertz
Date: November 9, 1970
Repository: Archives Center, National Museum of American History

Tape 9, Side 1

MERTZ:

This is an interview conducted with Dr. Herbert Grosch on the ninth of November 1970 at his office at the National Bureau of Standards in Gaithersburg, Maryland.

GROSCH:

In our last conversation we passed by, rather rapidly, the advent of the hardware people at the Lawson Laboratory about late 1945, early 1946. I'd like to go back now because those people were important in the development of some of the early IBM machines, and at least one of them is still important in the art today. That is, at least one of the people is important in the art today. The three men who were brought in as a part of the first batch were chosen by Wallace Eckert from a fairly large group of young, technical men who were available at the Radiation Lab at MIT. Several people who were close friends of Eckert's had been at the Rand Lab during the war, Columbia faculty members that he had known before he went to the Naval Observatory, with whom he continued to be in touch. After returning to Columbia and after settling down into the five lunches a week at the faculty club routine, he called on these friendships - I remember specifically that one of them was I. I. Robbey, for instance, the Nobelist, for advice on who he might talk to. He selected, early in the game, a young man named Phillips, who I believe already had his Doctors Degree. His name is preserved in one of our early Lawson Lab Bulletins. Phillips decided to take a job somewhere else in the more academic circles, and didn't come, and this was somewhat of a disappointment to us. But the three hardware men that we invited all, in the end, came. They were, in order of their appearance, Robert Walker, who was an older man more experienced in what you might call the practical side of the business - a ham radio sort of a person, although at a high level, you understand; Byron Havens, who is today the Director of Engineering for the World Trade Corporation, working for IBM in Niece, France because of its proximity to the IBM La Gaude Laboratory there, but actually with responsibilities all over the world since World Trade Corporation governs everything except North America; and the third one was John Lentz, L-E-N-T-Z, the youngest of the three who was still intent on continuing his education. Among the interesting properties that Havens had was the fact that he was very shy about his middle name, which turned out to be Luther; and among Lentz' interesting properties was the fact

that he was an expert - a very expert square dancer and organ player, and it turned out that his wife a niece of Burton, the Supreme Court Justice.

MERTZ:

Were - did any of these have advanced degrees?

GROSCH:

No, none of them did and I believe none of them ever succeeded in getting their Doctorate. Havens was sort of - Walker was too old and didn't really intend to do so. He was content to continue to be a laboratory technical type. Havens was sort of past it. He had acquired such a reputation at the Rand Lab that he was actually offered, I believe, the head of one of the smaller electrical engineering departments in the country. I believe it was Louisiana State, but I'm not absolutely sure at this length of time. He turned it down to come with IBM. So that he had, so to speak, gotten into the senior circles of the electrical engineering advanced communications kind of electrical engineering business without a degree, although he was still quite young. And Lentz intended to get his degree in Physics at Columbia as part of the arrangement with the Watson Lab, but was so deeply engrossed in Watson Lab activities that as the years went by he really never quite succeeded in making it. Lentz is also still employed by IBM. I believe Walker is also, but Walker sort of dropped out of sight. At a later date Lentz became sort of the grandfather of the line of small IBM scientific machines. And I'll mention that very briefly in a few minutes, because the grandfather term implies that he wasn't directly concerned with it - that they were actually developed in the regular engineering laboratories, but he had some ideas at the Watson Lab which, in turn, stimulated other developers to go into this scientific line. I'm thinking, particularly, of the IBM 1620, for instance, and its predecessors.

Well, these three young - these three men appeared rather early in the game. Walker busied himself hiring some technicians and setting up a working electronics laboratory in the second floor spaces - high-ceiling second floor spaces at the Watson Labs on 116th Street while it was under construction.

MERTZ:

Approximately when was this?

GROSCH:

This would have been the Spring of '46 by this time. I'm not sure, but I believe the negotiations were conducted quite late in '45, and I remember particularly that Phillips was supposed to teach a course or two in the beginning of the fall term in the fall of '46, so any disappointment that we felt about his not appearing obviously was in the spring of '46.

Walker's business of setting up the electronics laboratory must have been in early '46 also, but he obviously couldn't proceed very fast with it until we had the remodeling done in the rest of the building. So he was doing this about the time that I was going through the setting up of the library and the moving of the punch card machines over from the…Laboratories. He had several fairly senior people, including one man whose main duty was to ride downtown in the subway and buy parts in Courtman Street Electronics Market that was still going at that time, now wiped out by the World Trade Buildings. That was because in those days, of course, there was an enormous amount of surplus electronics available from the military business, but very little that you could order in quantities from a catalog from the factories which were converting over to civilian production, rather with some difficulties. Now, remember, this, of course, is back in the vacuum tube days and, in fact, in the rather large vacuum tube days. A large number of the bottles then in use was a full, you know, each diameter - large sized tubes that we used in ENIAC and in the SSEC. I think I mentioned in an earlier interview, for instance, that there was something like twelve thousand vacuum tubes in the SSEC, of which a large number of four or five thousand were twenty-five and that they had been purchased on this surplus market chosen, simply, because it was possible to get that many of them at one time. Well, they…(inaudible due to loud noise)…to work on a longer range basis than that. They were not concerned with building an SSEC the day after tomorrow. They were not concerned with turning out a production machine of any sort. It was their instruction from Watson, conveyed through Wallace Eckert, that they should go into the use of modern post technology rather than to develop the patents that Halsey Dickenson and others had worked up for IBM in the past, and in fact Dickenson and the rest were still continuing in that somewhat more old fashioned line up at Endicott while this was going on.

Walker and the semi-professional people that he hired also set up a small machine shop in the sub-basement of the Watson Lab, with the lays, and the drill crafts, and chassis punch, and all that sort of thing for making mechanical gadgets, and some of this was used in experimental work at a later date. Primarily, however, it was a shop to enjoy yourself in, really, rather than to do really complicated work. If anything more than small quantity work was required it could have been done at Endicott by use of the normal IBM facilities. In fact most of the work was electronic, and the fancy machining wasn't required to a very considerable extent. I think the intention probably was to explore mercury delay lines. And, in fact, I remember distinctly that Havens did make some attempts in this direction, but it was already - the technology was already marching so fast that it was pretty obvious that the mercury delay line principle was obsolete. And I remember By, as we called him, Byron Havens, By went on to solid delay lines very quickly before he'd even gotten a full scale adder build, or a full scale piece of equipment going.

By the time that he began to experiment on large scale systems, he had expanded into the fifth floor attic, which was the old fraternity house chamber where they used to, as the joke goes, ride the…None of these spaces was air conditioned and it was a little difficult

to work under these conditions, but there was considerable dedication on the part of this small group working with these men, so they handled it very well in spite of their rather restricted conditions.

Now, Walker set himself to building an analog computer in…Francis J. Murray, then a professor of mathematics at Columbia, and I think probably still is today. Murray was a consultant to several of the analog military projects; Cyclone, Whirlwind, and so forth, and was very much interested in things like the convergence of mathematical processes in analog computers. So he and Walker set out to build a simultaneous equation solver. Ten or twelve linear equations in ten or twelve unknowns, I forget the size of it. Walker's contribution being essentially to use the non-linearity of some vacuum tube circuits in such a way as to compute squares with them, and them summing the squares to get the criterion for convergence to be the sum of the squares of the residuals of all the equations lumped together, which is, of course, a guaranteed - not that it's convergence, although the practical use of the machine didn't turn out to be too satisfactory. The major failure of the machine was that Walker had designed and built some static card readers in which you punched your inputs in two or three significant figures in digital form on standard IBM cards, and then locked them in to a platen device with little pins that would go down through the holes and make electrical contact. Some question in my mind about whether or not this may not have violated some Remington Rand patents, but since it was an experimental device, nobody worried much about it. In fact the contact performance was inferior, and usually you had to jiggle around a good deal before you could get all of the poles to work. So it wasn't really a very satisfactory device, and, in fact, it was, I think, fifteen years before they really had good static card readers on the market. IBM, itself, at a later date, conquered the problem by holding the card still but stroking it with moving electrical brushes in the 407 feed, for instance, which had the same effect - that you could read the card over and over again without physically moving it, but got around the idea of the purely static contact.

Well, that was Walker's project. He did use the machine shop in the basement and it wasn't very satisfactory.

Lentz began working on what we would not think of as a disc computer, but the disc involved was a plated one rather than a…one. And he wasn't concerned with the development of the componentry so much as putting together a small, convenient desk calculator kind of machine. Sort of a predecessor of the modern electronic machine. Again, he didn't carry this forward very rapidly because many of the things he needed weren't available. But the high speed circuitry, of course, the modern radiation lab pulse technology that he brought with him from MIT, of course, was useful. And, as I say, he, in a sense, became the grandfather to some of the small, scientific calculator developments in IBM at a later date.

Meanwhile Havens, who was the biggest prize of the three, I think, in Eckert's eyes, and turned out, indeed, to be the most important of the three for IBM. Havens, challenged by the SSEC and it's old fashioned circuitry and its monstrous size and the fact that it clearly

could not be turned into a commercial device for IBM mass production, decided to build something different. After working for some time on much more high speed circuitry, solid state delay lines, and having constructed adder boxes, that were small in size that used the miniature half inch, or three eighths inch diameter vacuum tubes, rather than the great big, full sized ones, he decided that he was now ready to go ahead into a full sized machine. And IBM, in ways that I was not completely privileged to, got a contract from the Naval Ordnance Research Lab - sorry, got a contract from the Dauldrin(?) Proving Ground, where you remember, one of the five multiple sequence relay calculators had been sent: Two to Aberdeen, two to the Watson Lab, and one to Dauldrin. They got a contract from Dauldrin to build a super, super, electronic computer which was to be called the NORC - Naval Ordnance - that's why I made that slip a moment ago - Naval Ordnance Research Calculator.

MERTZ:

Are you sure it was Dauldrin?

GROSCH:

Yes sir. It was installed at Dauldrin and worked until...

MERTZ:

...(inaudible)...actually Dauldrin...

GROSCH:

You mean in the sense that the contract might have come from Washington?

MERTZ:

Yes.

GROSCH:

Yes, I think that's certainly possible.

MERTZ:

Because there were two competing facilities - facilities for the machine at the time.

GROSCH:

Um Hmm.

MERTZ:

...(inaudible)...

GROSCH:

Um Hmm. That's probably true, if that's the case. The contract was probably let in Washington, and Dauldrin, as you say, won. I was perhaps misled by the fact that Dauldrin did have the earlier IBM relay machine, and I thought it was a one to one relationship.

MERTZ:

...(inaudible)...

GROSCH:

Um Hmm. I don't believe I ever knew this. It isn't something I've forgotten. I don't think I was privy to those discussions. Havens, of course, was.

MERTZ:

Uh, one question. When you were mentioning solid state delay lines, what specifically were you...

GROSCH:

Solid delay lines. If I said state it was a mistake. I didn't mean to imply that. You could send acoustical waves back and forth in solid physical material as well as in...mercury. I don't remember at the moment what material he was interested in investigating, whether it was quartz or some plastic or other, or something. It's so far back in the past, I'm not even certain whether he incorporated these devices in the NORC machine, because by the time NORC was actually delivered, I was working for General Electric and was a - if not a competitor, at least an observer from the sidelines. I did observe, you might call it the physical circumstances of their working, which, among other things, involved Havens' running out of space as he got closer and closer to a real machine and moving around the corner to a second and third story operation on Broadway near the Choc-Full-'O-Nuts emporium on Broadway between 115th and 116th Street.

MERTZ:

Right. One other question in that regard. Do you - was he concerned with any internal storage kinds of problems of that type, do you recall? This is a time in which there were, perhaps, there was a great deal of preoccupation with exploring a variety of things?

GROSCH:

I'm sure, for instance, that he was interested in core storage and so on, because of having come from MIT and being an avid reader, you know, of the Journal of Applied Physics and so forth. He must have been aware of the coincident, current article that Forrester published. But I don't remember discussions about it. One of the reasons I'm not as clear as I should be on the hardware developments of this time was that these three men tended, of course, to be in a little world of their own. Eckert and I and Hillof(?) Thomas tended to be the applications and user groups, and they tended to be the new equipment group. Of course Eckert was deeply concerned with what they did, and know a great deal more about it than I did since he was the boss, but, in fact, the major difference, really, was a social one. That is that Eckert, Thomas and I used to be great frequenters of the Columbia Faculty Club and tended to regard ourselves as members of the Columbia fraternity, and the other three, not having Doctors Degrees, and not being as welcome in electrical engineering, and physics, and so on as Eckert and I were in astronomy, and Thomas was in physics, tended not to sat at the Faculty Club, and when they occasionally over to eat with each other. So there wasn't the free exchange of what we did today sort of thing around the table that you got within these two half groups. No animosities. We were all very good personal friends, but it simply was a division of two kinds of interest and two kinds of knowledge.

MERTZ:

How early on was it being ...(inaudible)...

GROSCH:

Havens and Walker were both in the 116th Street Watson Lab at the time that I left, which was in the summer - or the fall of 1950, I guess it was - spring of '51, I'm sorry. I'm having trouble with my years here. Havens had already moved down around the corner, and I would guess it was probably around '49 when he did so. He'd been around the corner for at least a year. The 115th Street location, which is now called the Watson Laboratory wasn't even dreamed of at that time. The decision to do that came much later when Eckert began to get much closer to fundamental physics and chemistry, partly as a consequence of the rest of IBM going very heavily, of course, into development technology at Poughkeepsie and later on York Town Heights. But at the time that I'm describing, in the '46 to '49 era particularly, this was really IBM's most advanced laboratory. It's hard to think of, but of course you also have to remember that IBM was something like a $200 million dollar a year company at that time, instead of the seven or eight billion a year that it is now. They had quite restricted laboratory facilities overseas. Almost nothing, in fact, overseas. I think British Tabulating Machines, which was affiliated with them, had some small development shops. The French factory had some small development shops, but it was really trivial. Poughkeepsie was really still just a typewriter and key punch factory in the early days of this '46–'49 era. Now as the - one of the first things that IBM did about this, of course, was that they took the old

Halsey-Dickenson patterns, I'm not sure that I mentioned this in an earlier talk, and they brought out a commercial machine called the 603. Quite quickly after the advent of the SSEC, it was, in fact, the same circuit sort of cut way down in size - the SSEC really doesn't have any grandchildren. That 603 was sort of an illegitimate son, and after that the line terminated because those circuits were old fashioned, the whole process of that line of digital computer development was an aborted one. But the 603 was made in quantity - the quantity being about twenty, as a matter of fact. It was physically a small gang punch, that is a non reproducing, single speed punch that was normally attached to a tabulating machine. And attached to it was this large box that looked like a room heating convector; black as all IBM machines were in those days, containing three or four or five hundred full sized vacuum tubes. And what this gang punch did was to read on a card, as it passed under the brushes, two six-digit numbers. The vacuum tube multiplied these together and produced a twelve digit answer, rounded it off to six digits, and punched it on the same card as had arrived under the next punch station. There was no flexibility. All you could do with the plug board was to change the columns in which you road the punched card. But you couldn't even control the rounding operation. It was six times six equals six, period. But it worked at six - thousand cards - six thousand multiplications an hour, which was nearly ten times as fast as the conventional electro-mechanical 601 which was its competitor. The 602, which was an electro-mechanical machine, but a much more flexible and interesting one was under development at that time, hence the number 603. Only twenty of these machines were built. I had the prototype at a somewhat later date, after they finished monkeying with it, at the Watson Laboratory, and there was one at Convair, one at Bowing, several at places doing inventory control. But it was a pretty restricted gadget, and to pay - I don't remember the rates any more, though I still have the sales manual with the price in it - but to pay on the order of five hundred to a thousand dollars a month just to do fast multiplication didn't attract many customers. Moreover, coming down the line from people, also, who were using MIT technology, but hired directly to work for IBM in the Poughkeepsie area, was the 604, which was, of course, an enormous success and is regarded by most people as the first mass production electronic machine. Actually the 603 was available for about a year and a half in advance of the 604. But there were so few of them made, and so little use for them in either scientific ordnance work that there's almost no trace of them left.

MERTZ:

Approximately when was this ...(inaudible)...

GROSCH:

I'd be hard put to hit it, right after that. Of course IBM announcement material will give it to you to the day. But I certainly had that 603 prototype in 1948. Whether I had it - whether it was available to others in '47 - I repeat that although I had the first one, I didn't get it first. They'd shipped a couple of production ones before I got the prototype. My guess, off hand, would be that I got it early in early '48, but I'd have to really look at the records to be sure. It was shown in the sales literature. I mean it was a standard item in

the line, but there was so little demand for it, and the 604 was coming down the road so rapidly that there was very little push behind it. It's referred to, for instance, in the recent Datamation(?) article by an anonymous, or by a pseudonymous character which refers to adventures in the air ship factory - the air ship factory concern being Boeing, and the machine that he mentioned being the 603. So you can - it's immortalized in that sense in a very disguised and informal fashion. And I still have a 1948 or '49 sales manual which shows it. And I'm sure I have an operating instruction book for it, too, which was only a few pages because it was inflexible. I don't have circuit diagrams of it.

Now the 604 was built by a group of people all of whom knew of Havens, Lentz, and Walker, but who had been hired directly from MIT to work in statistics as IBM began to convert Poughkeepsie into an engineering laboratory from the typewriter factory. And the - perhaps the first of these people that I met, and the one that I knew best in the very earliest days was a chap named Nathaniel Rochester - Nat Rochester. Nat had been asked to - had been offered several jobs when he left the Rand Lab, and was thinking, seriously, of going to Tracer Lab, which was going to build radiation instrumentation. He decided not to do it because Tracer Lab set up its shops in downtown Boston, overlooking the Harbor, and he didn't relish the idea of several tons of radioactive water being dumped over, and by a bomb which we then expected to drop at any moment in those days. So he came to IBM, Poughkeepsie, instead, which he figured was outside the radius of the typical twenty kiloton atomic weapon of those days. He set up shop in the old Kenyon estate. Now Kenyon was a rich man who had built a mansion outside of Poughkeepsie, and it's now the IBM guest house in Poughkeepsie. It's across the back road from a fairly large Laboratory operation that IBM has there now, but at the time I'm talking about nothing was there but the Kenyon house, itself. The head of the operation was Ralph Elmer, who was senior to Rochester. And along with Rochester IBM had hired Werner Buchholz(?) - oh, half a dozen other names, all of which are well known in the trade now, but Buchholz and Rochester were probably the two that I met first. And they had, you know, they turned the sun parlor into a drafting room, and they had equipment scattered around the place that was converted - mostly converted to 604 equipment. And this was the group that, in the end, turned the 701 - the first of the IBM large calculators. But before that, working more in the more conventional IBM surroundings, they - Palmer and the others had brought out the 604 and started mass production of it. As I remember it the punch part of the 604, the part which you ran the cards through was still made in Endicott, but I'm not positive of that at this date. Certainly, it was an only slightly modified standard machine. How much of the modification was done in Poughkeepsie, and how much in Endicott I don't know, but the big box of vacuum tubes, which was a small ⅜ inch diameter vacuum tube - the ones without bases - just the pin bases - that was done in Poughkeepsie, and was the first major electronic machine that IBM built anywhere in the world - the 603 being, of course, a very limited thing.

And, oh, quite a few of the older people that had worked on the relay calculators and so forth were involved in this in one way or another. But it was pretty much a new development. The -Frank Hamilton, for instance, remained at Endicott and worked on what turned out to be the 650. Working with Hamilton, originally, but later with Palmer

was Jerry Haddad who was the first technical - who was the first Vice President of IBM not to have gone through sales school. I think that's an accurate way of putting it. John McPherson was, in a sense, the first technical Vice President - he was the Vice President of Engineering. But John had gone through sales school, although he had an engineering course in college, he'd been an IBM salesman and an applications expert.

MERTZ:

Was he a vice president at that time?

GROSCH:

McPherson became vice president before any of the rest of them, yeah. He was made vice president, if I may say so, too early. That is he was made vice president at the time when they were looking for a technical man to call a V. P., but at a time when, really, the sales hierarchy wasn't prepared to accept a technical V. P. They were prepared to accept a chief scientist type of person, which they didn't have, or a Wallace Eckert type of person who was the closest thing they had to a chief scientist in those days. But they weren't really prepared to have a full corporate vice president who would really participate in the decision processes, as far as there was one other than Watson, Sr. The result was that McPherson suffered from excessive honesty and excessive frankness never really rose much above his initial vice presidency, and in fact is near retirement today in that same level. Meanwhile a whole bunch of salesmen and ex-salesmen were made vice president later, and went on past him to group executive positions - executive vice president positions and presidencies of the company. But McPherson was a great guy. To this day is undoubtedly, by far, the largest single source of information about the early adventures in IBM, far more than I am.

MERTZ:

Was he - where was he situated?

GROSCH:

He was at Gallaki(?) headquarters in those days, 590 Madison. The Armone(?) headquarters, of course, didn't come along for another ten or fifteen years. But, of course, he had to make frequent trips to Endicott, and later to Poughkeepsie. But he was a collector of papers, too, so that I'm sure his archives, if you could ever get your hands on it, would be enormously valuable. Much more indicative of the whole history of IBM than I can make.

MERTZ:

In that respect, did he participate at all in the selection of personnel either at Poughkeepsie or ...

GROSCH:

Yes, I'm sure that he o.k.'d every one of the people I mentioned.

MERTZ:

...or of Eckert's...

GROSCH:

Eckert, himself, probably not. But I would think that every other single person that I mentioned, including me, Hill, Thomas, Rex Seiburg(?), who ran the SSEC, Havens, Walker, Lentz whom we're talking about now, Palmer, Haddad, everyone of those people. The exceptions being either someone that the old man brought in personally, like Wallace Eckert, or somebody who came in very low in the system and worked up through it. For instance, two or three of the young people who worked for...

MERTZ:

(inaudible)...

GROSCH:

I can't answer that. Kuspaert(?) came in sort of at a middle level. Whether he came - I'm sure that McPherson was told he was coming and met him before he arrived and all that, but whether he actually helped select him, whether he had the right to say no or not, I really don't know. Probably, but I can't be sure. I'm almost certain that Eckert did not. He came in, as I say, at a middle level from Oak Ridge. And I have the impression that he sold somebody on the - in the sales hierarchy on himself and went into Watson, Sr. without either McPherson or Eckert knowing about it until afterwards. But they probably had to O.K. him before he was...

MERTZ:

Now the relationship between McPherson and Eckert so far as the people who worked ...(inaudible)...I assume that they, since they worked for Eckert, were the responsibility for their selection was largely his, rather than...

GROSCH:

You have to remember that IBM in those days did not have a budget and did not have an organization chart. Now that doesn't mean to imply that somebody wasn't keeping track of how the money was spent, and it doesn't imply that there weren't unofficial, but very clearly understood people who reported to people, sort of thing. Eckert used to, in a

sense, report to McPherson. But the fact that he had been brought in by Watson, Sr. and the fact that Watson, Sr. made almost all the high level decisions for everybody meant that most of the things he wanted he either had authority to do on his own, minor things, you know like hiring a secretary or requisitioning a typewriter he could do on his own. Or if it was so important that he needed somebody else's o.k. the chances were that he had to have the old man's o.k. anyhow. And once he - and since he had permission to ask the old man directly, there wasn't really much point in asking McPherson, because McPherson couldn't say no if the old man said yes, and couldn't say yes alone in most cases. But, for instance, in the detailed expenditure of funds, after the old man had said, o.k. take a million bucks and build the SSEC, sort of thing. Certainly McPherson was very important in that. And certainly Wallace Eckert regarded McPherson as being his superior in the company, if he had a superior. The chain of command...

MERTZ:

...(inaudible - both talking at once)...

GROSCH:

...used the normal chain of command.

MERTZ:

...over the execution of being...

GROSCH:

Right.

MERTZ:

...after the basic decision had been made.

GROSCH:

development techniques at IBM also. That, for instance, the Endicott Laboratory - first of all the projects were initiated and authorized only with the old man's consent. And the general feeling of how much you could spend you got from the old man. Well now, in addition to that, there was a laboratory manager who sort of reported to McPherson, but McPherson was really staff rather than line, and he sort of reported to the Vice President in Charge of all of Endicott. He was mostly a manufacturing man.

MERTZ:

For example, when you or some of the others...

GROSCH:

I don't remember who these people were...

MERTZ:

...associated with Eckert were originally employed, did you in addition to Eckert meet, or be...

GROSCH:

The only one that I ever met before I actually started getting a pay check was McPherson. And I'm pretty sure that if McPherson had not o.k.'d me, beard, sports coat and all, I wouldn't have been hired. But McPherson had had instructions either through Eckert, or directly from the old man to get the Watson Lab going quick, and when Eckert certified that I was a hot shot numerical analyst and knew all about the desk calculator kind of computing, and was available, why I'm sure McPherson didn't hesitate to say yes. But I would imagine that at a later date McPherson probably was criticized because he let a guy with a beard into the company. Eckert wouldn't have been, you see, because he'd been brought in to start something new. It was McPherson's task to insure that in doing this he didn't do anything too far out of line with Watson's policies.

MERTZ:

Now, was there ever a point in which these people brought in by Eckert - when I say these people I'm including yourself...

GROSCH:

Umm Hmmm, of course.

MERTZ:

...well both the sort of academic people as well as the engineering - the NORC people - were they - did they have access to...(inaudible)...Watson, Sr.

GROSCH:

No, I'm pretty sure they didn't. Now, I would imagine that Havens, Walker, and Lentz were probably run past him at one time or another. I'm pretty sure that they were aboard first before they met him just as I was. But they undoubtedly met McPherson. In fact I'm sure they told me they met McPherson. It's gone down into the noise level, now, and I've forgotten it, but I'm sure that they told me so.

MERTZ:

How about Watson, Sr. When was he...

GROSCH:

He was simply too busy for an ordinary person to meet him very easily. I mean you met Watson Sr. when he had time to see you, not as a formal process. Now if you were so important that you had to meet him before you came in, and certainly Eckert himself was a case in point. If Eckert had been hired without Watson knowing in advance there certainly would have had to have been a meeting at that level. Well, in that case you'd just have to sit around until the old man was free. You might be asked to come into the Gellaki(?) headquarters on Monday morning to meet with Mr. Watson, you might not see him until Wednesday. Even in those days at 140th - that was in '45, that was a $145 million a year company. Even in those days it was pretty broadly understood through the business equipment world that that was the way the company ran, and you weren't insulted by it unless you were a complete stranger. Sitting next to you, you know, would be the head of the fund raising committee of the Metropolitan Opera, and you know he had a little priority over you, but he was waiting too.

MERTZ:

And did he consider himself with the working of the lab more than just the appropriate...

GROSCH:

Oh, you have to remember that not a sparrow fell in the IBM Company without Watson knowing about it, or caring about it. This meant that things that in an ordinary company of that size wouldn't get even to a vice president got all the way to Watson's office. But they got in and out pretty briskly because there wasn't much time to waste on things like this. An example, for instance, is that after we'd been in business for a couple of years I wanted to get an adding machine, electrical adding machine that would print a rather complete record of what it was doing on a tape. And, of course, this is very common nowadays, but in those days adding machines that printed on a tape were pretty primitive and machines that did things more complicated, like the Marchant that I'd used for my Doctorate, didn't print. And the closest combination of the two was a Remington Rand Machine. Well, Remington Rand, of course, was competition although IBM in those days had more than ninety percent of the punch card business, and it wasn't in the computing business yet. And they hadn't yet bought UNIVAC, or Eckert and Mauchly as it was then. The fact remains that Remington Rand was all the competition that IBM had. So in order for me to buy that adding machine Wallace Eckert had to ask Mr. Watson's permission, personally. And he had a long list of things he wanted to talk to Mr. Watson about. He put this at the bottom of the list and when the list got to contain something important enough that he had to see Mr. Watson, he took the whole list down, sat outside the office for several days - I'm exaggerating, it might have been only a few hours - but he sat

outside the office until Watson, Sr. could see him and then went down through the list. And when he got to my adding machine the old man said. "Buy and Underwood", and that was the end of that, the decision process taking merely thirty seconds.

Obviously when you got into extremely technical matters that sort of thing wasn't possible. And one of the great problems that the engineering and later research people had toward the end of Watson Sr.'s life was that he still wanted to make many of these decisions, and really, hardly understood even the words in which the questions were posed. I mean you don't ask questions about investment in solid state physics research in one syllable English, you have ask them in pretty fancy physical language. And the old man simply didn't have that kind of background. But you couldn't tell him that, of course. The art of being able to ask him an indicative question, one which would produce money, personnel, physical space, or even more important that kind of organizational support that you had to have in a closely knit, centralized company like IBM - the art of asking that question was one of the most important art that a senior person could have. Watson responded quickly. It wasn't a question of drilling into his mind...(inaudible)...anything like that. He was an extraordinarily intelligent and responsive person. But you did have to explain it to him in such terms that he'd understand it, and in many cases he didn't understand it - the explanation went awry, and I'm sure that this was one of the main reasons why IBM was slow in getting into the large computer business. But he simply didn't understand from the technical arguments presented to him, the industrial, the commercial rate of growth that was projected. You had to convince him, you know, you couldn't just give him a snow job. He had to have some good argument.

MERTZ:

Well, that wasn't unusual at the time. I believe that...(inaudible)...

GROSCH:

I don't really remember that clearly, you know. You think you'd say that that would be the thing that I'd remember more than anything else, but in fact I was so deeply convinced myself, that I lived in an atmosphere in which one simply took this for granted, and I was not, personally, ever a part of that high level Watson decision process sort of thing.

MERTZ:

Well, kind of a similar example was the history of the Forrester...(inaudible)...when MIT…said there was no commercial value to this patent...

GROSCH:

Yes.

MERTZ:

...and initially refused to obtain the patent.

GROSCH:

Yes. The thing is that if you were a part of a controversy involving this you see both sides. You see, during this period from May of '45 when I was hired to the time that I was sent down to Washington to start the Technical Computing Bureau for them, there really wasn't any period in that intervening point where I had much contact with people who weren't already convinced of the power of this thing. Of course I was one of the major convincers, but I wasn't out in the frontier arguing with patent lawyers, or even with customers in that sense of the word. And though there was the kind of customer that said we don't need a big computer for business, was, you know, an insurance executive or department store magnet or something. And he was saying this to a standard IBM salesman or to an IBM vice president, something like this. The guys that I met tended to be vice presidents of engineering from Boeing, and so forth, and were saying, "For Christ sake, fellows, when are you going to hurry up and make us a better machine. We need this, this, this." So they were pretty convinced, and our problem, really, was to turn this into new equipment, better application methods, and so forth, and to a considerable extent jiggle of the sales and planning apparatus of the company to produce what they needed. Now I did get a lot of resistance, there. But you'll remember that kind of resistance was from people who were also convinced that technology and data processing were good things, they just weren't sure that we should go as far as a giant calculator, or make as many as several hundred 650's. But it was still not the resistance to the whole concept which you got in much of the outside world.

MERTZ:

What - do you recall any direct participation or involvement from Thomas Watson, Jr. in this period?

GROSCH:

Well, my contacts with Tom, Jr. were a little more extensive than with the old man, simply because he was at a lower level and a little more available. For a few months he was actually my salesman while I was running the computer side of the - computing side of the Watson Lab. When he came back from the Air Force he was assigned a territory like everybody else coming into the IBM sales organization, and within a few months he was made a manager of some sort, and a few months more he was a vice president. But for those first few months, yes, somebody in my entourage probably ordered punch cards from him, or something like that. And I used to have some contact with him at that level, and also socially in the sense that when there was an IBM function that he would quite

frequently be there. And he was a good deal more approachable than Watson, Sr., of course, who was surrounded by the bigger wheels. So Eckert would go and say hello to Watson, Sr., and I would go and say hello to Watson, Jr. But...

MERTZ:

Did he visit you in the Watson Lab?

GROSCH:

To the best of my knowledge I don't remember Tom, Jr. coming to the Watson Lab during the period that I was there. The old man made only one spectacular, formal visit, which is described in the Rogers book, in which I almost got fired for disagreeing with him about the decor of the lobby. But I can't remember Tom, Jr. coming independently. He was probably in that entourage, by the way. Of course I remember distinctly McPherson, and Douglas, and several other V. P.'s. But I suspect that Tom, Jr. may have been in that group, also. If so he doesn't stand out in the crowd in my memory. Probably in the old IBM Business Machines Weekly Newspaper there's probably a picture of the visit to the laboratory and one could count heads in it. But if so, I don't believe that I have it in my files. I've got pretty extensive scrap books and files and stuff, but I don't believe I have that. It may not have been published, I'm just not sure. Anyhow, the process was such that any major decision about starting a new machine, whether electronic or old fashioned, any major decision about hiring a new class of personnel and most of the senior people within that class would certainly pass through Watson, Sr. 's hands.

MERTZ:

Would you have any direct, or indirect knowledge of the role, if any, that was played Tom Watson, Jr. with regard to the decision by his father to go into high speed electronic computing...

GROSCH:

Second hand information, which I think is not really good enough for your purposes. The sort of thing that one engineer says to another, or one scientific or applications guy says to another - somewhere down the road some person was present at, or heard a discussion indicating that, you know, Jr. was pushing the old man to produce so much money, and so forth. And then, in probably garbled form, it gets passed down among the other technical people. I was present at two or three of the turning point decisions in IBM's entry into the field, and can talk about them, first hand, but neither of them was the big one which was essentially the one to start the so-called Defense Calculator Project. Gossip was, that Watson, Jr. had persuaded his father to put up $24 million to get that going. And that was started at the Kenyon Estate. Ralph Palmer was in charge of it; Nat Rochester, Werner Buchholz, John Murphy, Bill McClellan, I, myself, and many other presently well-known people in the field were involved in that. And I had no doubt,

whatsoever, that figure was correct and that it was Watson, Jr. that pushed it. But I was not present at the conference when it was done.

MERTZ:

Well, in your conference...

GROSCH:

For instance, I think I probably got that from Rochester who probably was present, but I wasn't.

MERTZ:

In your own contacts with Watson, Jr. would you say that he - that his interest or enthusiasm for it was reflected through your personal contact? Or would that be difficult to...

GROSCH:

I didn't get any feeling that he was mad for this sort of thing, no. The feeling that you got in dealing with all of the senior IBM people, with the exception, perhaps, of John McPherson was that they regarded the large computer as just one of many data processing tools, that it cost more to develop one than it did to improve the key punch and you'd want to sell more key punches. On the other hand I want to make it clear that you didn't get the feeling of dullness, or lack of vision, or anything like that. It was simply that these guys were all salesmen. And in the end it was that salesmanship, that orientation toward sales that saved them - not hurt them, but saved them. Because they didn't...

MERTZ:

I was speaking particularly of Watson, Jr. now.

GROSCH:

Yeah, but Watson, Jr. was a salesman. It's true that by this time he'd been given fancier titles, and all that, but he'd been through sales school and he made lots and lots of calls on customers in company with district sales managers, and local sales managers, and even individual salesmen. And, for instance, I seem to remember that after the 604 passed at Oak Ridge when Red Dunwall(?) and I went down there to find out why the first machine didn't work and a few things like that, I seem to remember that Tom Jr. either was in on the decision process on that, or maybe even made a trip down there himself. I seem to remember somebody saying that he went down there later on, probably with Red Dunwall to see, you know, if indeed we did have to re-tube the whole machine, or if

having done so it worked, and things of this sort. So you see him at a - in a typical sales posture, with a great deal more interest, of course, in large scale problems. Not just, you know, whether we could sell an extra key punch to such and such an insurance company, but much more high level than that. But only regarding, I think, in those days the computer as being one of several things that could be built.

For instance, NORC, certainly - I remember distinctly when I was invited to the NORC dedication ceremonies which were held at the Columbia Faculty Club, and which, as I remember, Johnny Von Neumann was the most honored guest - I remember coming back from that and writing a letter to either Tom, Jr. or to Cusper(?) with a copy to Tom, Jr. - I seem to remember it was to Tom, Jr. - in which I specifically said that the General Electric Company would be very much interested in buying a commercial…with NORC, and that we'd like to have a series of meetings to discuss the possibilities of this, and so on. And, practically speaking, not getting an answer because in those days the sales judgment was that there was no real market for this thing. We loose money on every one, why should we build more?

MERTZ:

And what day was this, do you recall?

GROSCH:

I would guess this was 1952, but I'm not really certain. The NORC dedication is a known date, so to speak, and I do have clippings and things on that. And the letter, which I still have a copy.

[End of Side Tape 9, Side 1]

[Start Tape 9, Side 2]

GROSCH:

Now, I think that the major point that all this indicates is that the lack of budget, and the lack of formal organization didn't really matter as long as the old boy was able to make these sharp decisions. He didn't have, though, anybody like Manny Poorie(?) on board yet to help him with the heavily scientific stuff, and increasingly Wallace Eckert wanted to go back to being an astronomer. He didn't want to leave IBM, of course, but he wanted to use the IBM Watson computational facilities to do his celestial mechanics research, to cooperate more with his old friends like Clements of the Naval Observatory and Bower of the Yale Celestial Mechanics and Observatory Group, and use his small entourage of people who were close to him at the Watson Lab to help with this. Specifically, he had an assistant named Rebecca Jones, now married I believe, who helped him write this book about Minort(?) Calculator called, wasn't it Faster, Faster. And Becky was engaged, almost entirely on doing astronomical work for him and pitched into things like helping

him write this book just to, you might say, keep IBM happy. I don't want to sound unpleasant about this, because, I mean this was the man's scientific career, and it was what he wanted to do with the IBM facilities in the same way that Hellus(?) Thomas, for instance, wanted to do the work in advanced numerical analysis and partial differential equations, and so forth. And Watson, Sr., of course, was happy to have Eckert do this, but it meant that Eckert was not pushing for the opportunity to help the old man make scientific decisions.

MERTZ:

How about McPherson?

GROSCH:

McPherson was always available to do this. His loyalties were entirely to IBM, and I think he had very little intention to write any papers, or do any research or development work of his own. He was neither a laboratory or a paper writing type, but because he was always close to the sales organization I think that he essentially responded to day to day pressures rather than to the kind of long-range thinking that Eckert could have done if he had been more interested in computer development and less interested in celestial mechanics. It's not clear, you know, that a thousand years from now a new idea in celestial mechanics wouldn't be worth more than a new idea in computer development. But it doesn't appear at this distance that it would have been. And certainly at that time I was quite disgusted with Eckert's lost opportunities. And it was, indeed, one of the reasons I decided to leave the Watson Lab in 1950.

I should back off for a moment and describe one little story about McPherson and the decision process that I think would be interesting. After the 604 became a mass production item there were people in the aerospace industry, then called I guess the airplane industry, that were interested in having more powerful equipment. Now, Bill Bell, for instance, who had split off from Lockheed to start what was then called the telecomputing corporation, and later was merged with Whitaker-Gerald(?), but retained the name Telecomputer Corporation, although Whitaker was larger, and then split off, the last elements of it being the Melonics(?) Division of Litton Industries at the present time. Well, Bill Bell split off from Lockheed to start what we would now call a technical service bureau in the 1948--1947--1948 era using standard IBM machines, and anything better that he could get his hands on. They were also designing and building their own input output devices mostly analog to digital, or digital to analog in form. And these came on the market in the late '40's. They were used extensively in the missile business, and the aircraft industry, and to a lesser extent in the atomic energy laboratories and factories. Bill Bell, was primarily as I was interested in operating the computing lab part of the business, not in designing the day to day converters, and so forth. Well, now Bill was interested very much in fancying up the use of the IBM machines, and his name appears in the literature in the early days as having contributed small tricks to making standard IBM machines do odd things. For instance, it was possible to load up the front

of the 604 plug board with all sorts of weird wires, some of which required the insertion of germanium diodes as one way devices - back circular eliminators. Well, IBM for some reason or another didn't want their customers to do this on the front of the plug boards. In later days they put such back circuit eliminators inside the computer or tabulator, or gang punch, or what have you and allowed you to wire such things on the face of the plug board with ordinary wires. But they certainly didn't like the customers to string any odd looking gadgetry on the outside. And remember these machines were all IBM's property in those days. This was before they were required to sell them.

Bill did this sort of thing, described the plug boards that he'd used which were much more complicated than anything I was able to make work at the Watson, and used them as commercial profit in his corporation. Well, while people like that were pushing forth others who were fully employed by a single company as he had been at Lockheed were trying to do the same sort of thing by wiring back and forth between IBM machines, using unusually long plug wires, and in some cases even putting amplifying elements, and in many cases putting these germanium diodes in the circuits. And a notable outfit that was doing this was Northrop Aircraft. There were three men at Northrop, one named Bill Woodbury, who later came to work for IBM; one named Rex Rice, who came to IBM and left again, and is now a fairly well known but not exactly prominent west coast computer designer, and so forth, with a smaller company.

MERTZ:

Rice is R-I-C-E?

GROSCH:

R-E-I-S-S, I believe in this case. I sort of lost track of Reiss, but he's still in the business somewhere. And another one was a chap named George Fend, who was sort of senior of the three at the time. He's still in the missile business, but not in the computer business anymore, and left Northrop some time ago. These three men came to McPherson's office, oh, I would guess in 19- probably 1950 or late '49. And it might have even been a little earlier than that, and described a way in which they had connected a 603 and an IBM 405 tabulator by wires running back and forth across the floor, and - I think it was the 603, I'm a little vague about this, now. And asked McPherson to have this engineered into a formal IBM product which would be serviced by the IBM engineers, and which would have an operators manual, and normal blueprints, and so forth. I was present at that interview because I had a great deal to do with the Northrop people and the airplane community, in general on the west coast. And McPherson refused point blank to do this on the grounds that they were essentially monkeying with standard IBM machine. They had no rights to do this, the contract specifically prohibited this, and they might hurt the machine, and etc., etc., etc. And Fend and Company produced a letter from Jack Northrop to Watson, Sr. saying, "Dear Tom, will you please have your people get to work on this right away, and we need it for the defense of the United States, and blah, blah, blah, blah", and threatened to take this up and plop it down on Watson, Sr.'s desk, after all it

was addressed to him, if McPherson didn't get off his duff. Well, of course, McPherson was forced in ten minutes to completely reverse his field and assure them that he would certainly do everything he could to see that the engineering department considered the possibility, and look into whether it could be done or not. And in less than six months the first deliveries began on the card program calculator, which is what it essentially was. Now, it's true that the card program calculator was a 402 tabulator, a fancier tabulator hitched to a 604, the then production, more elaborate machine. The fact remains that it was possible for IBM, as it had been in the SSEC case, once the decision was made or forced on them to produce an entirely new machine in some small quantities - by small I mean they made only twenty or so in the first model 1CPC, but they got them running down the line fast, and shoved them out the door. And, some-how or another, there was a customer engineer at each customer location who was able to maintain it and see that it did what it was supposed to do. The customers did the figuring out of what to do with it. There was no application studies made in advance, or anything like that. That machine was followed by an improved version called the Model 2 Card Program Calculator, which was made in quite considerable quantities - more than a hundred, certainly, were made, and it might have been a great deal more than that. That had a much larger plug board on the electronic box, and a much more sophisticated tabulator. And I had the prototype of that at the IBM Laboratory that I had begun in Washington in 1951. I had the very first model of that. But I think that the Model 1 CPC, the genesis of which this Northrop Aircraft request that I just described probably dates back, as I say, to about '49 or"50, something like that. That was the one that came out fast. After that the normal processes of development slowed up again, and it took a while to get the Model 2 out. The model 1 came out with a great rush, and Bill Bell got one of those at Telecomputing, and North American got one, and so forth. And it became quite a common article of commerce in the trade.

MERTZ:

CPC 2?

GROSCH:

CPC 1 - the very first one. Then they all screamed for something better, and that's where the CPC 2 came along. IBM didn't want to make more than a couple of dozen of the 1's because they were sort of cobbled together.

MERTZ:

You mentioned that there was this gap in the leadership of IBM so far as scientific development at the appropriate managerial decision making level at this time, which perhaps could account for the slow involvement by IBM...(inaudible)...This gap was eventually filled, who do you feel is the first person to fill that gap?

GROSCH:

Eckert was certainly the first person to realize that it existed. In spite of his concern with celestial mechanics and all that, he was well aware of this. He didn't want to, so to speak, fill it himself - I mean in the first place he wasn't broad enough, and in the second place he had his own interests. But he was aware of the gap and wanted the company to fill it very badly.

MERTZ:

Well, he did bring this to the attention of the...

GROSCH:

Oh yes. And he had, you see, he had Watson's ear, so that if there if there was sales resistance I would not, for a moment believe that there'd been any resistance from McPherson, but there certainly would have been resistance from the more conservative vice presidents, like Ned Douglas, for instance and others, Charley Kick before his death, and so on, who simply regarded this as another way that people were pumping money out of the old man along with Metropolitan Opera, the Metropolitan Museum of Art, Syracuse University…and what have you. So they would resist these things almost automatically. McPherson, I'm sure, did not resist them, but it was nevertheless, I'm sure, Eckert's direct pressures, and his direct access to the old man that got this to the Old man's attention.

Well now, specifically, for instance, very early in the game, like around 1947 or '48 IBM signed a contract with Johnny Von Neumann for $5,000 a year for his consulting services. Now this, as I remember, was something on the order of one week or less a year. In other words it was an enormous sum of money for a day. McPherson operated at a level where I didn't normally penetrate. He would talk, you know, only to Watson, Sr., to John McPherson, and Eckert maybe in a little private room somewhere. And I saw him at scientific meetings and at early meetings of the ACM, and so forth, rather than in the IBM conclaves. But the fact remains that he did talk a little bit to IBM senior people at a time when the - when most of them didn't recognize there even was a gap - or that there was an enormous development coming down the road. You've got to remember that the time that we're talking about there was not yet a storage program computer working anywhere in the world. The SSSEC was not storage program, remember.

MERTZ:

Did this also apply to Tom Watson, Jr. ?

GROSCH:

I'm sure Watson, Jr. was in on this. Yes, I'm sure he was.

MERTZ:

...that there was a lack of awareness of the missing technical expertise...

GROSCH:

I think that the gap was brought to his attention by the Wallace Eckert's, and the John McPherson's, but that he adopted it without much argument. And then, in turn, as...

MERTZ:

...(inaudible)...

GROSCH:

I think that he did not convince the old man until later. I think that Watson, Sr. didn't get the message, and probably got it from Watson, Jr. and to a lesser extent from T. B. Learson(?) who was still pretty far down in the echelon at that time. But I think he got it - some directly from Eckert, and some from Eckert and McPherson through Watson, Jr. But I don't believe he got that message until 1950...(inaudible)...'50 or '51.

MERTZ:

Notwithstanding...(inaudible)...

GROSCH:

Watson, Jr. certainly got in earlier. I think that's right. I think that he would see Von Neumann and he would say, "Isn't it wonderful to have conversations with such a tremendous scientist, and he would get - he'd refer to it in his family talks to the rest of the – to the rest of the IBM employees and all that, but he wasn't prepared to back it up with $10 or $20 or $50 million dollars until later on. Now that doesn't mean that Von Neumann, for instance, supported the NORC project, but the wouldn't have been an important artifact in deciding to build the NORC, which I'm sure they lost money on, there's no question about it. They got a contract, but I'm sure that it wasn't anywhere near enough to cover even the direct manufacturing cost of the machine, let alone…prior research and development. But the fact remains that the process worked reasonably well, and that the enthusiasm that the process worked reasonably well, and that the enthusiasm of Jim Rand for buying Eckert and Mauchly, and so forth, which was essentially, in my view, I'm speaking as an outsider, but my guess would be that it was essentially an unreasoning enthusiasm - that Rand didn't know what he was buying anymore than Watson knew what he was refusing. Much less than Watson knew what he was refusing - I take that back - much less than Watson knew what he was refusing. My guess is that the slow acceleration rate of Remington Rand after it acquired Eckert and Mauchly made it

possible for IBM to still lag and then catch up and pass them as if they were standing still.

MERTZ:

Well, then who - you mentioned two individuals who might be forerunners to this role of scientific adviser to Watson, Sr., that would be Von Neumann and Eckert who would, in terms of your hierarchy of IBM itself, administratively speaking, be one of the early...

GROSCH:

I would think that almost all the rest of them would be negative...

MERTZ:

...(inaudible)...

GROSCH:

All the rest of them would be negative, I think. Wally McDowell, who's name is now connected with an ACM or Aphips(?) Award, was a sort of a senior engineer during this period. He ran the Endicott Laboratories for a while, and he was vice president after a while - quite a while after McPherson. McDowell was a technical man. I'm not sure that he was a successful salesman, but he'd been in the sales force like McPherson had. And he was probably in favor of it as soon as he decided that the old man wasn't against it. But I wouldn't think of him as a stimulant to the old man's decision. He wanted to be sure that the old man wanted to do it.

MERTZ:

You touched on...

GROSCH:

Peoria didn't come on board until a long time after this. When Manny joined them it must have been '54 or '55 or something like that.

MERTZ:

How about Irving(?), was...

GROSCH:

No, there wasn't really anybody else, not really. Now you've got to remember that there were many very expert technical men in IBM who were trying to influence the old man.

This included Pete Ruen, for instance, Ralph Palmer, Hamilton - the man who built the SSEC, and to a lesser extent some of the older engineers who were, you know, back in the relay and tabulating machines era, but nevertheless, in favor of technical advance, and so forth. All of these were positive influences. All of them were trying to push IBM ahead in this field. But, you know, like me they didn't get to see the old man everyday, or anything like that. I think that Eckert and McPherson were more important, and that the people that Eckert, in turn, introduced the old man to on sort of a semi-social basis like I. Robbey at Columbia, and other Columbia scientists, and so forth - remember the old man was a Trustee, perhaps the most important Trustee at Columbia during this period, and, of course, the Von Neumann thing that I mentioned. Those were the major influences.

MERTZ:

...(inaudible, both men talking at once)...

GROSCH:

...plus, you know the competitive thing...(inaudible)...fortune magazine, you know, that something was going on. That would stir him all up, you know.

MERTZ:

How about Harvard?

GROSCH:

I really don't know, but my impression was - perhaps I'm too pessimistic about this, but my impression was that after the Aiken, Watson arguments about the Mark I that Harvard was, essentially, nonexistent to the old man for many years to come. Ted Brown of the Harvard faculty was on the Columbia - was on the Watson Lab Board of Advisors, but to the best of my knowledge that board never met, so - if it did it met for an hour or two once and disbanded and never came together again.

One possible influence that I'm not able to assess because I had no contact with it directly would be the old line Columbia relationship that Watson, Sr. had established back in the '20's and early '30's. Specifically, the old boy that - for whom they built the first statistical tabulator, - curious blank now, I can't get...

MERTZ:

Well, this is not in the Psychology Department...

GROSCH:

Yeah, sort of Psychology Department. This is somebody I know very well.

MERTZ:

You're not thinking of Abraham Waller or anything, are you?

GROSCH:

No, this is pre Waller. Just - (tape stopped)

Yes, Dan Wood, who was concerned with the - with Teachers College at Columbia back in the late '20's and through the '30's. Watson, Sr. had direct contacts with people like that. I think that they were even direct in the sense that they didn't involve Eckert. They simply were early Columbia contacts that he had when he was first interested in Columbia. And I have no doubt that those were also positive influences. But you see Dan Wood, and people like that weren't aware of the modern pulse technology radar kind of electronics at all. So they'd been just generalized influences saying yes, you know, you ought to be in this computer business - statistics, census work, demographic calculations, educational research needs it, but they wouldn't have the specifics to say you ought to back Havens, or you ought to back Rochester, or you ought to back old fashioned Halsey-Dickenson, or something like that. They wouldn't even know about that.

MERTZ:

Well, at various times there might have been another influence through the insurance world's...(inaudible)...computers...

GROSCH:

That would filter up to the sales department, though. And it's not at all clear that the sales department didn't filter out a good deal of this on the way up. Again, I'm now aware of this. But, certainly, when we had our early get-togethers at Endicott in which the - many of the influential people on the technical computing side came - in fact almost all of them at one time or another - there was almost no representation from the business community, whatsoever. It just didn't seem to be brought. And invitations to that meetings were distributed by high level sales people all over the country to people that they thought would be interested. And, in fact, it turned out to be almost entirely airplane, airplane engine, and nuclear physics guys...

MERTZ:

I was going to ask you, Sir, wasn't some conscious effort made at one point to involve both real business, and more academic...(inaudible)...applications problems through this series of symposiums, or colloquia, which involved other IBM offices...

GROSCH:

Yeah, well I arranged the - I arranged the details of the first one, and edited the proceedings for it. That was 1948. There had been an earlier one in 1946, which Eckert sort of gummed up with the help of a native from Endicott, NY, and at which I gave a paper. Now, that 1946 one, which is not in the literature, - I have a copy of the proceedings, but it's not mentioned in the literature practically anywhere. That 1946 one was oriented toward educational research and statistics. By the time '48 came along there was already the strong bid toward the airplane companies and atomic energy. And while we invited people who had been involved in that first '46 meeting, practically none of them came and they tended to steer off into other colloquia that IBM salesmen were sponsoring at which there was a recital of the power of the new 603 and the new 604, the new card program calculator, but no force from the customer to get it. In other words, these were people that had to be sold - the people that came to the colloquia that you and I are talking about now were beating on the doors to get the stuff. They wanted faster delivery, more complications, higher priced. So it really stood at that point, that they were still trying to sell the business community, while the technical community was begging them for the equipment - down on their knees, just practically banging their heads on the ground. Now, from that first 1948 meeting, and remember the ACM was already in existence by them - the ACM came into existence by then - the ACM came into existence in '47 - the small community of 100 people that were interested in using any kind of big machine, any kind of machine, in fact, any kind of electrical or electro-mechanical or electronic - IBM or Remington Rand, or relay calculator, or Bell Relay Calculator, or analog machine - anything they could get their hands of for large scale computing. These people had already begun to realize that while there was plenty of competition that most of the available stuff came from IBM. They could look around at their…and they found that, you know, eighty of ninety percent of them were involved with IBM and the rest were sort of hanging on to one of the kind of machines, and so on. For instance, at the '48 meeting we had two people from the Bureau of Standards where this record's being made today - Gertrude Blanche, and I think it was Edward Yowell. Well, now the Bureau of Standards was consulting for census and others in the orders for the first UNIVAC - the first UNIVAC I's, then called the Eckert-Mauchly machines, or whatever they were. And they were building their own SEAC, which was to be the first actual operating American stored program machine, starting about 1950 - operating in about 1950. Nevertheless, Blanche came with great interest to out 1948 seminar because the Bureau of Standards was still using punch card machines to do the math tables calculations that it was doing while it was waiting for the fancy machines to be delivered. So we had an intersection with virtually all of the technical computer people. And there were a few others - fifty or so that came to that meeting. Most of whom have now disappeared, but a few who are still in the art.

Now, that was followed by a meeting in '49. By this time Cusper had come on board and he'd sort of taken the game away from me. And two meetings in 1950 at which, for instance, Von Neumann came because by this time he had some obligations to ward IBM because of his consulting arrangement, and so forth. And after that they sort of petered out. By that time the professional organizations were able to take over most of the

necessities, and the meetings were being held all over the country. Both the east and the west coast joint computer conferences were in the process of starting, and so forth. But for a period of time invitations to those four or five IBM symposiums were extremely highly valued, not only by IBM customers, but by outsiders. And the publications that came from them had some interesting stuff in them. I just got a letter the other day from a chap in some remote corner of the earth asking me about the publication that I had in the '48 one, which is still of interest to people doing approximation theory ...(inaudible).

MERTZ:

Then, to get back to NORC and its personnel, you mentioned three individuals who'd come on board first and their move to...(inaudible)...

GROSCH:

Umm hmmmm.

MERTZ:

Uh...

GROSCH:

Havens only moved around the corner, Lentz and Walker remained in the other building.

MERTZ:

Remained...I see.

GROSCH:

This was only Haven's shop, and was, theoretically, only to build NORC. (Tape turned off)

Havens had had to build up a fairly substantial group in order to do this. I think that he must have had somewhere around twenty people working for him by this time, and - whereas Lentz had maybe one or two, including a young man named Dan Mason, who is still marginally in the business at the present time, and Walker had one or two, mostly more senior people helping with his analog computer projects and stuff. But Havens had built up to about twenty of his own, and they moved out to the Broadway location. And I can't remember the names of most of those people, now. There were obviously several senior people - several young but professional engineers, as well as technicians, and I'm sure their names are in the dedication material for the NORC, which I have in my files, but which I don't have right in my hand at the moment. And that group finally broke up shortly after the NORC was delivered to Daldrin(?). Havens swore on a stack of bibles

that he would never, under any circumstances, move into the main IBM engineering and development activities. Partly, I believe, because he wanted to-really wanted to be more university connected. He really regretted not being more deeply involved with Columbia. And partly, I believe, because he felt that there was an inside track in Endicott in Poughkeepsie, and he was not on it. Well, in fact, after the NORC was delivered and was running, and everybody was happy with it, he worked for a while on a Columbia University Project in the physics department - I believe it was the cesium cloth, or one of these half nuclear physics, half electronics projects, but shortly thereafter sort of disappeared from that kind of activity, and the next I heard, as I say, he was Director of Engineering for World Trade. Very happily espoused in Europe from which I understand from Watson, Jr. himself, he refused to return several times. Tom told me just a couple of years ago that - in a laughing way - that he tried a couple of times to have Havens to come back, and Havens essentially refused, which is not something one does very easily with any of the Watson's. So he's obviously very happy over there.

Now, I can dig up more material on the NORC, which was in its day a very spectacular machine, but one off, of course. It was - there was my attempt to get them to build others for G.E., and so forth, all fell through. Cusper I think, had another request I think from something like probably Data Taylor(?), in lieu of your remarks about them being another government requester, but these were never put together into a formal offer, at least for General Electric.

I might now, in talking about hardware, back off and go in another direction, and that is that is the beginning of the 700 series machines, because I was slightly involved in that before I went down to Washington, and I'll come back in a later interview, probably, to this question of how I went to Washington and what I did there, and how I left IBM for MIT.

The 701 group ensconced in the Kenyon estate, was building a thing first called the Tape Processing Machine, TPM. And this turned out to be the prototype of the IBM 702, which was a business machine, character by character, decimal and alphabetic, intended almost entirely for information for insurance companies, and so forth. We were saying that they weren't pushing for equipment. This is true, but at the same time they were being sold by people like UNIVAC, for instance, and were placing orders - Metropolitan and Prudential were placing orders for such equipment. IBM was conscious of this and was attempting to sell them also. And behind the scenes they were working on this Tape Processing Machine. Now you must remember that at this time there was no really workable high-speed storage. Cores weren't working yet. The Williams Tube sort of idea - the storage on the face of the cathode ray tube was being talked about I guess in the laboratories, but I don't believe it had reached my attention at that time. I hadn't been to Manchester yet, in fact I hadn't been out of the United States at this - hadn't been to Europe at all at this time, but I'd had a considerable influx of friends from overseas. People with whom I became friends from overseas, and I was getting word from Manchester and so forth in that vein, but I think Williams Tubes were not yet working at Manchester. And the attempts in this country by Yon Riechland, and others to make

discreet storage elements within the vacuum envelope, so called selection, which was to go into the RCA equipment at a slightly later date, were only just beginning, in fact weren't working very well. I was aware of this, and there were papers being given on it occasionally, but it was also known that it wasn't working too wall. So most of the ideas still revolved around the mercury delay lines, and things of this sort.

Well, also you had the business of the large scale input equipment. Remember we has used seventy-eight column wide paper tape in the SSEC. In the later IBM machines like the Card Program Calculator, it reverted to card storage - individual card storage - both for program material and for data. Mauchly in particular – was planning to use coated metal tape; and finally, of course, the idea of using coated plastic tape which is now so universally accepted, was working fine in audio equipment and was beginning to be used in computers, or plans for computers. Hence, the term tape in tape processing machines. The...(inaudible - both men talking at once)...

MERTZ:

GROSCH:

Yes, well they did have a drum at that time, plans for the Tape Processing Machine, as I remember it - Now, I was not completely conversant with that plan. Ralph Palmer and John McPherson would be better sources of information on that, and to a lesser extend Jerry Haddad, but I believe - and Nat Rochester - I believe that they did not plan the drum for the prototype 702. I think it was to be a tape machine with essentially six or four type electronic circuitry for the arithmetic unit, and so forth. But it was to be storage programs, no question about that. And it was planned that this would, you know, be deliverable as of '52, '53 era. But there wasn't much money behind it. It was being run on a typical, we'll ask Mr. Watson for a million dollars, and run on that until it's gone, and then ask him for another million basis. It was almost universally used in those days. There was no - to the best of my knowledge there was no major program set up with a plan to produce twenty of these machines and sell them to these twenty people, and so forth. It was all a laboratory development sort of thing. Now, with the threat from UNIVAC, and the fact that UNIVAC had firm orders from the Pentagon for the number two machine, and so forth, and of course the pressures of international affairs - the Korean War was heating up at this time, and so forth - there began to be a feeling that there should be an IBM machine that would contribute to the national welfare, and that would, not incidentally, also cut off some of these UNIVAC orders. And the term "defense calculator" was originated for this. Defense calculator was just the scientific version of the Tape Processing Machine. It was to built under crash, crash conditions, and they were to make a substantial number of them so as to satisfy the country's needs for such a machine for, you know, the foreseeable future. And Tom, Jr., apparently was the key person in this, although I was not present in the actual conferences. The gossip was that he got twenty-four million dollars from Watson, Sr. to get this program going, which was, of course, distributed among the outfits that were going to manufactures the

machine; partly Endicott and partly Poughkeepsie, and then, of course, used to fund development work by Palmer, Rochester, Haddad and company in the Kenyon estate.

Now at the time I encountered this I was ready to leave the Watson Lab, ready to go to Washington to start a technical computing bureau for IBM there to use a prototype Model 2 Card Program Calculator, but I was not able to get delivery on this prototype machine from Endicott where it was being built because of the slowness of the development, and the fact that a lot of the good people had been put off on Hamilton 650 development and Palmer's 701 or Defense Calculator development. So while I was sort of out of the Watson Lab, but not yet all the way into Washington I spent considerable time - I would guess on the order of eight or ten almost full weeks - at the Kenyon Estate as sort of a - as a sort of a imitation customer, sort of a synthetic customer.

MERTZ:

This was in 1950?

GROSCH:

This would be the spring of '51. And I can substantiate that to the day. I don't have the figures right in front of me, but that would be the spring of '51. I stayed mostly at a bum old hotel in downtown Poughkeepsie, which I remember had, I think it was called the Nelson Hotel, and had a bar called the Regada Room with swizzle sticks made up like the cars of Poughkeepsie at a boat race, and I would work with the people I've already mentioned - Rochester, and Buchholz, and so forth, during the week. Bill McClellan from Cusper Herd's group was there, and then we would all go away for the weekend, and come back again.

MERTZ:

What was the nature of your...

GROSCH:

The most of the group that I have mentioned were concerned with that we would now call programming. The word had hardly been invented yet, but they were essentially drawing up the stored programs for the Defense Calculator...

MERTZ:

Compilers?

GROSCH:

We didn't have compilers yet. The major thing that they

were doing, other than just, you know, make sure that the machine added "A" and "B" and got "A plus B", the main thing they were doing was to do some simple relocation type programs so that you could program - you didn't have to program an absolute. You could assign a number - an arbitrary number to your storage position, and then later on go back and relocate the program in a different part of memory without changing all the numbers yourself. And since the right hand side of the single instruction order sometimes contained other than addresses, like you could give the numbers of shifts in the shift construction, the number of positions to be shifted in the shift construction. There had to be some sophistication in this. You had to look, first, at what the instruction was, and then decide whether that was a location next to it, if it was relocatable, or whether it was a sign of a shift or something else. So, it was by our present standards, of course, trivial. The sort of thing a good programmer will do in an afternoon, now, but it was not that trivial then, especially if you didn't have any prior software to help you do it. You had to write your first programs in absolute, and sort of bootstrap yourself up into this more sophisticated level. Well, by the time I got there Rochester and some of his friends had most of that going, and I tried to use if to do, for instance, double precision arithmetic, and...polynomial approximations and stuff. Now with the intention of doing anything that would be really useable to the customers, but to simulate a customer so that I could see how their relocating instructions, and so forth, would work. Now, there was no 701 to do this. There was a sort of a simulation on it which was built up out of an old 604 with some additional stuff on it. And among the additional stuff was the first Williams Tube storage that I had seen. They had bought the rights - IBM had bought the rights to the Williams cathode ray tube storage from Manchester, or from Williams, I don't know who was the actual owner of the patent, and had imported enough of the know how to build machines of their own at this time. And it was a pretty coarse and there were lots of other things wrong with it, but it was sufficient to give them some feelings for the production machine which was being designed and built in parallel for the 701.

So, I had a chance there to see magnetic tapes working, Williams tube storage working, what we would now call some rather primitive software being written, and wording; and in turn I was supplying some sample problems and working with the people, and so forth. So, I knew at that time, perhaps more than any person outside of IBM-Poughkeepsie about what was going on. Bill McClellan, working for Cusper Herd was another possibility, because he was taking this message back to New York, as I was taking it back to Washington so to speak. And Cusper Herd who was then running the Applied Science Department counted as his specialized customers - of course there was a regular salesman for each one of these accounts, also, but counted as his specialized customers almost all of the people that had ordered these Defense Calculators.

Now, an interesting artifact about this was that they had orders for most of these machines before they really ever started assembling the first one. I don't believe they had very many orders when I was there in early '51, but they certainly had some. I wasn't aware - wasn't told how many, but they certainly had some. And by the time the first machines were delivered, about a year later, they had orders for, I would guess, anyhow twelve, maybe fifteen. And, in fact, they had orders for all they planned to make right off

the bat, but they finally stretched the order to nineteen or twenty. And the last two or three, you know, came in after they delivered the first fifteen.

MERTZ:

Did - who was instrumental in...(inaudible)...

GROSCH:

Well, I guess I was in the sense that I got to racketing around behind the scenes to the point that I was offered the chance to go to Washington or be fired. What had happened was that I was dissatisfied with the milieu at the Watson Lab. It was clear that Eckert was turning away from the advanced computation except to the extent that he needed it for celestial mechanics. He was not interested in interfering very much in the development of the new machines that was going on at Poughkeepsie, and to a lesser extent Endicott without him. He was always willing to have me and other members of the staff consult, or advise, or work with these people, of course, but he didn't want us to initiate any major project. And I had decided, as a personal decision, not the fault of IBM or anything, that I'd be happier in management than in research. Because I found that while I had really very wonderful facilities at the Watson Lab to do research, to do automatic lens design, and celestial mechanics, and all these other topics that I was interested in, I found I was ...when I was actually talking to customers, or recipient customers about their problems. I was much happier talking to Bill Bell about his plans or Hans Craft(?) from the General Electric Company, or Jack Strom(?) from North American than I was in - than I was talking to my own subordinates about putting a problem of my own on the machine. My wife worked with me a good deal during those days and she would frequently, you know, harass me for instructions on what next to do on my research program that night, while I was extensively talking to some General Electric type about whether or not he should order a Defense Calculator. So it was clear that my natural bent was in the direction of management, and so forth, rather than in the direction of doing work, myself. And I decided to go along with this. I had been teaching courses at Columbia during all this time, five one semester courses in numerical analysis, and one semester course in elementary celestial mechanics. So I was having the opportunity to be part of the Columbia environment; enjoying it, but not enjoying it as much as this advisory and managerial sort of thing.

So, partly because of my disgust with --disgust is--dissatisfaction with Eckert's course, and partly because I wanted to get more into management and administration, I decided to leave the Watson Lab. Well, I tried, first of all, to do something elsewhere in IBM. I didn't want to leave IBM. I thought I had a life job there and enjoyed it very much, admired the - even enjoyed the rather peculiar Watson environment, but I wasn't able to get past Cusper Herd. He intended to run the whole applications thing if he possibly could, and intended to - well, as I say he'd already taken away the editorship and the management of these colloquia from me with no resistance on my behalf from Eckert or McPherson. And clearly would have been glad to have me join his group and work for

him, but I considered myself considerably more senior in the field than Cusper. And, moreover, many of the young fellows that were working for Herd, like McClellan, had been in my classes at Columbia, that was one of the places he recruited them from - so I had an itchy feeling about this. So I wasn't really able to hit something that looked good for the IBM thing, and I began casting around for other alternatives. And this was at the beginning of the time in which the idea of consoling in this area was beginning to take hold. Consoling, as you well know, often follows one's dissatisfaction with the present job, or expulsion from same. And enough people were getting expelled from failed computer projects - that is projects that should have worked but didn't, especially those that were follow on to the ENIAC - that there were beginning to be a substantial number of hardware consultants. It seemed to me that the success of Telecomputing as a computing facility on the west coast, and of a certain number of hardware consultants around the country indicated that there might be room for an applications consultant, also. So I began to look around for possible finds while still working full time for IBM. And I made a proposal to Mutual of New York - Mutual of New York was still in the punch card era, but it was - it had had some studies made on how it might use computer facilities at the time Prudential and Metropolitan were ordering the predecessor to the UNIVAC. Well, I went in and got to a vice president who was concerned with this, and made him a formal proposal, of which I believe I still have a copy, that sufficiently impressed him, although it was a pretty small amount of money - I think it was $50,000 for the investigation, including the services of some other people that I had in mind - sufficiently impressed him that he took it to the Board of Directors. But, you see, I was a very naive young man about management. I had been mostly an astronomer and an optical designer up to this time, and a numerical analyst, and celestial mechanics instructor, and so forth, and I hadn't done the homework that a person beginning to be a consultant should have done. Among the things I hadn't found out was that I hadn't looked up the Board of Directors and the executives in some such reference book. If I had I would have found that Watson, Jr. was a member of the Board of Directors. (Laughter). So, when they took it to the Board of Directors Watson, Jr. 's eyes popped out and he said, "Christ, this guy works for me". So the next morning there was a call from Gelackie(?) headquarters, and I went hopping down town to find Tom Jr. very irritated, indeed, at me. And he gave me twenty-four hours to decide whether I should do one of these IBM things that I'd been talking about, or get out of the Company. And I said, "Well, I have been attempting to find something else in IBM", and recited the two or three things that I had been interested in, but I said, "they all seem to be blocked off by Cusper Herd's desire to monopolize this area", I considered myself senior to Cusper and didn't want to work for him. "Well", says Tom, "why don't you think about this Washington thing. That wouldn't be under Herd, would it?" I said, "No, it would presumably report to the boss in Washington", and he said, "I'll call Red Lamont about it."

MERTZ:

This Washington thing that you're referring to was a proposal of yours?

GROSCH:

A proposal that I had made in verbal form only to McPherson and to several other people, but not to people - but not in Washington - to start a technical computing bureau for the - for Government business. Either in New York or in Washington.

MERTZ:

to McPherson the proposal...

GROSCH:

Uh, yes, and I think it was never in writing. I think it was always just conversational as IBM proposals tended to be in the early stages.

MERTZ:

Eckert

GROSCH:

Uh, I guess so. Wallace knew that I was unsatisfied and was trying to help me in his quiet way, but, you know, he didn't particularly want me to leave, so he wasn't just beating the bushes or anything. And he especially would not front for me with Herd, nor would McPherson. McPherson's view was, you know, we hired Herd to do this sort of thing, why do you insist in getting in - not working for him? And my answer was, "Why didn't you ask me to do it, instead of hiring Herd?" It was a little late by that time, because he'd been aboard a year and a half by then.

Anyhow, call it jealousy or what you will, I didn't want to do it, and the end result was that Watson arranged that I should go to Washington directly with Red Lamont, who was then the big shot in Washington. And they informed Herd, you know, at a later date, that I was going and that he should cooperate with me. The - Lamont was, at that time, a vice president of the company, and a very high level vice president, perhaps the third or fourth most important man in the company. And his particular job was to run the relationships of IBM with the Washington community, and of course, 99% of which was the Federal Government.

MERTZ:

When you had your interview with Tom Watson, Jr., this proposal, then, took on a written form?

GROSCH:

I don't believe there was ever a written form. No, that wasn't the IBM style. I then went down the Washington, needless to say, and talked with Lamont.

[End of Tape 9, Side 2]

[Start Tape 10, Side 1]

GROSCH:

Lamont was, of course, more than a little startled at my advent. All he had was a brisk call from Tom, Jr. that day before. But we worked out the details in an informal fashion. Under Lamont there was a man named Don Gambl -G-A-M-B-L- who was the local manager in Washington, and I was, ostensibly to report to Gambl, but since neither Gambl nor Lamont had any real idea what I was going to do I didn't get very exact - very detailed supervision. Still, someone had to sign the expense accounts, and authorize the changes in the building, and so forth, that I needed made. I had already been promised access to this prototype Model 2 Card Program Calculator, so I knew that that was available. That, of course, was, perhaps, the crux of the decision. If I couldn't get that there was no point in going down to Washington and running a 601 and 602. But, between Watson, Jr. and Lamont and myself, we were able to tie that down in the initial conversation. And It was decided that I would have some space in the new service bureau building that they had just started to remodel on 19th Street, here in Washington. This was under the management of a young fellow - of an older fellow named Scott who had just moved in from the old card plant - IBM used to make punch cards in town out on New York Avenue somewhere, and they used to have the Service Bureau in the basement, or out back somewhere there, and they moved it into town where it would be more accessible to Government business, and so forth. Scotty was running the shop in front, it was decided I should have some space in back which was not yet used up, and next thing I did, of course, was to plan the remodeling of that space-the first time I'd done it since the original Watson Lab days. Then I began to worry about the staffing, and so forth. I'd brought one young man named Stan Rothman down from the Watson Lab with me; one attractive young lady named Libby Linberg from - got married during the transitional period, as a matter of fact, from the SSEC group which was still going in the 590 Madison Avenue Building. An older man from Cusper Herd's group named John Mayhill, who had had a considerable experience at Convear(?), Fort Worth in aerospace business, whom I thought was convertible to FAA - or what we could now call FAA- I think it was then the Air Navigation Board, or something like that - activities that I had my eye on. I, myself, and then I was assigned a specialized customer engineer named Carl Suther(?), who just left IBM this last year after many years with them, who was a sort of pet of Lamont's, and who was an extraordinary capable, informal customer engineer. And since there were no blueprints, and no real information available in advance on the card program calculator, which was quite different from the Model 1, I needed a highly superior customer engineer in order to do this. And I had been warned of the importance of this by the way that Wallace Eckert success at the Naval Observatory back in 1941 had been due to a chap named Dick Denim(?), who had done all this

detailed work for him at the time. So Suther was that sort of person, and, in fact, he spent a good deal of time in Endicott helping them assemble this prototype machine, and in the end rewired the whole back of the plug board, single-handed, to conform to the production machine from the prototype, which was different. This sort of thing normally takes weeks to do, and to check out, and he did it over a long weekend. He was just that sort of person, you know, when he came to check it out, why, almost everything was perfect and he found the one or two. He was one of the few IBM customer engineers that was sloppy. He dressed poorly, and he always had a dirty old cigarette dropping ashes over everything including the machines he was working on. I took this as a favorable sign, since any customer engineer who acted this way at IBM would immediately be fired unless he was superlatively good. So that was the sheer proof that he was, as indeed he turned out to be. He joined the engineering force later, and as I say he worked for IBM until a year or so ago.

Well, Suther had to get the machine going, and it took a while, as I said, including this rewiring of the plug board. In order to get the machine I had to make certain agreements. One was that Stan Rothman, who was working with Suther, was writing, essentially, the test procedure and test manual for this machine. We were already at the stage where we had to have diagnostics to make sure whether such a machine would function correctly. And Stan agreed to do this. He was very capable at this sort of work, but wasn't good at describing what he did - what we now call documentation, was very difficult for him. He had a Masters Degree, but hadn't learned to read and write very well, so far. He's quite an important artifact in the computer field on the West Coast at the present time, and he worked for me, again, later on in my career, but those were the first two times he worked for me - at the Watson Lab, and the Technical Computing Bureau in Washington.

Well, when the machine was finally delivered, and Stan began to get this diagnostic plug board wired and described, Mayhill and I and Libby Linberg were out looking for business, and we did some work for Fanny Mae, the mortgage outfit, some business for the predecessor agency of the Air Navigation Board - predecessor agency of the FAA. And I think it's interesting that that was the simulation of two dimensional air traffic patterns. And to do this on a machine that had, as I remember, sixty-four words of storage, and that produced a multiplication according to the card cycle of the tabulator, which was probably - I'm guessing - but it was probably nine-hundred cards an hour - I'm sorry, nine-thousand cards an hour, indicates a certain paucity of equipment by present day standards. Well, you had to be clever instead. One reason I had worked in this approximation theory business was that if you wanted to get sine and cosine, and logarithms, and so forth out of such a thing, and you obviously needed sine and cosines for air traffic control work, you had to be able to approximate these functions by polynomials or something that a simple computer could evaluate without wasting very much storage. So I've become a world expert in squeezing down approximation polynomials to the minimum possible size.

MERTZ:

Was this in the summer of 1950?

GROSCH:

This would be in the summer of 1951. And while I was waiting for Suther to get the machine rewired, I was at the Kenyon Estate playing games with the 701, as I've already described on a previous tape.

MERTZ:

I see.

GROSCH:

(inaudible - both men talking at once)...

MERTZ:

you had moved and...

GROSCH:

Uh, during this time they were remodeling the space on 19th Street, and so forth. We were - the thing sort of went along together. Rothman was pretty much in Washington at first. Then he went up, during the final days, with Suther to help get that done. Mayhill moved down and stayed in Washington, and Libbey came later as her new marriage permitted, and so forth. It was an informal arrangement of only half a dozen people. But we were relatively happy. I had gotten permission to have a small library of books, for instance, the sort of thing that was hard to do - a small library of books, and little, minor Watson Lab kind of operation - a microcosm of what' I'd done some years before at the Watson Lab. And I was quite delighted with the opportunity, and so forth.

Well, around this time, the external forces began to work on me again. I think that probably the gang in New York realized that this was going to be a success when I got my second or third contract with this Air Navigation Board, so the next thing I knew, before he'd finished doing this test board, and documentation, why, Herd requisitioned Rothman to move back to New York and work in the Applied Science Department. Rothman didn't want to go, and I told Lamont that if he were requisitioned, why I just wouldn't be able to go on with what I was doing, and Lamont said, you know, that's part of working for IBM, you just have to do with what we give you. If we don't - if we choose to take away one of your men, that's just too bad. I said, "I'm sorry, I won't work under those conditions." So he fired me. And, of course, the whole group came apart. Rothman went to Rand Corporation, I believe it was - or Systems Development Corporation. Mayhill went back up to the Applied Science Department, and they struggled along for a couple of years trying to keep the place going without ever really

accomplishing very much. Then they closed it down, or rather they merged into a larger organization, which became feasible at that time. A big chap, who's name escapes me at the moment--George something--who had been in the Applied Science Department, tried to run it for a while after my disappearance. I had tried to recruit a chap named Sarahand

MERTZ:

Sarahand?

GROSCH:

I think that's right, from the Naval Research Laboratory, who had been involved in the construction of the One-Off(?) machine at MRL. And he's still, I think, with IBM and has been a very useful member of the computer field since that time. And I believe he took over directly from me, and then George what's-his-name from the Applied Science Department was the next one down the line. It turned out that without someone who had a relatively strong external relationship with the Government customers, this wasn't too practical a thing. In other words, you couldn't really send out the standard IBM salesman to beat the bushes and get very much business. And if he did get the business you weren't usually in a position to carry it out, because the type of people that could turn the big customer request into an actual simulation were pretty much these applied science guys, and they were in short supply.

Meanwhile, following this as a pattern, Herd opened similar branches in Los Angeles and other parts - this was, of course, now important to him after my departure. He opened other branches in other parts of the country and began to expand his applied science group by hiring people to work directly in those centers. So that a couple of years after this, why he had a couple of hundred people to...

MERTZ:

Roughly when was this crises thing?

GROSCH:

The crises was in November 1951.

MERTZ:

Oh, I see.

GROSCH:

...or I guess it was in October and they gave me a month's salary in lieu of notice or something. I, of course, by this time was very well wired in on the speech making circuit,

and in all the ACM and AIEE professional activities, and so forth. I had been - I'd been a charter member of the ACM in 1947, and I had been co-opted by the Electronic Computer Committee of the Electrical Engineering Society - not the IRE, but the Electrical Engineering Society, as I remember - no, I said it wrong, the IRE not the Electrical Engineering, the two societies were separate then.

MERTZ:

(inaudible)...

GROSCH:

Yeah. McPherson was a member of the AIEE, the Electrical Engineers, from his college days. And I had been co-opted by the Electronic Computer Committee of this Institute of Radio Engineers to work with them. Particularly to work with a chap from RCA who was constructing a bibliography of the early computer articles that were appearing in the literature. Because I had this library at the Watson Lab with a lot of substantial holdings. And one of my girls actually constructed what we would now call a uniturn(?) index on punch cards. I'd actually gotten the idea from an old book on - an old British book, which called it the peck-a-boo system. And we ran that for a couple of years for the IRE, and I was sort of an ex officio member of this committee in consequence. And then somebody discovered I wasn't a member and insisted that I become a member or quit, so I finally became a member. As a - in fact, became a senior member from the beginning. I never did go through the normal associate, or full membership category. So, I was well wired in with all these groups. And, among many others, I was aware of the work going on at MIT, originally an analog project converted, now, to a digital project called a Digital Computer Laboratory under J. Forrester. And, having at his disposal the original Whirlwind I calculator, and finding the Whirlwind II. Well, now there are a lot of interesting things about that machine. In the first place, it, too, was going to use an electrostatic storage. And they were building their own tubes to do it with. And, of course, behind the scenes, Forrester was building the first core memory to go as a substitute to the electrostatic tubes. And then, of course, they were heavily funded by the Air Force for this air traffic - air defense thing and I was working on air traffic control, you know, very much. Cruder and simpler fashion on the CPC. Finally, I'd had quite a few ideas in these days about the construction of machines from working with Havens, and Lentz, and so forth. This is all gone by the board, now, but in those days one could be both an applications guy, and a logical designer, and so forth. I didn't know enough about, you know, choosing which kind of tube to use or anything like that, but at the level of organization I was still interested in it.

So, in the end, among the several places I talked to I accepted an offer from J. Forrester to come up and be in charge of what he called logical design research - odd ball thinking about logical design, rather than a specific design of Whirlwind II, which, of course, became the sage computer. And I went up there, as I remember, on November 1, 1951 at essentially the same salary I left IBM at. They paid my moving expenses, and I became a

member of the Industrial Cooperation's Staff, or something like that, I forget what the phrase is, which paid a little better than the academic salaries that didn't have the power and the MIT structure that the academics had.

MERTZ:

What division...

GROSCH:

Division of Industrial Cooperation is right, DIC.

MERTZ:

(inaudible)...

GROSCH:

I rented a house in Belmont from a university professor...

(tape shut off)

The MIT thing was extremely pleasant. They were in the Barter Building still. There was the jolly old Whirlwind I buzzing away. Charlie Adams was in charge of the non-military use of it. And Jack Gilmore and Dave Israel, and a whole bunch of well known figures in the art nowadays were on the military side - the Air Defense side. Bob Everett was the number two man, and Bob and Jay and I were quite friendly. I used to object to the fact that they were essentially following a set of specifications that I regarded as already outmoded for the air defense problem, even though, you know, they hadn't even started to build the machine yet. My reason for this was that by '51 I was the national president of the American Rocket Society, and I was deeply involved with the Von Brown's and the Lawrence - and the Lovell Lawrence's, and so forth, who were building red skill(?) missiles, and jupiters, and so forth. And I was well aware of the fact that, what you might call plunging fire of intercontinental missiles was going to be the mode of attack of the United States long before they got this dumb air defense network. So I used to say this to Jay, and Everett, and to a lesser extent to Israel and so forth, at frequent intervals, and they would all pat me kindly on the head and say, "Now, we'll worry about this because, you know, we have to do what it says in the contract. Meanwhile, if you just think of something sexier, you know, that we can use for the Whirlwind III or IV or V, or whatever it is, why so much the better." So, I put my Rocket Society prejudices aside, and started working on, as I remember it, sine ternary(?) arithmetic - that is counting by threes instead of twos, and with the symbols a plus sign a zero, and a minus sign - I was led to this by the idea that there might be some forms of something like core storage, that using little pieces of…rather than sindred(?) magnetic material which might have three stable states. There might be a neutral state, and a polarized one. Actually, no such

material ever was used commercially, but there are some forms of Polaroid that exhibit this - permaloid(?) that exhibit this sort of thing.

So I was trying to work out a ternary arithmetic that would use this, and I was led by the idea of the two stages of polarization in the neutral state, the plus and minus, which I later discovered, much to my amazement, was in the ERA, high-speed computing devices book. I thought I had done it independently, but then I thought I had invented polynomials a few years before, too, so - disappointing. I finally did invent Grosch's Law about this period, so I remain one out of three on those three inventions of mine.

But, anyhow, this sine ternary arithmetic was interesting and I laid out some simple block diagrams of ways that one might do these simple arithmetic operations. It turned out that it made multiplication very simple, indeed, and rounding disappeared - the art of rounding disappeared, entirely, which is nice from the point of view of the numerical analyst. But had the disadvantage of putting the difficulties at once, practically, back into it in the division process. Well, you don't do division very often, so what I was looking for was a machine that would work very rapidly, indeed, in addition, subtraction and multiplication, and be kind of slow in division.

And, about this time, why, I got drafted again. Remember I'd been drafted at the Lawson Lab - to the Watson Lab by the Manhattan Project. Well, while I was at the Watson Lab, toward the end of my stay in '49 and '50, I'd been helping people from General Electric in both the steam turban department, and the aircraft gas turban people, who worked closely together because of the similarity of their turbines, to make decisions on, for instance, whether or not to order a defense calculator. And G. E., in the person of Harry Winning, the then Vice President of Engineering, and IBM in the then person of Ned Douglas, one of their sales Vice Presidents, who had decided that G. E. should get machine number 6 of this string of fifteen or twenty 701's being built. I had suggested that they share this between the gas turbine division and the steam turban people, most of whom were in Schenectady, and that they locate it in Cincinnati, Ohio, partly because Cincinnati was to receive most of the new gas turban aircraft gas turban division; partly because I had an old astronomer friend, Paul Hergert who preceded Wallace Eckert at the Naval Observatory after Eckert came to IBM, and who was still very much a power in the use of these card program calculators sized computers. He never did get upstairs to the really big ones, but he finally got himself a 650, in the end. And he was the Head of the Department of Astronomy at the University of Cincinnati, would not leave the university once the war was over because he'd spent his whole life working upstairs in this particular faculty, and wasn't about to go anywhere else in spite of offers from IBM, the Naval Observatory, and many other places - Daldrin among other places. And the atlas group out at Converse, San Diego wanted him - everybody wanted him, but he was stuck in Cincinnati, so I said, "Why don't you put the machine in Cincinnati, then you can get Paul Hergert at least as a consultant, and maybe as a part-time manager of the thing, and get some hard rock types to do the dirty work, and you've got a good computing shop. Well, they took my advice on everything except Paul. When they talked to Paul they found that he wanted to be mostly an astronomer like Wallace Eckert, and was willing to

give one afternoon a week, sort of thing. So the recruiting group, which was half steam turban, and half gas turban came up to MIT, went behind my back to Forrester, and essentially had me detached from Forrester for G. E. Service. Well, you'd think that Forrester would have objected, and of course, MIT at that time had a very large and important gas turban laboratory under a Professor Taylor, which was funded almost entirely by G. E., which then had most of its gas turban work at Lenn(?), just up the road a piece - that was why they were going to move to Cincinnati. So, when they whistled MIT tended to at least respond, if not to jump, at least they tended to respond. And I've always suspected that, actually, Forrester and Everett didn't really think I was going to be all that good a logical design research man, and I think they were probably right - my heart lay with applications, let's face it - with numerical analysis, and software, and the organization of management of computer shops, rather than, you know, designing equipment. So, they were probably right.

So, what with one thing and another, the General Electric Company drafted me away from MIT, and I found that at that time they had a Model 1 card program calculator, one of these twenty or so that were built, working, not at Lenn Mass., but at Northrop Station in Boston. They had some office space for this group that was to move to Cincinnati. I closed that up, moved the small group that I found there to Cincinnati with me, and received, in Cincinnati, two Model 2 card programmers calculators, of which, of course, I was a great expert. Expanded the group, got ready to receive this defense calculator - in fact got the number six 701 about a year later.

MERTZ:

Did - you produce any technical memorandum when you were with the Whirlwind...

GROSCH:

Yes. There's a memorandum on this ternary calculator, and there's some very miscellaneous memoranda on sales...computers, how to make a completely - how to make a perfect computer out of imperfect components, and something, I think, on matrix arithmetic on IBM machines, or something like that. The ternary one was the only one that I remember with any affection. The only one, I think, that might survive in literature. It's referred to in a recent IEEE letters to the editor sort of thing by a couple of Argentines who got interested in sine ternary arithmetic while studying at MIT. And three or four years ago went back to Argentine and wrote the note which was refereed - was sent to me for refereeing, because my name was on the reference book. Startling me no end, I since I thought that had disappeared into limbo for many, many decades ago. That would have been - let's see, that would have been the spring of 1951. Actually, I had only worked for Forrester for six or seven months. I was drafted away...

MERTZ:

(inaudible)...

GROSCH:

I was drafted - that would be the spring of '52, right - I was drafted away in May of '52 and went immediately, almost, to Cincinnati.

MERTZ:

That would have been about six months?

GROSCH:

Yeah. Very much like the experience. I was drafted out of Frian(?) to work for Wallace Eckert after only six or seven months, also.

The G. E. experience was a very pleasant one. I should mention - we've just about got enough time for me to mention my relationship to this man, Craft. He was the General Electric expert in the shape of steam turban buckets. The cross sectional shape - the air foil shape of steam turban buckets. He'd been a pupil of Von Karmon's(?), and of course I had some other relations with Von Karmon through the Rocket Society since Karmon was the founder of the aero jet rocket activity out at Cal Tech, and Kraft had been a pupil of Von Karmon's in the early thirties in Germany - or late twenties in Germany, and emigrated to the United States in order to work for General Electric in the steam turban plant before the Hitler forces came to work on him. He was Jewish, but he left before that. And he attempted to do a very elaborate aero-dynamical partial differential equation kind of theory work on compressible flow of steam through these cascades of blades using not only Karmon as a consultant, but also Stephen Bergman, who was at Stanford in those days. And every resource of advanced applied mathematics that he could buy or invent for himself. He had a method involving transformation to the plane which worked fine for incompressible flow, but he wanted to generalize it to compressible flow. Well, he was going to have to calculate some big two-dimensional complex tables - complex in the sense of complex arithmetic as well as complex in the sense of difficult. One different one for each mark number, or some such relationship, to replace the single such tabulation that he made on punch card machines for incompressible flow. And it turned out that nature didn't want him to do this. I realize I sound foolish, it sounds like nature was like the statue of liberty, wearing you know, holding up the sword to stop him, but, in fact it was almost like that. We had a feeling that nature was his enemy on this. She really didn't want him to perform those calculations. No matter what mathematical method he used to approach this transformation - Bergman, for instance, I'd know used repeated complex integration, when we tried to tackle it on head on by straight forward methods, using mesh techniques, as I had done in the old shock wave work in the 1945-46 at the Watson Lab, it didn't matter. If he tried the direct approach it turned out that they lost so much significance that he had to do everything to like a hundred decimal places. If he tried to do it to this complex method that Bergman worked out, it turned out the process didn't convert fast enough so he had to use hundreds of thousands of currents,

and so forth, and so forth. And he started to do this, first on punch card machines, then with my help he put it on the SSEC, and finally, in the end, he went out and ran it to a hundred decimal places on the second big computer in the General Electric Company, which was the 702 that they had installed at Stanford. You remember the 702 was a character by character machine. And if you would wait long enough it would multiply two one hundred digit numbers together, although it took one hell of a long time to do it. And it wouldn't store very many at a time, of course, on its Williams tubes. So I had these continual intersections with Kraft as he tried to solve these problems. And he, in sort of an opposite number from the aircraft gas turban group were the ones that recommended to the top G. E. management that I be hired to install the 701.

I should say, in closing, that that 701 was the first large computer to work anywhere in the middle west. One off or otherwise. There just weren't any major machines anywhere in the middle west.

MERTZ:

When was it functioning?

GROSCH:

It came in the spring of '53. About a year after I arrived, and as I say, was the number six machine. It was also the first one to be used in the aircraft engine industry, and it was the first one in the General Electric Company. G. E. was building its own one off machine at that time, called the OMIBAC(?). I'll tell you more about that on another tape, and also was building one on contract for the Air Force called ORAC(?) which was a copy of Aiken's Mark IV - Mark III, I guess it was. But they didn't didn't have one working at that time, and the UNIVAC, which was to go into Louisville, and which is world famous at the present time, was delivered a year after mine, and didn't work until about two years after mine. So that was G. E.'s first venture into using the big machines, and long before they decided to build their own.

MERTZ:

How about ENIAC?

GROSCH:

No, ENIAC wasn't working yet either, at this time. It was - Meager had done an enormously effective job of redesigning the old EDVAC, OR EDSAC or whatever it was - EDVAC I guess it was. And it was to work very effectively and very impressively, but it didn't really work, as I remember, until about '52 - just about the time that I was going to Evandale(?), and long after G. E. had ordered the 701. Moreover, there was no prospect of getting another copy. That was a one off machine, also, you remember.

MERTZ:

And so you then went to - went to Cincinnati - you lived in Cincinnati for...

GROSCH:

For four years.

MERTZ:

for four years?

GROSCH:

Four years. And then I went to Phoenix for G. E.'s initial adventure into the computer field.

MERTZ:

I see, and...

GROSCH:

We'd better stop. It's twenty-five after.

MERTZ:

That's right. This is the end of this interview.

[End of Interview]